Reply to K A Kirkpatrick

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Abstract

This is a reply to an article with the same title in which Kirkpatrick claimed that the considerations I put forward some thirty years ago on quantum mixtures are incorrect. It is shown here that Kirkpatrick’s reasoning is erroneous.

1. Introduction and a preliminary remark.

In a recent paper [1] K. A. Kirkpatrick criticised the distinction I introduced long ago [2] (see also [3] and [4]) between the notions of proper and improper mixtures. In the present article I shall explain why, in my view, his criticism is unfounded. The subject must first be introduced and, quite appropriately, Kirkpatrick did this by referring to von Neumann’s book [5]. “Von Neumann – he wrote – introduced mixtures of pure ensembles into quantum mechanics exactly in the manner of classical probability, as a matter of ignorance. Introducing the statistical operator (density matrix) as the descriptor of a mixture he said: “if we do not even know what state is actually present – for example, when several states $\phi_1$, $\phi_2$, ... with respective probabilities $w_1$, $w_2$, ... constitute the description – then the statistical operator is $\rho = \sum_s w_s |\phi_s><\phi_s|$. Besides, Kirkpatrick also made a recall: “... von Neumann proved – he wrote – that the unique statistical descriptor of a subsystem $S$ of a joint system $S + M$ is given by the partial trace $\rho^S = Tr_M(\rho^{S+M})$. [...] This statistical operator can always be expressed (in many ways) as a convex sum of pure-state projectors, exactly in the form of the ‘ignorance’ mixture first introduced”. In his introduction Kirkpatrick then further noted that I chose to call proper mixtures the mixtures defined by von Neumann in the first stated manner, improper mixtures those made up of the subsystems $S$ of a composite system $S + M$, and that I somehow questioned the possibility of identifying the two notions (this is why I introduced these epithets).
Needless to say: on all this I fully agree. Where I first start disagreeing to some extent with Kirkpatrick is on his precise characterizing of my questioning. He wrote: “D’Espagnat claims that an ignorance interpretation of the improper mixture is mathematically inconsistent”. This formulation does not fully satisfy me in that it conveys the - wrong - idea that the reason why I claim the improper mixture cannot be given an ignorance interpretation is essentially of a mathematical nature. This, actually, is not the case. Basically, the reason in question is, as we shall see, not mathematical but logico-semantic. It consists of the fact that, in science, all the words we use must be have a meaning, so that, when we say that a given mathematical description is consistent with such and such an interpretation we must be able to explain the meaning of the words by means of which the said interpretation is stated. My claim essentially is that, when this is done concerning the word “ignorance”, then, the “improper mixtures” represented by the partial traces of a composite system pure state statistical operator cannot be given an ignorance interpretation. It is therefore this claim that my critics should aim at disproving. We shall see that Kirkpatrick’s paper does not succeed in doing so.

In the next section we shall go, properly speaking, into this matter. Before that, however, it is appropriate that a preliminary remark should be made, concerning a notion Kirkpatrick makes use of.

*Preliminary remark.*

It has to do with the “rule of distinguishability”. By this name Kirkpatrick referred to the well-known rule of quantum computation: “when the alternative processes are indistinguishable, square the sum of their amplitudes, when distinguishable, sum the squares of their amplitudes”, a rule Feynman frequently used in his books. It is true of course that, as a guide and as a “short cut” sparing tedious computations, the rule in question is a most useful one. It should however be well noted that it does not rank among the basic quantum mechanical rules or “axioms” (such as the correspondence between observables and self-adjoint operators, the quantum law of evolution or the generalized Born rule concerning probabilities of observations). Actually, it is merely a consequence of the latter. It simply follows from the fact that, whenever a system $S$ interacts with a system $M$ that may react to this impact in a nonnegligible way, in order to study what happens to $S$ it
is necessary to consider the wave function of the composite system \( S + M \). If we are interested in \( S \) alone we must of course sum over all the conceivable measurement results concerning \( M \) (hence, technically, “trace \( M \) out”), and this normally leads to the disappearance of cross-terms involving amplitudes concerning \( S \). The distinguishability rule follows. This shows, first that within the realm of questions concerning which the rule of distinguishability is applicable its use is in principle redundant (in last resort, what really counts is the detailed experimental arrangement and it is on its basis that one must argue in case of doubt) and second, more importantly, that its range of applicability is well defined. In fact, it is limited to the type of computational problems for the investigation of which it was conceived and proved. This means, the rule is fully reliable when we want to derive, from our knowledge of how a system was prepared, the probabilities we have of getting such and such measurement outcomes. But we would have no right to extend it to the conceptual analysis of basic questions falling, partly or totally, outside this range. Trying to apply the said rule to such questions may amount to depriving oneself of any possibility of defining notions that are needed for their very formulation. And indeed, in the next section it will become apparent that this is precisely the kind of error Kirkpatrick fell into.

2. On Hugues’ argument.

We can now turn to our subject proper. Kirkpatrick’s discussion of my standpoint starts (his Section 3) as follows: “D’Espagnat insists that the improper mixture, although represented by the same statistical operator as the proper mixture, does not represent a mixture of ensembles in pure states \( \{ |\phi_s > \} \); the ignorance interpretation may not be applied to it”. He then tries to show that this view is flawed.

To this end, he first considers the argument - very similar to my own - by means of which R.I.G.Hughes [6] justified this distinction; so, let us begin by analysing what he objects to Hugues. According to Kirkpatrick, Hugues’ argument runs as follows. “Consider a composite system \( S + M \) in the pure state \( \rho \), of which the component states are the mixed states \( \rho_S \) and \( \rho_M \). For the sake of the argument assume that \( \rho_S = a_1 |u_1 > < u_1| + a_2 |u_2 > < u_2| \), while \( \rho_M = b_1 |v_1 > < v_1| + b_2 |v_2 > < v_2| \), with
$a_1 \neq a_2$ and $b_1 \neq b_2$ so there are no problems of degeneracy. Then, according to the ignorance interpretation of $\rho^S$ and $\rho^M$, system $S$ is really in one of the pure states $|u_1 >$ or $|u_2 >$ and system $M$ is really in one of the pure states $|v_1 >$ or $|v_2 >$. But this would mean that the composite system is really in one of the four states $|u_j > |v_k >$, with probabilities $a_i b_k$ respectively - in other words, that the composite system is in a mixed state. Since this contradicts our original assumption, the ignorance interpretation simply will not do”.

After having thus, for the benefit of the discussion, reproduced Hugues’ reasoning, Kirkpatrick claimed that it is wrong. “This argument is so clearly stated - he wrote - that its error stands out”. And he explained: “the claim that “the composite system is in a mixed state” is not supportable - nothing external to $S + M$ distinguishes those states $|u_i > |v_k >$ from one another. We must add the state vectors (not the projectors) $|\Psi > = \Sigma_{j,k} \psi_{jk} |u_j > |v_k >$ - a pure state”.

Sweeping as these statements are, I claim they are in fact unjustified and incorrect. Their first defect is that Kirkpatrick’s reference to non-distinguishability is out of place. The problem we are here faced with is not one of calculating the probabilities we have of observing this or that on a system $S$, given the way $S$ was prepared. It is a general problem of interpreting the formalism. The arguments developed in Section 1 above (Preliminary Remark) show that the idea or trying to apply the rule of distinguishability to such questions is unjustified.

The second defect in Kirkpatrick’s rebuttal of Hugues’ argument can be described as follows. Note first that, in it, Kirkpatrick tacitly endorses Hugues’ assertion that the considered composite system is really in $|but$ one of the four states $|u_j > |v_k >$. Indeed, in view of the starting assumption he accepted - that $S$ is really in one of the pure states $|u_1 >$ or $|u_2 >$ and $M$ is really in one of the pure states $|v_1 >$ or $|v_2 >$ - he could not reject the said assertion (which, incidentally, means that in an ensemble of such composite systems some of them are in state $|u_1 > |v_1 >$, some others in state $|u_1 > |v_2 >$ etc.). But on the other hand - and this is precisely the point that makes his reasoning inconsistent - while he seems oblivious of what this assertion usually means (in terms of differences in possessed values), he offers no alternative definition of its meaning. Indeed, nowhere does he state what the words “is really” actually mean to him. However, as already stressed, in any statement aiming at objectivity all the words used should have a meaning, so that, when we
claim that a given interpretation is consistent we must be able to explain what the words expressing it actually signify.

Now, in science a statement is meaningful only if it corresponds, directly or indirectly, to some conceivable piece of experience. Consequently, when Kirkpatrick, following Hugues, states that the composite system is really in one of the states $|u_j> |v_k>$ he should be able to explain what he means by referring to some possible experience. Not necessarily to an experience we actually have, but at least to one that, conceivably, some people could have. This, however, he did not do. And if we ourselves try to fill up this gap, we find that there is but one possibility. It consists in identifying the expression “System S is really in state $|u>$” to the conditional statement: “if, on S, somebody measured an observable $G$ having $|u>$ among its eigenvectors he/she would, with certainty (probability 1), get as an outcome the precise eigenvalue of which $|u>$ is an eigenvector.

To see what this implies let us focus on the simplest possible case (implicitly but appropriately used by both Hugues and Kirkpatrick for introducing the problem), namely the one in which both the Hilbert spaces $H^S$ and $H^M$ of systems $S$ and $M$ are two-dimensional. Let then $G$ be the observable of $S$ (a spin component for example) that has $\{|u_1>, |u_2>\}$ as eigenvectors and let $g_1$ and $g_2$ be the corresponding eigenvalues. Similarly, let $R$ be the observable of $M$ that has $\{|v_1>, |v_2>\}$ as eigenvectors and let $r_1$ and $r_2$ be the corresponding eigenvalues. Hugues and Kirkpatrick both assert that system $S$ is really in one of the two states $|u_j>$ and that the composite system $S+M$ is really in one of the four $|u_j> |v_k>$ states of $S+M$. So, according to them, if we consider an ensemble $\hat{E}$ of such $S+M$ systems the latter must be distributed in four subensembles labelled $i,k$ ($i,k = 1$ or 2). Let us then consider the subensemble $\hat{E}_{i,n}$ of $\hat{E}$ labelled $j = i$ and $k = n$. In it, all the systems $S$ are really in state $|u_i>$. From the foregoing definition, we therefore know that if $G$ were measured on an element of this subensemble there is a probability 1 that the outcome $g_i$ would be obtained. Then, however, we may resort to a well known (and easily proved!) lemma that, partly using the above defined notations, can be stated as follows. Let $Q$ be an ensemble of composite systems $S+M$, let, again, $G$ be an observable nondegenerate in $H^S$, having $\{|u_1>, |u_2>\}$ as eigenvectors and $g_1, g_2$ as the corresponding eigenvalues and let the probability be 1 that, within $Q$, the outcome of the measurement of $G$ be $g_i$. Then, the statistical operator (density ma-
matrix) describing $Q$ factorizes, with $|u_i><u_i|$ as a factor. Here this lemma applies, so that we know that the statistical operator $\rho^{i,n}$ describing $\hat{E}^{i,n}$ has $|u_i><u_i|$ as a factor. For the same reason we know that it has $|v_n><v_n|$ as a factor. It therefore is:

$$\rho^{i,n} = |u_i v_n><u_i v_n|.$$  

Then, however, nobody can deny that, $a_j$ and $b_k$ being the proportions defined by Hugues in his example, $\hat{E}$ is describable by the density matrix

$$\rho' = \sum_{j,k} a_j b_k \rho_{j,k}$$

for indeed, from the very way in which this $\hat{E}$ has been constructed it follows that the probabilities concerning the outcomes of any measurements whatsoever that we could choose to perform on its elements are obtained by first evaluating the corresponding probabilities $p_{j,k}$ on each $\hat{E}_{j,k}$ and then combining them according to the usual laws of combined probabilities, in the form

$$\sum_{j,k} a_j b_k p_{j,k};$$

and it has been common knowledge ever since the appearance of von Neumann’s book that these probabilities are exactly those yielded by $\rho'$. 

But then, whether Kirkpatrick likes it or not, $\hat{E}$ is quite obviously not describable as a pure case $|\Psi >= \sum_{jk} \psi_{jk} |u_i v_k >$ since $\rho'$ is not a projector ($\rho'^2 \neq \rho'$), a fact implying in particular that, in whatever way we decide to choose the coefficients $\psi_{jk}$, there are observables concerning which $\rho'$ yields verifiable predictions differing from those yielded by the pure case $\rho = |\Psi><\Psi|$. 

It follows from this that - again, however we choose the $\psi_{jk}$ – the ensemble of the $S$ systems whose density matrix is obtained by partial tracing of this $\rho$ over the Hilbert space of $M$ and the ensemble of the $M$ systems obtained by the symmetrical procedure (exchanging symbols $S$ and $M$) cannot be mixtures defined, a la von Neumann, as a matter of ignorance - that is, by combining subensembles endowed with different characteristics (“proper mixtures” in my terminology) - since, as we just showed, if they were, the ensemble of the composite $S+M$ systems would be describable by $\rho'$, which it is not. To sum up, we here have been careful to give a
meaning to the expression “is really”, used by Hugues and endorsed by Kirkpatrick, and consequently also to the expression “ignorance interpretation”, similarly used by these authors, and this has led us to agree with Hugues - and disagree with Kirkpatrick - in asserting that the ignorance interpretation cannot be applied to the ensembles yielded by the just described partial tracing operation.

Remark

The foregoing argument may appear somewhat roundabout since we might consider that, as soon as Kirkpatrick granted that the composite system “is really” in one of the four states \(|u_j v_k\rangle\), he was thereby forced to admit that it is describable by (2) and cannot, therefore, be in a pure state. I myself tend to be convinced by this simplified argument but the very existence of Kirkpatrick’s paper shows that this standpoint is not shared by everybody. The reason may be that, so long as one just ponders on formulas without making precise what words mean – by referring to experience –, some vagueness remains that leaves a place for disputable views. So, after all, there is a reason for considering that the argument above is not totally redundant.

3. The peculiarity of Quantum Mechanics.

The interpretation of Quantum Mechanics always raised conceptual problems. It is natural that questions concerning the basic nature of quantum mixtures should not be totally independent from these problems and it is therefore appropriate that we should here have a look at the latter.

For that purpose, let us make a detour to classical physics. It may be considered that - perhaps setting apart classical statistical mechanics which is a debatable case - classical physics, considered as a universal theory, was ontologically interpretable. This does not mean that such an interpretation was logically necessary. It was not. But it does mean that it was admissible, in the sense that it did not generate contradictions. All the fields and particles that appeared in the classical formulas could without difficulty be viewed as being really existing entities so that, when their values or, respectively, positions were measured, the outcomes of the measurements could, without qualms, be interpreted as revealing the values these
quantities actually had. I use to express this fact by saying that the corresponding statements were “strongly objective” ones. As is well known, the same does not hold true in quantum mechanics where, for example, interpreting the wave function as being a real entity in the above sense leads to a host of conceptual difficulties (nature of collapse and so on). To be sure, quite a number of physicists still go on thinking that all this questioning is just an old story. That such interpretational problems were satisfactorily solved by Bohr a long time ago. In a sense they are right. Bohr could write with confidence: “The description of atomic phenomena has [] a perfectly objective character” [7]. But let us have a look at the remaining part of this sentence of him. It reads: “... in the sense that no explicit reference is made to any individual observer and that therefore [...] no ambiguity is involved in the communication of observation” (emphasis ours). In other words, according to Bohr atomic physics is indeed objective, but not in the sense that its statements describe what really exists. Only in the sense that they are valid for anybody. This I express by saying they are but “weakly objective”. In a way, the attempts at building up theories of measurement that were made after Bohr’s time may be viewed as efforts aimed at imparting to “orthodox” quantum mechanics the status of a strongly objective theory, but it can be considered that these efforts failed.

Hence, quantum mechanics as we know it is not ontologically interpretable. This is not necessarily to be considered as a defect but it implies that, in the realm of interpretational problems such as the one here on hand, we should not argue as if it were. In particular the “collapse riddle” should prevent us from tacitly assuming that the wave function possesses in every circumstances all the attributes of reality. In fact the safest way to make use of the wave function is just to consider it as a component of a computational algorithm (or “rule”) that enables us to know the probability we have of observing such and such a measurement outcome on a system $S$ when we know how $S$ was prepared. Such measurement outcomes, described in a kind of a realist language (the pointer is at such and such a place etc.) may then be considered as elements of an empirical reality constituted by the phenomena understood in a Kantian sense, that is, as more than mere appearances but less than elements of some ontologically defined Reality, since they depend partly on us.
4. The proper mixture cannot be created.

This is the title of one of Kirpatrick’s sections and, in a sense, the statement it conveys - presented by its author as an objection to my views - is a correct one. Indeed, if we cling to the (here undisputed) view that a pure quantum state $|\Psi>$ of a system yields the maximal information that can be obtained on this system we have to consider that the idea of an observable physical diversity that would exist, independently of us, in an ensemble of isolated systems $D$ described by $|\Psi>$ is self-contradictory, and that the time evolution operator cannot all by itself generate that physical diversity. Concerning the case in which the systems $D$ are composite we must also admit that choosing to focus our attention on such and such features of $|\Psi>$ (by mathematical operations such as taking partial traces and so on) will never make $|\Psi>$ generate that physical diversity, which, however, constitutes the defining characteristic of what von Neumann called mixtures and I called proper mixtures. So, in this Kirkpatrick is right. Moreover, I showed ([3], chapter 17) that, in this respect, replacing such a pure state $|\Psi>$ by a mixture is of no help.

However, this means that the diversity in question has to be rejected, and that the same is therefore true concerning the ignorance interpretation of the “improper” mixtures (of subsystems of the $D$’s). But still we do observe diversity when, on statistical ensembles of systems, we perform observations. So, we face a difficulty. To study it let us consider the way Kirkpatrick presented his “proof” (that “the proper mixture cannot be created”). We must here quote him at some length. He asked “How might we go about creating a mixture, in particular a proper mixture?”. And he continued: “We return to von Neumann’s original description of the mixed state (echoed by d’Espagnat for the case of the proper mixture). The preparation of the system $S$ varies randomly among the possible output states $\{|\alpha_j>\}$; when $S$ is prepared in the state $|\alpha_j>$, the state of its relevant environment $E$ (a system external to $S$ such that $S+E$ has no correlations with its exterior) is $|\eta_j>$ and the composite system is described by the state $|\alpha_j\eta_j>$. Because $S+E$ has no exterior correlations, these states are indistinguishable; the Indistinguishability Rule requires the state of $S+E$ to be pure, the sum $|\Psi^{S+E}>=\sum_s\gamma_s|\alpha_s\eta_s>^\sim$. He then rewrote $|\Psi^{S+E}>$ in the form of a bi-orthogonal Schmidt-like decomposition, and claimed that the state of $S$ is the improper mixture obtained by tracing out $E$ on $|\Psi^{S+E}><\Psi^{S+E}|$. His
conclusion was: “it is not possible to create d’Espagnat’s proper mixture”.

I already explained why I consider that, in contexts of this type, Kirkpatrick’s use of the Indistinguishability Rule is faulty. Here, however, this is by no means an essential point for, in this passage, Kirkpatrick explicitly considered the preparation of a mixture, taking the environment of the system into account. Now, when we think in terms of preparation we may without generality loss (see above) imagine that the combined $S + \mathcal{E}$ system on which the mixture of the $S$’s is prepared is a pure state.

In fact, the trouble with Kirkpatrick’s approach lies at a much deeper level, tightly connected with the fact, commented on in the foregoing section, that “orthodox” quantum mechanics is not (as Bohm used to say) ontologically interpretable; that, in other words, the Reality it describes is but Empirical Reality. To see what the said trouble consists of, it suffices to have a careful look at the structure of Kirkpatrick’s above reported argument. What is crucial in it is the specification that the considered environment, $\mathcal{E}$, of $S$ be “[a system external to $S$] such that $S + \mathcal{E}$ has no correlations with its exterior”. When (or assuming that) this is the case, everything that Kirkpatrick wrote nicely follows... but on the other hand it is just pure mathematics, deprived of any bearing on possible observations since, in order that we should be be able to observe anything, some interaction must take place between us and either $S$ or $\mathcal{E}$ or both. Admittedly it could be assumed that the observing elements are themselves parts of $\mathcal{E}$. But then we would have to face a dilemma: either we consider that these “observing elements” are inanimate objects such as counters – but then nothing is gained since these counters must themselves be observed from outside $\mathcal{E}$ – or we assume that we, the “observers”, are ourselves within $\mathcal{E}$. However, we then have to face a riddle that all the (numerous) attempts at building up a consistent quantum measurement theory have not been able to resolve, namely the “and-or” enigma: wherefrom does it come that we have the feeling – nay the “certainty”! – of being either in the $|v_1>$ state or in the $|v_2>$ state even though the finest possible description of the whole state of affairs is of the type $c_1|u_1> + c_2|u_2> |v_2>$ i.e. contains both $|v_1>$ and $|v_2>$? Note that, obviously, in this matter dropping Kirkpatrick’s condition that $S + \mathcal{E}$ should have no correlations with its exterior would not help.

And yet, since we are dealing not with just pure mathematics but with physics,
we simply cannot do as if the just mentioned feeling – or rather, certainty – we have of always seeing pointers and other objects at definite places did not exist. One way of resolving this paradox (the only way I know of!) is to keep in mind something like Bohr’s above quoted assertion, that is, consider that the purpose of physics is not to describe “Reality as it really is” but, less ambitiously, to synthetically describe our communicable experience. Within the framework of such an approach wave functions, state operators and so on are, to repeat, essentially means of prediction of observations and the elements of the ensemble that we work with are pieces of empirical reality. And indeed, one of the most remarkable facts that quantum mechanics revealed is that, far from being restricted to the description of some – conjectured – man-independent Reality, mathematics are fully suitable for describing such a man-dependent empirical reality.

In particular, mathematics yield convenient tools for synthetizing our predictive knowledge concerning systems on which we assume that measurement have somehow been done without their outcomes being known to us (or, more generally, of which we assume that they interacted with macroscopic objects that can be treated classically). Von Neumann’s “ignorance” mixtures (my “proper mixtures”) are precisely the tools in question. And, within the empirical reality approach that we are here considering, to say that these tools cannot be created is no more true. Take a beam of spin 1/2 particles polarized along $Ox$. Send it through an inhomogeneous magnetic field directed along $Oz$ and put counters on the two emerging paths. If, in accordance with what the “man in the street” would say, you claim that, corresponding to each one of the beam particles, one (only) of the two counters did really click (in the empirical reality sense), then you have to grant that (in the same sense), beyond the counters the ensemble of the particles is a proper mixture.

It is true that, in principle, you are not quite obliged to take this standpoint. You may boldly say: “to claim that the counters either really clicked or did not really click is an overnaive conception of what Reality is”, and then you can resort to Kirkpatrick’s reasoning and state that the mixture is an improper one. If, notwithstanding the conceptual difficulties, you cling to the view that the wave function is an element of “Reality as it really is”, then – if, moreover, you believe quantum mechanics is universal – you are even forced to take up the latter viewpoint. But, to repeat, the conceptual difficulties just alluded to are, in fact, insuperable. The
Bohr-like view that quantum mechanics is really a weakly objective theory is therefore a considerably more reasonable (and I would even say: “scientific”) approach. And, to repeat, within its realm the notion of proper mixtures is fully valid.

5. Conclusion.

I think that, in substance, I answered all of Kirkpatrick’s objections, including those bearing explicitly on measurement. In the latter, what Kirkpatrick stressed is, in fact, just that, when a measurement occurs, the values of the measured observable must necessarily be correlated with something outside the system. This of course is true but I have already considered the matter in Section 4 above since the “something outside the system” is obviously a part of $E$. As his parenthesis beginning by “Curiously...” reveals, what Kirkpatrick did not realize is that, in the case of a measurement, the same subensemble of measured systems should be considered either as a proper or as an improper mixture, according to whether we choose to consider the instruments as being “on the classical side” or “on the quantum side”, that is, according to where we decide to situate – by thought – the quantum-classical cut (which, according to the views here reported in Section 3, separates the domain in which we can use a realist language from the one in which we cannot, and depends on what we are interested in).

One last but (I hope) not very significant point concerns Kirkpatrick’s remark, at the end of his Section 5, relative to the ignorance interpretation. He there speaks of the “temptation” to the interpretation of mixtures by ignorance, and he claims that the fact all mixtures are improper gives a clearer understanding of the said “temptation”. This language seems to mean that an ignorance interpretation of the improper mixture is actually inconsistent. But then, when, in the first section of his paper, we read: “D’Espagnat claims that an ignorance interpretation of the improper mixture is inconsistent” we are somewhat at a loss. We get to wonder where, according to him, the difference between us lies. Still, since he repeatedly speaks of my “error” and emphasizes its importance, there must exist some difference! One conjecture would be that he situates “diversity”, “well-defined values” and, therefore, “ignorance” at the extreme end of the von Neumann chain: within some nonphysical “taking cognizance of definite values” element in it. On the other
hand, it is clear that in his analysis of Hugues’ argument he took positions quite incompatible with this hypothesis. I, therefore, have no clue. In last resort I cannot completely rule out the hypothesis that, on this point, Kirkpatrick’s approach was not entirely consistent.

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