On the Unnikrishnan approach to the notion of locality

Bernard d’Espagnat

Abstract

Recent proposals by C.S. Unnikrishnan concerning locality and Bell’s theorem are critically analysed.

1 - Introduction

In what follows, two particulars play a major role. One of them (i) has to do with the quantum mechanical von Neumann formalism. It is that, according to the latter, not only wavefunctions get reduced upon measurement but even state reductions at space-like separated points seem to intervene in some cases; specially in those involving measurements on entangled systems. The other, quite different, one (ii) is that knowledge of elementary classical physics gets students (including future physicists) used to the idea that all of the basic entities science considers should be ontologically real. That fields, in particular, do exist by themselves, with the properties we ascribe to them, quite independently of us; or, at least, that assuming they enjoy such an existence and such properties is often useful and always harmless. This has led many beginners in quantum physics to take it for granted that, being quite basic, the wavefunctions necessarily have such an ontological reality.

The conjunction of facts (i) and (ii) results in that occurrence, in the formalism, of state reductions at a distance is naturally viewed by students in the field as the clearest evidence we may have in favor of nonlocality. And this, in turn, may prompt a theorist inclined to question the pertinence of this notion to consider that the task of restoring locality should primarily be a matter of disproving state reductions at a distance. It may well be that this kind of approach to the subject was that of Unnikrishnan. Indeed, this author recently published a series of papers [1],[2],[3],[4] concerning nonlocality and Bell’s theorem and, for instance, in the abstract of [4], with regard to state reduction at space-like separated points, he wrote: “I show conclusively that there is no such spooky state reduction, vindicating the strong views against nonlocality held by Einstein and Popper”. And it is only after having made this statement concerning wavefunction reduction that he added: “The Bell’s inequalities arise due to ignoring the phase information in the correlation function and not due to nonlocality”. This, together with several other statements of the same vein in his papers, is an indication that indeed
his reflection proceeded \textit{from} a critical analysis of the wavefunction reduction notion \textit{to} a criticism of the Bell theorem, rather than the opposite way.

Admittedly there are reasons that, a priori, make such a march of thought quite normal and natural. It is, in particular, strictly true that, historically, the presence, in von Neumann’s formalism, of such “spooky state reductions” was the first hint pointing to the idea that, within physics, the notion of locality might conceivably be questioned. So that it is fully understandable that Unnikrishnan should have had his attention first drawn on them. This - let it be stressed here - had a positive overall effect since the question he thus focused on, that of knowing whether the notion of wavefunction collapse at a distance is contradiction-free or not, even though it is an old one, still seems, at present, not completely elucidated. His pointing out of this has rightly attracted attention.

Because of point (ii) above, it is also understandable that, having finally reached the conclusion that such wavefunction reductions at a distance do not occur, Unnikrishnan interpreted it as vindicating Einstein locality. And that this prompted him to look for loopholes in the Bell proof of nonlocality. It is, to repeat, understandable and natural. And it is therefore most unfortunate that it happened such a rational move led him astray. The reason why it did is that, as defined by Einstein (and Bell) the notion of locality cannot be dissociated from that of elements of reality, taken in the sense of “elements that really exist, quite independently of our knowledge”. So that, with this Einsteinian definition of the words, the notion of wavefunction reduction at a distance can possibly have an impact on the locality problem only if the wavefunction is considered as being a real entity. For, obviously, under the opposite assumption - the view that the wavefunction merely describes our knowledge - the idea that, upon some measurements, it gets reduced at a distance, even if true, merely concerns our knowledge. It has no bearing whatsoever on the existence or nonexistence of a real, physical event at the place where the reduction is considered as occurring. Now there are sensible arguments in favor of such an “operationalist” conception. One of them is the fact that the idea the wave function of a physical system is “real stuff” is in no way an integral part of the quantum mechanical formalism. At any rate, it lies much beyond the “hard core” of what the latter consists of, which essentially is a set of general (and never falsified) general rules yielding the probabilities of such and such observations. Another, even more weighty, argument is that, conceptually, the idea that the wave function is real stuff is -
obviously - quite difficult to reconcile with the general notion of measurement
collapse. Indeed, it seems that the latter argument dissuades most physicists
- including those with a “realistic” turn of mind - from explicitly referring,
in their reasoning, to the reality of the wave function. This is why, a fortiori,
on the tricky question whether or not reduction at a distance is real or not,
most of them take up an attitude of reserve.

Things being so, it must be granted that what, concerning the wavefunc-
tion, we know for sure is merely that it is an efficient tool for predicting what
will be observed. It follows from this that - contrary to what Unnikrishnan’s
thinks is true - we shall be justified in dissociating the conceptual problems
concerning reality from - as it turns out - more “technical” ones, concerning
quantum mechanics. To claim - as Unnikrishnan did - that specific notions
(such as phases and so on) associated with wavefunctions must necessarily be
introduced in analyses of questions concerning elements of reality and Ein-
stein locality seems quite natural at first sight but is in fact preposterous. It
may quite well happen - and historically it did often happen - that concepts
and representations that are most useful for some purposes get nevertheless
superseded, when more general purposes are at stake, by entirely different
ones. When considering one particular problem it is therefore advisable to
- whenever possible - carry on the relevant investigations without assuming
at the start the absolute validity of some currently used notions. And this,
of course, is especially true in cases in which, as here, some obstacles exist,
making it objectionable to raise the notions in question to the level of true
descriptions of what “really is”.

Here we choose therefore to exclusively discuss Unnikrishnan’s analysis of
locality and the Bell theorem. This implies that the criticisms we shall state
exclusively bear on his treatment of this subject. To repeat, his questioning
concerning collapses at a distance is interesting in itself, independently of
any specific interpretation (realistic or otherwise) of the quantum mechanical
rules, Simply: it will not be investigated in what follows.

The criticisms in question are explained in Section 2, which constitutes
the essential part of the present paper. Sections 3 to 6 are to be viewed as
answering possible objections and bringing in additional information.

2 - Locality and Bell’s theorem.

Bell of course acknowledged the tremendous power of quantum mechan-
ics at predicting future observations on the basis of past knowledge. But he
did not rest content with just this. He was a realist in the sense that he
considered physicists should strive at knowing reality as it really is, quite in-
dependently of us. And he was clear-sighted enough to realize that quantum
mechanics meets with hosts of difficulties when it is interpreted as yielding
a knowledge of such a kind. Concerning it he therefore held the somewhat
“operationalist” view sketched above. He regarded it as being essentially -
at least in its present stage - just a set of efficient previsional recipes, with
the consequence that he did not attach any binding ontological significance
to the quantum mechanical concepts. What he therefore tried to do - and
succeeded in doing - was to derive, concerning reality per se, some (possibly
negative) knowledge independent of the said concepts and directly grounded
on experiment. Let it be stressed once more that Unnikrishnan’s standpoint
is quite at variance with this. For example, in section 1.4 of [2] - entitled:
“Where is the phase” - he insisted that “every reasonable description of mi-
croscopic phenomena should have theoretical constructs at the single particle
level that can represent the wave nature, and specially the phase”. Now, as
pointed out above, strictly speaking, this statement of his is not true. It is
too conservative. The “should” that appears in it indicates a conception of
quantum mechanics that unduly raises to the level of elements that must be
taken into account notions (here the phases) that may well just be parts of a
successful predictive recipe. As if, in Astronomy, noting that, notwithstand-
ing its descriptive deficiency, the geocentric theory can serve as a recipe for
predicting observations - eclipses, say -, we claimed: “therefore, any reason-
able description of the Solar System should have theoretical constructs of the
nature of epicycles”. This constitutes my first criticism to his approach.

My second criticism reaches even more to the crux of the matter. It is that
Unnikrishnan completely missed the nature and bearing of Bell’s theorem. To
repeat: Bell did not consider the “quantum states” as being objective, that
is as being the states in which elements of “reality per se” really are. But of
course, being a realist, he nevertheless had to consider “objective states”, that
is, states in which physical systems can be. Concerning the nature of these
objective states he kept extremely general. In fact, he merely postulated
that they are specified by a (discrete or continuous) set of parameters h
(commonly called “hidden”) having the nature of real numbers. He then
- still quite generally - observed that even spacelike-separated events may
exhibit correlations since they can be influenced by past common causes,
but that it is natural to believe that, when they do, it is exclusively for this
reason. And that, consequently, within subensembles in which the common causes are fixed, no correlation should take place. This natural idea is what he called “local causality” or, sometimes, just “locality”. An often overlooked but nevertheless crucial point is that it is merely from these general views that he could derive, for the correlation function P(a,b), an expression leading to predictions that are violated by the experimental data, thus disproving local causality. Therefore Unnikrishnan’s statement (in Section 1.4 of [2]) that “there is no quantity in P(a,b) that reflects the phase information associated with the individual particle” is just simply false. According to Bell, the parameters h are, by definition, all the parameters (known or unknown, knowable or unknowable) that define the objective state of the pair. And he could prove his theorem without assuming anything concerning the nature of these parameters (except that they are real numbers). Consequently, if it is considered that there are “phases” among them (as would have to be assumed by a physicist inclined to interpret quantum mechanics in an ontological way), these phases are ipso facto included within the set h. As we see, one of the main interests of Bell’s proof lies in its generality. It holds good whatever the structure is of the physical theory that we care to believe in, and supposes a meaning of the word “locality” extending but minimally beyond the one in which it is commonly used.

To this quite basic criticism let a few remarks be added, that illustrate the extent to which Unnikrishnan misunderstood Bell’s position. First, let it be noted that, while Equation (5) of [2] reproduces with but minor changes Bell’s correlation function P(a, b), the there accompanying specifications of the nature of the h’s and of A and B are faulty. h (the distinction between h_1 and h_2 is inappropriate) indeed collectively designates a set of hidden variables but, contrary to Unnikrishnan’s assertion, these variables are not associated with the outcomes. They characterize (completely) the objective state of the particle pair before the measurements take place (otherwise said, the “common causes” at the source). And as for P(a, b) itself, it is an average, but not on “the product of eigenvalues” as stated by Unnikrishnan. Indeed, since, as we saw, quantum mechanics is not involved in this derivation, the notion of eigenvalues is here radically irrelevant. And, for the same reason, Unnikrishnan’s statement in Section 2.1 that “the defect in the local realistic theories is that the correlation is calculated essentially by averaging over the product of eigenvalues” is meaningless. In the version of Bell’s theorem for which formula (5) of [2] holds true, determinism is assumed and A(a,h)
and $B(b, h)$ are, respectively, the outcomes (+ or -) that are bound to emerge as results of measurements performed with instrument settings $a$ and $b$, when the parameters of the pair are $h$. In view of all this it is quite clear that Unnikrishnan’s statement: “The assumptions that went into the formulation of Bell’s theorem have not encompassed the essential physical properties” ([2], Section 1.4) is plainly erroneous. It just stems from the fact that Unnikrishnan did not understand what Bell did.

3 - Unnikrishnan’s model is not a counter-example

In the past, many attempts were made at building up counter-examples to Bell’s theorem, with the aim of proving that the latter relies on some additional implicit assumptions. All of them failed. But a priori it might be conjectured that the model Unnikrishnan describes in [2], Section 2, is a new, and (this time) successful, counter-example. Indeed, this model does reproduce the quantum mechanical predictions and Unnikrishnan puts forward arguments to the effect of showing that it is local. So, does its existence show Bell’s theorem is flawed? In fact, it is easy to see that the answer is “no”, for the author’s considerations are based on a definition of locality that is totally different from Bell’s one.

4 - The definitions of locality: two reasonable candidates

The foregoing remark should induce us to have a look at various possible definitions of locality and try to appreciate their relative pertinacy. Clearly, since we want to keep general we must not postulate determinism. In considering indeterminist theories we cannot, of course, assume that the outcomes $A$ and $B$ of the measurements are functions of $a$ and $h$ and $b$ and $h$ respectively (in the above used notations). Hence, we must generalize the “local causality” notion. This Bell and others did as follows. Denoting quite generally by the symbol $(X|Y)$ the conditional probability that “$X$ if $Y$”, they identified “local causality” (alias, simply “locality”) with the assumption —call it ASN— that the conditional probability $(A|a, h)$ that “the outcome on the first instrument is $A$ when the setting of the instrument is $a$ and the hidden parameters are $h$” is independent of both $b$ and $B$; and symmetrically, of course, (interchanging $A$ and $B$ as well as $a$ and $b$) concerning the second instrument. For the above stated reason, such an assumption is a priori quite a natural one, adequately reflecting what we intuitively have in mind when we assert that, within two, distant, given ensembles of random
events, the events in any one of the ensembles exert no influence on those of
the other ensemble. From the general rules of probability calculus the joint
conditional probability that “the outcomes on the first and second instru-
ments are A and B respectively, given that the settings are a and b and the
hidden parameters of the pair are h” is \((A|\mathbf{a},\mathbf{b},\mathbf{h})(B|\mathbf{a},\mathbf{b},\mathbf{h})\) and because
of assumption ASN this reduces to just the product \((A|\mathbf{a},\mathbf{h})(B|\mathbf{b},\mathbf{h})\), so that
the correlation function \(P(\mathbf{a},\mathbf{b})\) is of the form:

\[
P(\mathbf{a}, \mathbf{b}) = \int d\mathbf{h} \rho(\mathbf{h})(A|\mathbf{a}, \mathbf{h})(B|\mathbf{b}, \mathbf{h}) \quad (I)
\]

where \(\rho(\mathbf{h})\) is the statistical distribution of the hidden parameters. And the
Bell theorem in its “final” form consists in the proof that both the quantum
mechanical predictions and the experimental data violate this relation (I),
that is, violate the, thus generalized, “locality”.

We have just seen Bell’s definition of this notion. But we must remember
that the way we associate words with concepts is, to some extent, a mat-
ter of free choice, and this is particularly true when, as here, the word in
question is an everyday one and we need use it outside the commonsense
domain. It is therefore not surprising that, in the relevant literature, at least
two reasonable definitions of the word “locality” were put forward, with the
(admittedly unpleasant!) consequence that the same set of data is compat-
ible with locality when the word is defined one way and is not when the
word is defined in the other way. More precisely, when defining “locality”
above, we specified, along Bell’s lines, that locality means the conditional
probability of event A for fixed \(\mathbf{a}\) and \(\mathbf{h}\)’s is independent of both \(\mathbf{b}\) and B
(and conversely, exchanging \(\mathbf{A}\) and \(\mathbf{B}\) as well as \(\mathbf{a}\) and \(\mathbf{b}\)). Some physicists
opted for a different choice. They decided to consider that locality holds
true as soon as the conditional probability in question is independent of \(\mathbf{b}\),
no condition being specified involving B. Such a weak definition of “locality”
is also called “parameter independence”. It suffices to guarantee that no
tural, no theorem similar to Bell’s one can be proven, so that models can easily
be found that reproduce the quantum mechanical predictions while being
“local” in this weak sense. In a way, it is a pity that the same word should
thus be used to define two different concepts, having very different conse-
dquences. And, incidentally, it may be pointed out that the idea the mere
outcome of a measurement should directly influence the outcome of another
one, spatially separated from the former, appreciably violates our intuitive view on locality. So that, when all is said and done, Bell’s definition appears as being distinctly more adequate than the “weak” one. At any rate, it is more in continuity with our commonsense understanding of the word. But it is true nevertheless that both do capture the same, reasonable, basic idea. To repeat, this idea intuitively is that, when two sequences $S_A$ and $S_B$ of events take place in two spatially separated space-time regions $R_A$ and $R_B$, the only instances in which $S_A$ and $S_B$ may happen to be correlated is when, within each sequence, the changes from one event to the next one are influenced by changes having taken place within some sequence $S_C$ of events pertaining to the common past of $R_A$ and $R_B$. So that, if subsequences $S'_C$ are considered in which the latter changes do not occur, the corresponding subsequences $S'_A$ and $S'_B$ should not be correlated. For further reference, let this condition be called “Condition C”.

For the purpose of avoiding an unreasonable dilution of the meaning of the word “locality” an appropriate convention is to restrict its use to the description of situations in which condition C is fulfilled. As we shall see, however, this condition is not fulfilled by the definition of locality adopted by Unnikrishnan.

*Remark*

Unnikrishnan is roughly aware of what constitutes the main idea in condition C: the idea that an appropriately defined conditional joint probability of events A and B is to be written as a product. He calls it the “separability of probability” idea. Unfortunately, he does not seem to have grasped the crucial point concerning condition C, which is, as just explained, that it is only the probability concerning the subsequences $S'_A$ and $S'_B$ - otherwise said, the conditional probability of events corresponding to fixed $h$ values - that is expected to factorize. In Section 1.5 of [2] he describes an experiment in classical optics - the Hanbury Brown-Twiss experiment - where all physical aspects obey locality, and even the Bell inequality, but in which, he writes, the separability of probability is invalid. He considers this as showing that separation of probability is not a good criterion for locality, and, without explicitly saying so, he sort of suggests that this might be another flaw in Bell’s proof. But in fact the argument misses its point. The Brown-Twiss experiment can be fully described within classical physics, that is, as a purely deterministic phenomenon in which every single detection event (call it A
or B according to whether it takes place in one or in the other detector) is
entirely determined by (a huge number of) elementary parameters that may
be collectively designated by the symbol \( h \). Now, Unnikrishnan points out
that the joint probability is not a product of the local probabilities at the
two detectors and that there is a correlation of the intensities. But this is
just because the joint probability he considers is the one that is experiment-
tally measurable: which means that it is grounded on the consideration of
a large sequence of elementary events, not all identical with one another,
taking place at the source. In other words, the observed correlation merely
reflects the fact that the sequences of events in the two detectors are due to
common causes. Clearly, this joint probability has nothing to do with the one
- call it, say, \( p' \) - that, in the proof of Bell’s theorem, is considered as being a
product. And indeed there is no reason whatsoever to assume that \( p' \) (which
of course is not accessible to experiment) is not a product. Otherwise said,
there is no reason to assume that the observed correlations are not due to
common causes at the source, distributed according to the density \( \rho(h) \). In
fact, here as in all deterministic theories, \( p' \) (roughly speaking and to within
normalization) should be a product of two delta functions so that A and
B are in fact two functions of \( h \). The experimentally accessible correlation
function can then be considered as having the form of Eq.(5) of [2], which,
of course implies the validity of the Bell inequality.

5 - A criticism of “Unnikrishnan locality”

In Unnikrishnan’s views a central role is played by the amplitudes \( C_1 \)
and \( C_2 \) he introduces and by an associated “amplitude correlation function”
\( U(a,b) = \text{Real}(NC_1^*C_2) \) where \( N \) is a normalization constant. And he claims
that, in the model he puts forward, “the locality assumption is strictly en-
forced since the two amplitudes \( C_1 \) and \( C_2 \) depend only on the local variables
and on an internal variable [the difference between \( \phi_1 \) and \( \phi_2 \) generated at
the source and then individually carried by the particles"). This may be con-
sidered as his (indirect) precise definition of what he means by “locality”. It
is easy to see that, as could be expected, it does not fulfill the above stated
Condition C (which is essentially, as may be remembered, Bell’s condition
for locality). In the model in question consider, for example, the conditional
probability \( P(+,+|\phi_1,\phi_2) \) that the outcomes of the measurements A and B
should both be +1, under the condition that the phases \( \phi_1 \) and \( \phi_2 \) have
fixed, given values. Since, in this model, \( \phi_1 \) and \( \phi_2 \) are the only parameters
attached to the particles, they alone can be the carriers of the influences exerted by the source on the measuring instruments: so that if, within the sequence of emission events at the source, we consider a subsequence of such events within which the values of $\phi_1$ and $\phi_2$ do not vary from one event to the next one, according to condition C, the corresponding subsequences of A and B measurements outcomes should be uncorrelated. Which means that $P(+,+|\phi_1,\phi_2)$ should be a product of two factors, one depending on $\phi_1$ and not on $\phi_2$ and the other one depending on $\phi_2$ and not on $\phi_1$. This, however, is not the case since, in the said model - according to assumption (3) of [2], Section 2.1 - $P(+,+|\phi_1,\phi_2)$ is proportional (to within a numerical normalization factor) to the square of $\text{Re } C_1^*+C_2^*$, that is to:

$$\cos^2[s(q_1-q_2)+s(\phi_1-\phi_2)]. \quad (II)$$

which is not such a product.

Now, admittedly anybody is free to use the words of the language the way he chooses provided he specifies the meaning he imparts to them. But, again, unrestricted use of such a freedom would obviously spread confusion. To avoid it, the best is, when we have to do with different concepts, to designate them by different names. In that spirit, the concept defined by Unnikrishnan, which is quite different from both Bell’s and “weak” locality, could be called, say, “Unnikrishnan locality”. It is a formal concept whose domain of meaningfulness seems to be restricted to the theories imparting to waves some type of ontological significance. With the help of the name we just gave to it we can state precisely what Unnikrishnan achieved and did not achieve. Actually what he has shown is that there is no proof that Unnikrishnan locality is violated in Nature. He did not show anything more. As we saw, he radically misunderstood the nature, significance and generality of Bell’s theorem, and, quite definitely, he proved neither that it is false nor that it is irrelevant.

6 - Conclusion

Unnikrishnan’s meritorious investigations are interesting, not only in themselves but also in that they yield quite a vivid example of the multifarious difficulties and pitfalls that threaten the scientists within the field of research he elected. What comes out from the above is that:

1 - Unnikrishnan’s interest for the detailed structure of quantum mechanics, and, in particular, state reduction at a distance may well be justified.
Here, this problem was not investigated and consequently the possibility was not ruled out that, as Unnikrishnan suggests, self-consistency problems should still remain in this domain.

2 - Although several definitions of locality were put forward in the literature, they are not all equally satisfactory. They are all the more so as they are less arbitrary, that is, as they are closer to the everyday meaning of the word. In this respect, the Bell-like definition, also called by him “local causality”, is, by far, the best one. In contrast, Unnikrishnan’s definition is an abstract one and depends too much on the idea that wavefunctions have ontological reality. This makes it artificial.

3 - Last but not least: Unnikrishnan completely missed the substance and bearings of Bell’s theorem, his assertions concerning its premises being erroneous. In fact, the class of theories that this theorem rules out is much wider than he considers it to be. Contrary to what he seems to suggest, it includes any attempt at a realist local interpretation of standard quantum mechanics.

References

1 C.S.Unnikrishnan, Resolution of the nonlocality puzzle in the EPR paradox, Current Science 79, 195 (200).

2 C.S.Unnikrishnan, Is the quantum mechanical description of physical reality complete? Proposed resolution of the EPR puzzle, Foundations of Physics Letters, 15, N° 1, February 2002.

3 C.S.Unnikrishnan, Proof of Absence of Non-local State-reduction in Quantum Mechanics, preprint.

4 C.S.Unnikrishnan, Einstein was right: Proof of absence of spooky state reduction in quantum mechanics, http://arXiv, quant-ph/0206175.