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Abstract. The Bell inequalities are based on a tacit assumption of a common probability distribution that precludes their application to the experiments of Aspect et al. The basic ideas of this argument have already been given in references [1,2], but the present presentation recollects them in a clearer and more concise way. The paper supersedes reference [3] which contains an error.

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1 The Bell inequalities and their application to the experiments of Aspect et al.

The subject matter of the Bell inequalities and the experiments of Aspect *et al.* hardly needs any introduction [4]. However, the argument has often been blurred by drawing in unnecessary issues, leading to some confusion. We give here an elementary derivation that removes all unnecessary considerations. This will show how elementary the argument is and how very hard it is to question the validity of the inequalities. We consider 4 variables $a_1 \in S$, $a_2 \in S$, $b_1 \in S$, $b_2 \in S$, where $S = \{0, 1\}$. The idea is that 0 correponds to absorption in a polarizer, and 1 to transmission. a_j will correspond to polarizer settings in one arm of the set-up, b_k to polarizer settings in the other arm. There are thus 16 possible combinations for the values of (a_1, a_2, b_1, b_2). By making a table of these 16 combinations it is easy to verify that we always have:

$$\forall (a_1, a_2, b_1, b_2) \in S^4 : \quad Q = a_1 b_1 - a_1 b_2 - a_2 b_1 - a_2 b_2 + a_2 + b_2 \in S. \tag{1}$$

We consider now functions $a_j \in F(V, S)$ and $b_k \in F(V, S)$. The notation means that the domain of the functions is V, while the functions take their values in S. Here V is a set of relevant variables for the experiment. We can call the set V the set of hidden variables, even if some of them may not really be hidden. One can imagine that V could be a subset of a vector space $\mathbb{R}^n$ or of a manifold, e.g. a non-abelian Lie group like SO(3). We have then:

$$\forall \lambda \in V : \quad 0 \leq Q(\lambda) = a_1(\lambda) b_1(\lambda) - a_1(\lambda) b_2(\lambda) - a_2(\lambda) b_1(\lambda) - a_2(\lambda) b_2(\lambda) + a_2(\lambda) + b_2(\lambda) \leq 1. \tag{2}$$

We can now consider a probability density p over V, i.e. $p(\lambda)\, d\lambda$. The function p belongs then to the set of functions $F(V, [0, \infty[)$ with domain V and values in $[0, \infty[$. We further require that $\int_V p(\lambda) d\lambda = 1$. We can now integrate Eq. 2 with p over V. Introducing the notations:

$$p(\alpha_j \wedge \beta_k) = \int_V a_j(\lambda)\, b_k(\lambda)\, p(\lambda)\, d\lambda, \quad p(\alpha_j) = \int_V a_j(\lambda)\, p(\lambda)\, d\lambda, \quad p(\beta_k) = \int_V b_k(\lambda)\, p(\lambda)\, d\lambda, \tag{3}$$

we obtain then:

$$0 \leq p(\alpha_1 \wedge \beta_1) - p(\alpha_1 \wedge \beta_2) - p(\alpha_2 \wedge \beta_1) - p(\alpha_2 \wedge \beta_2) + p(\alpha_2) + p(\beta_2) \leq 1. \tag{4}$$

This is the CHSH Bell inequality used in the experiments of Aspect et al. It is a purely mathematical identity and does not depend on any physical considerations. It is also free of any considerations about statistical bias and statistical independence. The probalities are identified with the mathematical expressions for the outcomes of the photon polarization experiments reported by Aspect *et al.*:

$$p(\alpha_j \wedge \beta_k) = \frac{1}{2} \cos^2(\alpha_j - \beta_k), \quad p(\alpha_j) = \frac{1}{2}, \quad p(\beta_k) = \frac{1}{2}, \tag{5}$$

where α_j and β_k are the angles of the polarizer settings in the two arms of the experiment. According to quantum theory the mathematical expressions are the limits of the measured probabilities when the number of registered events tends to infinity, i.e. when the statistics become perfect. For a function $f \in F(\mathbb{N},\mathbb{R})$, the limit $n \to \infty$ is defined by:

$$\lim_{n\to\infty} f(n) = F \Leftrightarrow (\forall \varepsilon > 0)(\exists N \in \mathbb{N})(n > N \Rightarrow |f(n) - F| < \varepsilon). \tag{6}$$

Here $f(n)$ would be the measured probabilities after n detection events, F the theoretical expression $\frac{1}{2}\cos^2(\alpha_j - \beta_k)$, and ε the statistical accuracy of the experiment required. An experimentalist has to worry about the statistical and instrumental precisions. For practical reasons the experimentalist can only reach a reasonable accuracy ε. But this should be well enough to establish beyond any reasonable doubt if the Bell inequality is satisfied or otherwise. We will adopt a mathematician's viewpoint and assume that the expressions $\frac{1}{2}\cos^2(\alpha_j - \beta_k)$ are exact, trusting that at least in principle the experimentalist could prove this to any accuracy ε, by improving the experimental protocol. We introduce thus the assumption (or act of faith) that the algebra of quantum mechanics is exact. This frees us from all imaginable polemics about experimental bias. For certain values of $(\alpha_1, \alpha_2, \beta_1, \beta_2) \in [0, 2\pi]^4$, the expressions in Eq. 5 do not satisfy the inequality in Eq. 4. This violation of the Bell inequality shows that the mathematical expressions in Eq. 5 are not compatible with Eq. 4. This seems to confirm Bohr's thesis that the polarizations cannot exist prior to a measurement and must be created by the measurement. But it is then extremely puzzling that we can obtain a definite correlation $\frac{1}{2}\cos^2(\alpha_j - \beta_k)$ because the polarizers can be separated by arbirarily large distances. It looks like the spooky action at a distance Einstein talked about and which has been called entanglement in the aftermath of the experiments. In the experiments of Aspect this issue is tested by ensuring Einstein separation of the detection events in both arms. The solution of this conundrum is in our opinion summarized in the last sentence of Section 3.

2 The tacit assumption

The derivation of the inequality looks unassailable. It is indeed ought to be too simple to possibly hide a logical loophole. But it does! What is not acknowledged is that the identification in Eq. 5 introduces a tacit assumption, which is admittdly hard to discern, namely that all four quantities $\frac{1}{2}\cos^2(\alpha_j - \beta_k)$ can be obtained from an integration over a same set V with a *same common distribution function* p. However, it can *a priori* not be excluded that in reality we can only obtain the quantities $\frac{1}{2}\cos^2(\alpha_j - \beta_k)$ from four different distributions p_{jk} according to:

$$\frac{1}{2}\cos^2(\alpha_j - \beta_k) = \int_{V_{jk}} a_j(\lambda)\, b_k(\lambda)\, p_{jk}(\lambda)\, d\lambda \tag{7}$$

rather than:

$$\exists V, \exists!\, p \in F(V,S) \parallel \forall (j,k) \in \{1,2\}^2 :\ \frac{1}{2}\cos^2(\alpha_j - \beta_k) = \int_V a_j(\lambda)\, b_k(\lambda)\, p(\lambda)\, d\lambda. \tag{8}$$

In other words, it is tacitly assumed that the quantities $\frac{1}{2}\cos^2(\alpha_j - \beta_k)$ can all be obtained from *one single common distribution function* p rather than from four different distributions p_{jk}. In view of the importance of the subject matter, one may desire an existence proof of such a unique function p. For a mathematician it will be immediately obvious that the necessity of such an existence proof is compelling. Unfortunately, a physicist may remain unfazed by a request for such a proof. It just would not set his alarm bells ringing. He may take the existence for self-evident and consider the request as unproductive and faultfinding mathematical nitpicking.

It is obvious that if we turn a polarizer, we turn a distribution of molecules. If one believes in hidden variables, then this distribution of molecules must be part of the hidden variables, because $\frac{1}{2}\cos^2(\alpha_j - \beta_k)$ changes when we turn one of the polarizers. Hence by turning a polarizer we change the hidden variables, such that *there are indeed four different sets* V_{jk}. One may have the intuition that it is trivial to make up for this. All one would have to do is to enlarge the sets V_{jk} to a set V, where $\forall (j,k) \in \{1,2\}^2 : V_{jk} \subset V$. But the problem is not only that we have to construct V. There is also a normalization problem as *the measured quantities* $\frac{1}{2}\cos^2(\alpha_j - \beta_k)$ *are beyond any doubt normalized individually according to* $\int_{V_{jk}} p_{jk}(\lambda)\, d\lambda = 1$ *rather than globally according to* $\int_V p(\lambda)\, d\lambda = 1$ (if V exists). In fact, the three probability distributions $p_{j'k'}$ for $(j', k') \neq (j,k)$ do not intervene in the measurement of $\frac{1}{2}\cos^2(\alpha_j - \beta_k)$, which stands on its own and is independent of our intentions to consider other experiments with the aim of measuring four quantities in total. This normalization problem may well be the reason for the violation of the Bell inequality.

3 Discussion: Why the objection is not at all futile or contrived

It is not my task to prove that assumption Eq. 8 is wrong. That would be a reversal of the charge of proof. All charge of proof is with the authors who proposed the Bell inequalities. I could stop here and wish them ironically good luck.

However, I think it will be a more reasonable and respectful attitude to try to provide the physicists in the audience with some arguments why my objection is not as farfetched as they may think. These arguments may look like a blend of physics and mathematics, but we will show that on close inspection they reveal to be all purely mathematical.

• Our first argument is that the contents of our objection are vindicated by quantum mechanics itself. If one wanted to claim they are wrong, one would thus have to claim that quantum mechanics is wrong. This argument will lead us straight into the heart of the Bohr-Einstein debate. The starting point of this debate was that when two operators do not commute, they will not have common eigenvectors. This is a purely mathematical truth. An example of this are $\hat{L}_x$ and $\hat{L}_y$.[1] According to Bohr the quantities L_x and L_y do then *not exist simultaneously.* Einstein wanted to prove that this cannot be true and proposed the EPR experiment for two correlated particles, whereby one would measure L_x for one of the particles at $\mathbf{r}_1$ in one arm of the set-up, and L_y for the other particle at $\mathbf{r}_2 = -\mathbf{r}_1$ in the other arm of the set-up. This would then demonstrate that Bohr was wrong. We know now that such a simultaneous measurement is indeed possible, because $\hat{L}_{x_1} = y_1 \frac{\partial}{\partial z_1} - z_1 \frac{\partial}{\partial y_1}$ and $\hat{L}_{y_2} = z_2 \frac{\partial}{\partial x_2} - x_2 \frac{\partial}{\partial z_2}$ do commute when $\mathbf{r}_1 \neq \mathbf{r}_2$. It is only when $\mathbf{r}_1 = \mathbf{r}_2$ that the operators do not commute, with all the consequences non-commutativity may entail. In a sense, Einstein addressed thus the wrong issue, because he no longer addressed operators that were not commuting, but he had been sidetracked by an overinterpretation of the mathematics introduced by Bohr.

We may in fact note that we discover here that the mathematical consequence of the fact that two operators $\hat{O}_1$ and $\hat{O}_2$ do not commute is in general not that O_1 and O_y would not exist simultaneously as Bohr claimed, but that $\hat{O}_1$ and $\hat{O}_2$ do not have a common probability distribution function. In fact, this will be true if we can use the eigenfunctions ψ_j of $\hat{O}_j \psi_j = O_j \psi_j$ to define the probability distributions $p_j = |\psi_j|^2$ for a set-up used to measure O_j, based on the Born rule. It is not because O_1 and O_2 cannot be measured simultaneously in a same location that they would not exist simultaneously in the same location. The difference with what Bohr said may look perhaps subtle but it is very important, because it implies that Bohr has overinterpreted the mathematics. In the case of the operators $\hat{L}_j$, the whole issue is actually a matter which is not at all subject to interpretation, because it is all just pure mathematics, the mathematics of the rotation group. These mathematics provide the full explanation about what is going on with the operators $\hat{L}_x$ and $\hat{L}_y$ and their eigenfunctions. Bohr presumably did not know this group theory. Using his physical intuition he cooked up a parallel theory by guessing an *ad hoc* explanation. But physical intuition and mathematical intuition are different things. His guesses were at variance with the correct mathematics. They were therefore plain wrong, but nobody knew at that time that group theory was at stake and nobody noticed. Nobody should be blamed for this. Bohr then went on pushing his ideas forcefully. Our rebuttal of the Bell inequality pinpoints his overinterpretation very accuractely. Bohr had a clash with Heisenberg over a very similar overinterpretation in a discussion about the uncertainty relations, which are also tied up with non-commuting operators.

Einstein reasoned on the overinterpretation provided to him by Bohr and was thus right with his intuition that this overinterpretation had to be wrong. We must further point out that the correct interpretation of the consequences of the fact that $\hat{L}_x$ and $\hat{L}_y$ do not commute can only be classical. The operators $\hat{L}_x$ and $\hat{L}_y$ exist in the group theory of the rotation group, which is mere Euclidean geometry. Their definitions and meaning are thus engraved in stone prior to any application in physics and are therefore not liable to any kind of warped alternative "physical" interpretation. Up to a proportionality constant $-\imath\hbar$, the operator $\hat{L}_z$ corresponds to $\frac{\partial}{\partial \varphi}$ in spherical coordinates (r, θ, φ) in $\mathbb{R}^3$ or polar coordinates (r, φ) in the Oxy plane. The operators $\hat{L}_j$ serve thus to calculate Lie derivatives in three mutually orthogonal directions. They are used to constitute a basis for the tangent space to the group at the identity element as they intervene in the calculation of the infinitesimal generators and of the elements of the Lie algebra. To obtain the Lie derivatives one must use one-parameter sets. These are just completely different for $\hat{L}_x$ and $\hat{L}_y$. The fact that $\hat{L}_x$ and $\hat{L}_y$ do not have common eigenfunctions only means that a rotation around the x-axis cannot simultaneously be a rotation around the y-axis [1]. In fact, $\hat{L}_x$ is not the operator for the x-component of the angular momentum $\boldsymbol{L}$, but for $\boldsymbol{L}$ when it is aligned with the x-axis, such that then $L_x = L$, $L_y = 0$, $L_z = 0$ and all components exist simultaneously [1] as they should in Euclidean geometry. This is illustrated by the fact that if we call R the rotation around the z-axis that rotates $\mathbf{e}_x$ to $\mathbf{e}_y$, then $\hat{L}_y = R\hat{L}_x R^{-1}$. Hence $\hat{L}_y$ is just the same Lie derivative as $\hat{L}_x$ but in another direction, i.e. for another one-parameter set.[2] Furthermore, one can associate an uncertainty relation with the fact that $\hat{L}_x$ and $\hat{L}_y$ do not commute. But as said, $\hat{L}_x$ and $\hat{L}_y$ are just part of the representation theory of the rotation group and there is absolutely no uncertainty in Euclidean geometry. For sure, all this flies in the face of many things we have learned, but that is just because we have been duped with misinterpretations of the mathematics. In summary, the whole algebra of angular momntum belongs just to the theory of the representations of the rotation group in Euclidean geometry and as such to classical mechanics. Hence, even though we are as physicists introduced to the conceptual world of

[1] Mind that we do not claim that the operators L_j are playing a rôle in the experiments of Aspect *et al.*. We just use them as an example.

[2] A spinor in SU(2) is a rotation [5], not a vector. Vector decompositions of spinors and rotations are therefore meaningless.

non-commuting operators and Lie groups by quantum mechanics, this world is not "quantum-mechanical" in the sense of non-classical. And to teach us these mathematics, our best guide may perhaps not be a quantum guru.

• Our second argument is another way of showing that the contents of our objection are vindicated by quantum mechanics itself. It is based on one of the ways one calculates probabilities in quantum mechanics. In fact, what one does is set up a Schrödinger equation, solve it to find the wave function ψ and then stipulate $p = |\psi|^2$, according to the Born rule. It must be mathematically obvious that one can in general not assume that the solution ψ_1 of a first Schrödinger equation will be equal to the solution ψ_2 of a second different Schrödinger equation. One can therefore in general not carry over probabilities that are valid for one set-up to another set-up. But this is exactly what the ansatz of a common probability distribution for the four different correlation experiments does. Of course this argument is completely equivalent to the first one. We just discover it here in a different guise.

• The first two arguments rely on the Born rule. This may come over as a cheat within a context where we are pitting quantum mechanics against classical thinking. We will explain below in Section 4 that the quadratic structure of the Born rule is not something specifically quantum mechanical but a very general result of group theory and therefore completely classical. The complete proof of the Born rule requires also physics and it can be proved in several cases that these are just classical physics. But at this stage the argument becomes too involved for what we need here, such that we abandoned the idea of developing it fully. The occurrence of various cases shows actually that there is not one Born rule, but several ones. To shortcut this problem we will therefore develop also classical arguments.

• A third argument which is completely classical beyond contention because it does not depend on the Born rule is that the definition of a probability depends on a full context and a protocol as one discovers by the paradox of Bertrand. One can connect this to the previous arguments. When we solve a Schrödinger equation, we take into account the necessity of outlining the full context by coding it into the boundary conditions, perhaps even unwittingly. It is well known that solutions of a Dirichlet problem can heavily depend on the boundary conditions. This illustrates the profound impact boundary conditions may have on solutions. This third argument highlights perhaps further the fact that the first two arguments do not need to be quantum mechanical and could be entirely classical. What would not be classical is Bohr's overinterpretation, which we can now appreciate to be wrong.

• The three arguments given up to now recollect what we already developed in [2]. A fourth argument consists in referring to Gleason's theorem which is obviously pure mathematics (and also does not depend on the Born rule). But this is of course very similar to the previous arguments. We list these four arguments as different arguments only because they might look different at first sight. When there is a hard nut to be cracked in order to solve a mathematical problem, one will forcedly hit it whatever the road one takes in trying to solve the problem. Call it a conservation law for hard nuts. By changing the approach we may only discover the hard nut in a different guise.

• In summary, we have been aware of this kind of objections for a long time, but some way they have been overlooked in deriving the inequalities, perhaps because it was considered that the objections were quantum mechanical and not classical such that one should not consider them in something that was supposed to represent classical thinking as opposed to quantum mechanical thinking! But what is here associated with classical thinking is only poor mathematical thinking based on "physical intuition" applied to problems that are purely mathematical! What is wrong and pollutes the whole debate are the overinterpretations of the mathematics the Copenhagen interpretation is teeming with and are supposed to define "quantum intuition". We must become aware of the fact that the Copenhagen interpretation has brainwashed us with the agitprop that mathematically wrong intuition would be a prerogative of classical rationalism while "quantum intuition" would be exact.

• Let us now explain why enlarging the sets V_{jk} to a set V in such a way that p will engulf all probabilities p_{jk} is all but trivial. It is obviously already in contradiction with the four arguments given above. In trying to follow our intuition and to define a common distribution function p one will run into all sorts of difficulties, which is normal because they are there to prevent us from deriving a contradiction from the mathematics. But as physicists we have been taught to take our strides with mathematical rigor, such that we are prone to make some booby traps go off. Very often we get away with our lack of rigor, but not this time. This time we have paid very dearly.

Let us try extend the sets V_{jk} to a larger set V.[3] Extending generously the set V to allow for all possible polarizer angles may render p a function of an infinite set of variables. Defining p for an infinite set of variables may require the axiom of choice, which is responsible for the Banach-Tarski paradox. In any case the statistical weight of a single angle in the extension would become zero: *An extension $V_{jk} \rightarrow V$ changes the normalizations of the probabilities p_{jk}*. What one can do in physics to avoid the zero probabilities is to select a polarizer angle α by introducing a delta measure

[3] Defining alternatively everything in terms of $V = V_{11}$, will leave us with different distributions p_{jk}. If one wants that Eq. 4 can be tested against Eq. 5 for all possible choices $(\alpha_1, \alpha_2, \beta_1, \beta_2) \in [0, 2\pi]^4$, we must make provision for all possible angles.

δ_α. But the derivation given above is based on functions, not on distributions. To circumvent this problem, one can introduce test functions T_u that in the sense of distributions converge to δ_α. An example is T_u, with $u \in]0,1[$:

$$T_u(x) = \begin{cases} \frac{1}{2u} & \forall x \in [-u,u] \\ 0 & \forall x \notin [-u,u] \end{cases} \quad \Rightarrow \quad \lim_{u\to+0} \int_{-\infty}^{+\infty} f(x)\, T_u(x)\, dx = f(0), \quad \lim_{u\to+0} T_u \rightsquigarrow \delta_0 \tag{9}$$

But how do we accomodate these test functions into the derivation given above? One cannot include the test functions for δ_{α_j} and δ_{β_k} into p else we will fall back onto the original p_{jk}. We may note in passing that the selection of an angle is actually not a probability. We are thus forced to consider $T_u(x)$ as outcomes of events. But the test functions cannot be considered as outcomes of events because they do not belong to $F(V,S)$, such that the inequality in Eq. 2 will no longer be granted. In the limit procedure the test functions even take arbitrarily large values as $\lim_{u\to 0+} T_u(0) = \infty$. The repair procedure does thus not fit into the scheme for the derivation outlined above. Its putative proof is not covered by the derivation and remains to be found. As explained above, both quantum and classical arguments indicate that the putative proof will just not exist. *That* is what the experiments are telling us! Other scenarios for constructing V and p *must* therefore lead to comparable problems (see e.g. Footnote 3), because there is also a conservation law for mathematical no-go theorems. We would finally like to point out that the angle $\alpha_j - \beta_k$ is *non-locally defined*, without any need for signalling and without any violation of relativity, such that this non-locality is not an issue [2]!

Epilogue. We noted that we could have stopped our rebuttal of the Bell inequalities at the end of Section 2. All the additions are just forced upon us by physicists, who are inert to requests for extreme rigor, such that we are forced to anticipate that they could pooh-pooh the objection in Section 2 despite the fact that it is a completely pertinent, terrible blow to the derivation of the Bell inequalities. We do not have the charge of proof to show that Eq. 8 is wrong. We only have to express reasonable doubt about it, making the reader wonder if it really goes without saying that we can introduce it without proof, under the pretext that it would be self-evident. The discussion shows that the derivation of the Bell inequality is not at all as obvious and trite as we were inclined to believe and that the purely mathematical problem of the existence of a common probability distribution is indeed the pivotal point on which the whole Bohr-Einstein debate hinges. In the derivation of the Bell inequalities, this issue has been swept unintentionally under the carpet. The lesson we must learn from this is that homespun intuition is bound to fail beyond a certain level of mathematical subtlety. Especially in probability calculus, which is fraught with nasty pitfalls, the danger of making an error is permanently lurking, This also transpires from our analysis of the double-slit experiment [2]. One should therefore eschew tackling deep foundational questions with a strategy that crucially depends on probability calculus.

4 Appendix: The quadratic character of the Born rule

We may think that the Born rule is eminently non-classical. But the quadratic character of the Born rule is an unavoidable consequence of the fact that vectors and four-vectors are "quadratic" rank-2 expressions in terms of spinors in the Lorentz group and in the rotation group [5]. It is just group theory. Let us give the reader some feeling for this idea. For the proof that the unit vectors $\mathbf{e}_x$, $\mathbf{e}_y$ and $\mathbf{e}_z$ of $\mathbb{R}^3$ are quadratic expressions of the spinors of the rotation group SU(2), we refer the reader to reference [5], more specifically to Eqs. 30-32. These equations tell us that for the basis vectors $\mathbf{e}_x = (x_1, y_1, z_1)$, $\mathbf{e}_y = (x_2, y_2, z_2)$, $\mathbf{e}_z = (x_3, y_3, z_3)$ of a rotated frame we have:

$$\begin{aligned}
&x_1 = \tfrac{1}{2}(\xi_0^2 - \xi_1^2 + \xi_0^{*2} - \xi_1^{*2}), && y_1 = \tfrac{\imath}{2}(\xi_0^2 + \xi_1^2 - \xi_0^{*2} - \xi_1^{*2}), && z_1 = -(\xi_0\xi_1 + \xi_0^*\xi_1^*),\\
&x_2 = \tfrac{\imath}{2}(-\xi_0^2 + \xi_1^2 + \xi_0^{*2} - \xi_1^{*2}), && y_2 = \tfrac{1}{2}(\xi_0^2 + \xi_1^2 + \xi_0^{*2} + \xi_1^{*2}), && z_2 = (\xi_0\xi_1 - \xi_0^*\xi_1^*),\\
&x_3 = \xi_0\xi_1^* + \xi_0^*\xi_1, && y_3 = \imath(\xi_0\xi_1^* - \xi_0^*\xi_1), && z_3 = \xi_0\xi_0^* - \xi_1\xi_1^*.
\end{aligned} \tag{10}$$

Here ξ_0 and ξ_1 are the components of the spinor $\boldsymbol{\xi}$:

$$\boldsymbol{\xi} = \begin{bmatrix} \xi_0 \\ \xi_1 \end{bmatrix}, \text{ which serves as a shorthand for the SU(2) matrix: } \mathbf{R}_0 = \begin{bmatrix} \xi_0 & -\xi_1^* \\ \xi_1 & \xi_0^* \end{bmatrix}, \tag{11}$$

of the rotation that was applied to the pristine canonical frame to obtain $\mathbf{e}_x$, $\mathbf{e}_y$, $\mathbf{e}_z$. By covariance, any vector $\mathbf{r} \in \mathbb{R}^3$ is a quadratic expression in terms of spinors. To generalize Eq. 10 to vectors $\mathbf{r}$ that are not unit vectors, we must use generalized spinors $\boldsymbol{\psi} = \sqrt{r}\,\boldsymbol{\xi}$. This can be generalized to Minkowski space-time. The four basis vectors of Minkowki space time are quadratic expressions of Lorentz spinors, and four-vectors in general quadratic expressions of spinors. The reader can get a feeling for this generalization from reference [5] where we generalize the results for the rotation group SU(2) in $\mathbb{R}^3$ to rotation groups in $\mathbb{R}^n$.

We know from relativity that charge and current density ρ and $\mathbf{j}$ are part of a charge-current density four-vector $(c\rho, \mathbf{j})$ for electric charges. A probability density is part of an analogous four-vector for "probability charges". We can call this the probability charge-current density four-vector. To stress the analogy, we will write this four-vector in what follows also as $(c\rho, \mathbf{j})$, such that we will use both ρ and p to refer to the probability density.

In the representation SL(2,$\mathbb{C}$) of the Lorentz group a four-vector $(c\rho, \mathbf{j})$ is expressed as $c\rho\mathbb{1} + \mathbf{j}\cdot\boldsymbol{\sigma}$ (see e.g. reference [1], Chapter 4). It transforms under Lorentz transformations $\mathbf{L} \in$ SL(2,$\mathbb{C}$) according to:

$$c\rho\mathbb{1} + \mathbf{j}\cdot\boldsymbol{\sigma} = \left[\begin{array}{cc} c\rho + j_z & j_x - \imath j_y \\ j_x + \imath j_y & c\rho - j_z \end{array} \right] \rightarrow \mathbf{L} \left[\begin{array}{cc} c\rho + j_z & j_x - \imath j_y \\ j_x + \imath j_y & c\rho - j_z \end{array} \right] \mathbf{L}^\dagger \tag{12}$$

Four-vectors are bilinear covariants, i.e. quadratic expressions in terms of spinors $\boldsymbol{\xi}$, which transform linearly: $\boldsymbol{\xi} \rightarrow \mathbf{L}\boldsymbol{\xi}$ under Lorentz transformations. The quadratic transformation law Eq. 12 is just another manifestation of the fact that four-vectors are quadratic expressions of spinors. One therefore often states that a spinor is the "square root of a vector".

The ultimate non-relativistic limit corresponds to $\mathbf{v} = \mathbf{0}$. We have then $\mathbf{j} = \mathbf{0}$ and $\mathbf{L}$ becomes a rotation $\mathbf{R}_0$, as the Lorentz group reduces to the rotation group SU(2), which is embedded within SL(2,$\mathbb{C}$). The four-vector reduces then to a scalar, because it becomes $(c\rho, \mathbf{0})$. For probability charges this scalar has to be positive-definite and of degree 2 in terms of a spinor $\boldsymbol{\xi}$, because, as said, four-vectors are quadratic expressions of spinors. The most simple quadratic expression for such a positive-definite scalar that comes to one's mind is $p\,[\,\boldsymbol{\xi}^\dagger\boldsymbol{\xi}\,]$. We have then $\boldsymbol{\chi} = \sqrt{p}\,\boldsymbol{\xi}$, and $p = \boldsymbol{\chi}^\dagger\boldsymbol{\chi}$. Well, to be quite honest, the solution is not unique. We could actually also propose $\boldsymbol{\chi}^\dagger\,\mathbf{L}^\dagger\,\mathbf{L}\,\boldsymbol{\chi}$, for some $\mathbf{L} \in$ SL(2,$\mathbb{C}$), but this would only correspond to a change of basis: $\boldsymbol{\chi} \rightarrow \mathbf{L}\boldsymbol{\chi}$ by a group transformation. The probality density is thus a quadratic expression in terms of a spinor field $\boldsymbol{\chi}$, that takes the values $\boldsymbol{\chi}(\mathbf{r}, t)$. What we still would have to explain is why this spinor field $\boldsymbol{\chi}$ is actually the wave function $\boldsymbol{\psi}$.

As explained, we abandoned the idea of explaining this in the present paper because the proof becomes really too involved. We can just give a sketch of the idea behind it. The spinor $\boldsymbol{\xi}$ represents a group element. It corresponds thus to a Lorentz transformation such that we can identify it with a Lorentz frame. For a single frame we have $\boldsymbol{\xi}^\dagger\boldsymbol{\xi} = 1$ (or $\boldsymbol{\xi}^\dagger\boldsymbol{\xi} = \gamma$ relativistically). This is the essence of what we explained above. Hence when we put $\boldsymbol{\psi} = \sqrt{p}\,\boldsymbol{\xi}$, p is actually a frame density. At this point we start needing some physics such that the argument will no longer be pure group theory, but the reasoning will be purely classical. In constructing the wave equation we attach a frame to each electron because the frame describes its dynamical state in terms of an instantaneous Lorentz transformation. Therefore the frame density p is the electron density, i.e. the probability density.

Of course, the detailed argument is quite elaborate because it relies on following it throughout the derivation of the Dirac equation given in [1]. We do not want all this to interfere with our discussion of the Bell inequalities. Our first two arguments of the discussion in Section 3 remain valid, even if we consider the Born rule as non-classical. It is only that a proof that the Born rule is classical would render these arguments even more poignant. It would prove that Eq. 8 is even wrong in classical mechanics and that our arguments do not rely on smuggling in "quantum effects". But the other two arguments are already pointing in the same direction. At least it must be clear from this section that the strangest aspect of the Born rule, viz. its quadratic character, is just a consequence of group theory. The probability density is thus a quadratic expression in terms of a spinor field $\boldsymbol{\chi}$, that takes the values $\boldsymbol{\chi}(\mathbf{r}, t)$, whereby it takes further physical arguments to prove that $\boldsymbol{\chi} = \boldsymbol{\psi}$.

The derivation above is valid within the context of the Schrödinger equation, where we neglect the relativistic effects of boosts. In a moving frame, we would have $\rho \rightarrow \gamma\rho$, but as $\gamma \approx 1$, its expression will not change in a non-relativistic context. We will only have also a current density $\mathbf{j} \neq \mathbf{0}$ when $\mathbf{v} \neq \mathbf{0}$. In the Schrödinger equation we actually only treat the phase of $\boldsymbol{\psi}$, considering the other components to be constant, such that we can drop them tacitly from the formalism. In other words, we replace:

$$\boldsymbol{\xi} = e^{\imath\varphi} \left[\begin{array}{c} 1 \\ 0 \end{array} \right] \rightarrow \xi = e^{\imath\varphi}, \quad \boldsymbol{\xi}^\dagger\boldsymbol{\xi} \rightarrow \xi^*\xi. \tag{13}$$

such that $\boldsymbol{\psi} \rightarrow \psi = \sqrt{p}\,e^{\imath\varphi}$ with $p = \psi^*\psi$. Within the Dirac theory, the definitions must be adjusted, but the expressions always follow from group theory. Strictly spoken, all this applies only to particles with spin 1/2. The Schrödinger equation is also used for other particles endowed with mass like He nuclei or atoms, which are described by other representations of the Lorentz group and the rotation group, such that these cases would require other proofs.

References

1. G. Coddens, in *From Spinors to Quantum Mechanics*, Imperial College Press, London (2015).
2. G. Coddens, `https://hal-cea.archives-ouvertes.fr/cea-01383609v4`.
3. G. Coddens, `https://arxiv.org/pdf/1309.6057.pdf`. This paper contains an error.
4. A. Shimony in *The New Physics*, Paul Davies ed., Cambridge University Press, Cambridge (1983), pp. 373-395.
5. An introduction to SU(2) and SO(3) is given in `https://hal-cea.archives-ouvertes.fr/cea-01572342`, combined with Chapter 3 of [1]. One cannot address quantum mechanics without a perfect mastery of spinors and group theory.