

# Correlation Coefficient of a Two-Channel Bell Test by Probability Theory

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**Abstract**—It is widely believed that only quantum mechanics can characterize the correlations of a Bell test. In fact, quantum mechanics is superfluous to the problem, and a correlation coefficient may be derived by probability theory. There is no “entanglement.” Everything in the experiment takes place by contact.

## I. INTRODUCTION

John S. Bell famously proposed a test supposed to prove that quantum mechanics was, in essence, magical [1]. I put the matter this way because the outcome of such an experiment is soluble by probability theory, and yet Bell seems to have claimed quantum mechanics alone could provide the One True Answer that surpassed all other solutions. At the time of this writing, Bell’s test is considered passed and the matter resolved in favor of the One True Answer.

Of course, what actually happened is that Bell *did not know* the test could be analyzed by means other than quantum mechanics, and also surely *already entertained the notion* that quantum mechanics might indeed be magical. Quantum mechanics has widely been regarded as some kind of “magic” for a very long time. However, some of us do not—at least when supposedly engaged in the scientific endeavor—entertain the notion that magic—or, indeed, anything remotely resembling it—exists. Furthermore, the Bell test problem is not, in fact, one of quantum mechanics at all. It is more properly a problem in random process analysis. It merely happens that quantum mechanics “knows” how to solve such a problem, without the “user” of quantum mechanics having to understand one iota of the subject of random process analysis.

You and I, however, wish to understand—and so shall we recast the problem as one of random signal analysis. We shall state a problem in random signal analysis, and the reader may confirm for themselves that it is equivalent to a two-channel Bell test as described in [1]. Then we shall derive a correlation coefficient, using probability theory rather than quantum mechanics. Because the result can be reached at all, of course it will be the same as that of quantum mechanics. Otherwise mathematics would be inconsistent.

## II. THE PROBLEM

There is a transmitter that sends a *signal* randomly from the set

$$S = \{\curvearrowright, \curvearrowleft\} \quad (1)$$

The transmission goes into both of two channels. Each channel attaches a *tag* to the signal, according to an algorithm to be specified below, and re-transmits the tagged signal. The tag comes from the set

$$T = \{\oplus, \ominus\} \quad (2)$$

The tagging algorithm works as follows. The channel is “tuned” by an angular setting  $\zeta \in [0, 2\pi]$ . Let  $r$  represent a number chosen uniformly from  $[0, 1]$ . Now suppose the signal  $\sigma$  is  $\curvearrowright$ . In that case, if  $r < \cos^2 \zeta$  then re-transmit  $(\oplus, \curvearrowright)$ . Otherwise re-transmit  $(\ominus, \curvearrowright)$ . On the other hand, suppose the signal is  $\sigma$  is  $\curvearrowleft$ . Then, if  $r < \sin^2 \zeta$  re-transmit  $(\oplus, \curvearrowleft)$ , else re-transmit  $(\ominus, \curvearrowleft)$ .

At the end of both channels is a receiver-recorder, which makes a record of received pairs of tagged signals, for some pair of “tunings” (or “ $\zeta$ -settings”)  $(\zeta_1, \zeta_2)$  for the two channels. For example, one hundred thousand or one million pairs of tagged signals might be recorded.

Now suppose we map tags to numbers,  $T \rightarrow T' = \{-1, +1\}$ , thus:

$$\oplus \mapsto +1 \quad (3)$$

$$\ominus \mapsto -1 \quad (4)$$

The problem is to use these numbers to calculate a correlation coefficient  $\rho$ , characterizing the correlation between tags in the received signal pairs, as a function of the difference between the two  $\zeta$ -settings.

## III. SOLUTION

We will use subscripts to refer to channel numbers. Thus, for example,  $\zeta_2$  may refer to a  $\zeta$ -setting for channel 2,  $\tau_1$  to a tag value for channel 1, and so on. An unsubscripted letter may refer to either channel. Thus, for instance,  $\tau$  may stand in for either or both of  $\tau_1$  and  $\tau_2$ . And so on like that.

We will use more or less conventional probability notation, though also always adding the letter “ $\lambda$ ” as a condition, meaning something such as “any relevant information we may so far have neglected.” It pays to be cautious.<sup>1</sup>

The  $\zeta$ -settings must be set each with respect to its own angular coordinate system. They cannot be assumed set with respect to the ether or the distant stars, nor with respect to each other. Therefore let  $\phi_{01}, \phi_{02} \in [0, 2\pi]$  be the *landmarks* (or *origins*) for the respective  $\zeta$ -settings, and let  $\phi_0$  represent either or both of these landmarks. Let  $\phi_1$  and  $\phi_2$  be respective setting values, and let  $\phi$  represent either or both of these values. Also introduce the notation

$$\Delta\phi_1 = \phi_1 - \phi_{01} \quad (5)$$

$$\Delta\phi_2 = \phi_2 - \phi_{02} \quad (6)$$

$$\Delta\phi = \phi - \phi_0 \quad (7)$$

By the problem definition, one immediately gets

$$P(\sigma = \curvearrowright \mid \lambda) = 1/2 \quad (8)$$

$$P(\sigma = \curvearrowleft \mid \lambda) = 1/2 \quad (9)$$

and

$$P(\tau = \oplus \mid \sigma = \curvearrowright, \zeta = \Delta\phi, \phi_0 \in [0, 2\pi], \lambda) = \cos^2 \Delta\phi \quad (10)$$

$$P(\tau = \ominus \mid \sigma = \curvearrowright, \zeta = \Delta\phi, \phi_0 \in [0, 2\pi], \lambda) = \sin^2 \Delta\phi \quad (11)$$

$$P(\tau = \oplus \mid \sigma = \curvearrowleft, \zeta = \Delta\phi, \phi_0 \in [0, 2\pi], \lambda) = \sin^2 \Delta\phi \quad (12)$$

$$P(\tau = \ominus \mid \sigma = \curvearrowleft, \zeta = \Delta\phi, \phi_0 \in [0, 2\pi], \lambda) = \cos^2 \Delta\phi \quad (13)$$

Here we are taking a liberty: actually, for  $\phi_{01}$  and  $\phi_{02}$  there should be a joint probability density function (pdf) specified in the conditions. However, we shall introduce this pdf only when it is about to be used. For now, consider it implicitly specified.

<sup>1</sup>Bell sometimes uses  $\lambda$  to represent what he calls “residual fluctuations” [2]. Whatever these be, they can be considered a portion of what we mean  $\lambda$  to cover.

We want to construct a table of probabilities of tagged signal pairs received by the receiver-recorder, so let us start by finding an expression for the following.

$$P_1 = P(\sigma = \curvearrowright, \tau_1 = \oplus, \tau_2 = \oplus | \zeta_1 = \Delta\phi_1, \zeta_2 = \Delta\phi_2, \phi_0 \in [0, 2\pi], \lambda) \quad (14)$$

However, because that does not fit well into a line of text, let us first introduce a shorthand, by writing something like

$$P_1 = P(\curvearrowright \oplus_1 \oplus_2 | \phi_1, \phi_2, [0, 2\pi], \lambda) \quad (15)$$

to mean the same thing. Then, by probability theory, and taking into account that  $\Delta\phi_1$  and  $\Delta\phi_2$  respectively pertain exclusively to channel 1 or channel 2 (so conditionality on the opposite channel's  $\Delta\phi$  may be dropped),

$$P_1 = P(\curvearrowright \oplus_1 \oplus_2 | \phi_1, \phi_2, [0, 2\pi], \lambda) \quad (16)$$

$$= P(\curvearrowright | \lambda) P(\oplus_1 \oplus_2 | \curvearrowright, \phi_1, \phi_2, [0, 2\pi], \lambda) \quad (17)$$

$$= P(\curvearrowright | \lambda) P_{11} P_{12} = \frac{1}{2} P_{11} P_{12} \quad (18)$$

where

$$P_{11} = P(\oplus_1 | \curvearrowright, \phi_1, [0, 2\pi], \lambda) = \cos^2 \Delta\phi_1 \quad (19)$$

$$P_{12} = P(\oplus_2 | \curvearrowright, \phi_2, [0, 2\pi], \lambda) = \cos^2 \Delta\phi_2 \quad (20)$$

By that and similar calculations the following table may be constructed.

$$P(\curvearrowright \oplus_1 \oplus_2 | \phi_1, \phi_2, [0, 2\pi], \lambda) = \frac{1}{2} \cos^2 \Delta\phi_1 \cos^2 \Delta\phi_2 \quad (21)$$

$$P(\curvearrowright \oplus_1 \ominus_2 | \phi_1, \phi_2, [0, 2\pi], \lambda) = \frac{1}{2} \cos^2 \Delta\phi_1 \sin^2 \Delta\phi_2 \quad (22)$$

$$P(\curvearrowright \ominus_1 \oplus_2 | \phi_1, \phi_2, [0, 2\pi], \lambda) = \frac{1}{2} \sin^2 \Delta\phi_1 \cos^2 \Delta\phi_2 \quad (23)$$

$$P(\curvearrowright \ominus_1 \ominus_2 | \phi_1, \phi_2, [0, 2\pi], \lambda) = \frac{1}{2} \sin^2 \Delta\phi_1 \sin^2 \Delta\phi_2 \quad (24)$$

$$P(\curvearrowleft \oplus_1 \oplus_2 | \phi_1, \phi_2, [0, 2\pi], \lambda) = \frac{1}{2} \sin^2 \Delta\phi_1 \sin^2 \Delta\phi_2 \quad (25)$$

$$P(\curvearrowleft \oplus_1 \ominus_2 | \phi_1, \phi_2, [0, 2\pi], \lambda) = \frac{1}{2} \sin^2 \Delta\phi_1 \cos^2 \Delta\phi_2 \quad (26)$$

$$P(\curvearrowleft \ominus_1 \oplus_2 | \phi_1, \phi_2, [0, 2\pi], \lambda) = \frac{1}{2} \cos^2 \Delta\phi_1 \sin^2 \Delta\phi_2 \quad (27)$$

$$P(\curvearrowleft \ominus_1 \ominus_2 | \phi_1, \phi_2, [0, 2\pi], \lambda) = \frac{1}{2} \cos^2 \Delta\phi_1 \cos^2 \Delta\phi_2 \quad (28)$$

Disregarding the signal values gives (by addition of probabilities)

$$\begin{aligned} P(\oplus_1 \oplus_2 | \phi_1, \phi_2, [0, 2\pi], \lambda) &= P(\ominus_1 \ominus_2 | \phi_1, \phi_2, [0, 2\pi], \lambda) \\ &= \frac{1}{2} \cos^2 \Delta\phi_1 \cos^2 \Delta\phi_2 \\ &\quad + \frac{1}{2} \sin^2 \Delta\phi_1 \sin^2 \Delta\phi_2 \end{aligned} \quad (29)$$

and

$$\begin{aligned} P(\oplus_1 \ominus_2 | \phi_1, \phi_2, [0, 2\pi], \lambda) &= P(\ominus_1 \oplus_2 | \phi_1, \phi_2, [0, 2\pi], \lambda) \\ &= \frac{1}{2} \cos^2 \Delta\phi_1 \sin^2 \Delta\phi_2 \\ &\quad + \frac{1}{2} \sin^2 \Delta\phi_1 \cos^2 \Delta\phi_2 \end{aligned} \quad (30)$$

We want to calculate the correlation coefficient

$$\rho = \frac{E(\tau'_1 \tau'_2)}{\sqrt{E(\tau'^2_1)} \sqrt{E(\tau'^2_2)}} \quad (31)$$

where  $\tau'_1, \tau'_2 \in T'$  and the expectations  $E$  are calculated with respect to the conditional probabilities derived above. The numerator is the covariance and the denominator is the product of the standard deviations.

The choice of values for the elements of  $T'$  makes it so the standard deviations in (31) equal one, and thus the correlation coefficient simplifies to the covariance

$$\rho = E(\tau'_1 \tau'_2) \quad (32)$$

which we now must calculate. To do so, not only must we compute a sum weighted by the probabilities in (29) and (30), but we must also eliminate the angular landmarks.

First let us calculate an average of  $\tau'_1 \tau'_2$  weighted by the probabilities in (29) and (30), and call that sum  $\rho'$ .

$$\begin{aligned} \rho' &= (+1)(+1)P(\oplus_1 \oplus_2 | \phi_1, \phi_2, [0, 2\pi], \lambda) \\ &\quad + (+1)(-1)P(\oplus_1 \ominus_2 | \phi_1, \phi_2, [0, 2\pi], \lambda) \\ &\quad + (-1)(+1)P(\ominus_1 \oplus_2 | \phi_1, \phi_2, [0, 2\pi], \lambda) \\ &\quad + (-1)(-1)P(\ominus_1 \ominus_2 | \phi_1, \phi_2, [0, 2\pi], \lambda) \end{aligned} \quad (33)$$

Substituting for the probabilities gives

$$\begin{aligned} \rho' &= \cos^2 \Delta\phi_1 \cos^2 \Delta\phi_2 - \cos^2 \Delta\phi_1 \sin^2 \Delta\phi_2 \\ &\quad - \sin^2 \Delta\phi_1 \cos^2 \Delta\phi_2 + \sin^2 \Delta\phi_1 \sin^2 \Delta\phi_2 \end{aligned} \quad (34)$$

and so

$$\rho' = (\cos^2 \Delta\phi_2 - \sin^2 \Delta\phi_2)(\cos^2 \Delta\phi_1 - \sin^2 \Delta\phi_1) \quad (35)$$

$$= \cos(2\Delta\phi_2) \cos(2\Delta\phi_1) \quad (36)$$

with the last step by one of the double-angle identities.<sup>2</sup>

To eliminate the landmarks  $\phi_{01}$  and  $\phi_{02}$ , we will have to compute a double integral weighted by a joint pdf. What pdf to use? The right side of (36) is symmetric in  $\phi_{01}$  and  $\phi_{02}$ , so let us arbitrarily choose  $\phi_{01}$  to talk about first. Whatever follows in our arguments must apply to all possible arrangements, and so the probability density of  $\phi_{01}$  must be uniform, independently of the density of  $\phi_{02}$ . For  $\phi_{02}$ , on the other hand, the situation is different. Our goal is to compute the rotation-invariant correlation coefficient, given the difference between the two  $\zeta$ -settings. Therefore  $\phi_{02}$  must equal  $\phi_{01}$  with probability one. The necessary joint pdf is thus the Dirac delta  $\delta(\phi_{02} - \phi_{01})$ . We do a double integration of (36), both integrals going from a point on the protractor to another.

There will be this complication, however: we have to treat all landmarks (and thus angular coordinate systems) alike, yet the cosine sometimes is reversed in sign, which would lead to  $\rho$  being reversed in sign in some quadrants. Luckily, the plus-or-minus sense of a correlation coefficient is arbitrary, so we can simply change the sign

<sup>2</sup>Though the function does not have the correct form and so cannot be the solution, this is probably where a quantum physicist would believe they had solved the problem. Their version would leave out the landmarks.

back to how we need it. We will also simplify the integrations by substituting  $\theta = \Delta\phi_1$ . Thus

$$\rho = \int_0^{\frac{\pi}{4}} \int_0^{2\pi} \cos(2\Delta\phi_2) \cos(2\theta) \delta(\phi_{02} - \phi_1) d\phi_{02} d\theta \\ + \int_{\frac{7\pi}{4}}^{2\pi} \int_0^{2\pi} \cos(2\Delta\phi_2) \cos(2\theta) \delta(\phi_{02} - \phi_1) d\phi_{02} d\theta \quad (37)$$

if  $\theta \in [0, \frac{\pi}{4}] \cup [\frac{7\pi}{4}, 2\pi]$ ,

$$-\rho = \int_{\frac{\pi}{4}}^{\frac{3\pi}{4}} \int_0^{2\pi} \cos(2\Delta\phi_2) \cos(2\theta) \delta(\phi_{02} - \phi_1) d\phi_{02} d\theta \quad (38)$$

if  $\theta \in [\frac{\pi}{4}, \frac{3\pi}{4}]$ ,

$$\rho = \int_{\frac{3\pi}{4}}^{\frac{5\pi}{4}} \int_0^{2\pi} \cos(2\Delta\phi_2) \cos(2\theta) \delta(\phi_{02} - \phi_1) d\phi_{02} d\theta \quad (39)$$

if  $\theta \in [\frac{3\pi}{4}, \frac{5\pi}{4}]$ , and

$$-\rho = \int_{\frac{5\pi}{4}}^{\frac{7\pi}{4}} \int_0^{2\pi} \cos(2\Delta\phi_2) \cos(2\theta) \delta(\phi_{02} - \phi_1) d\phi_{02} d\theta \quad (40)$$

if  $\theta \in [\frac{5\pi}{4}, \frac{7\pi}{4}]$ .

In each case the result is the same, so

$$\rho = \cos(2(\phi_2 - \phi_1)) = \cos^2(\phi_2 - \phi_1) - \sin^2(\phi_2 - \phi_1) \quad (41)$$

regardless of quadrant. The formula obviously extends to all  $\phi_1, \phi_2 \in \mathbb{R}$ .

This result accords with quantum mechanics, as it must. Otherwise either quantum mechanics or our solution would be wrong.

#### IV. DISCUSSION

I should not belabor this note with too many details of how Bell went wrong in his arguments. His writings simply are devoid of *correct* methods of logical inference. Instead we encounter *imitations* of logical inference.

For example, Bell famously introduces a mathematical contradiction with the goal of producing a physical absurdity [2]. In fact, this is the crux of his argument in its best known form, but it is unsound reasoning. One cannot deduce *anything* from a mathematical contradiction, except that the mathematical assumptions behind it are wrong. In other words, all Bell proved is that he made a mistake! Indeed, he made what is probably the most frequently made mistake in probability theory: factoring a joint probability incorrectly. However, Bell was *imitating* logical inference, not employing the real thing with skill. For himself and his audience that had not thought this through, an imitation was good enough.<sup>3</sup>

Perhaps, finally, we have reached the limit of what we should tolerate. Perhaps, now that millions and millions of dollars have been wasted, and graduate students' lives increasingly are being wasted, it is time to put our foot down and say it is enough. Bell was unable to reason like a true scientist. And now an entire batch of fields related to his work is supposed to be our society's greatest "geniuses," but, by a failure in education, its members do not visualize that a two-channel Bell test is a totally ordinary, causal, contact-action random process that is, mathematically, shaped like a pair of wheels, which may have imposed upon them any angular coordinate system one wishes.

<sup>3</sup>Perhaps Bell confused mathematical "*reductio ad absurdum*"—more properly called proof by contradiction—with the more general method of argumentation called *reductio ad absurdum*. But one cannot use a mathematical contradiction to prove a general absurdity, because a mathematical contradiction is vacuous.

Among the corollaries of that "shape" is that, if you rotate  $\phi_1$  and  $\phi_2$  in unison, the frequencies of tag values change (also in unison)<sup>4</sup> but the correlation coefficient remains invariant. Furthermore, it should be experimentally *impossible* to violate the so-called "CHSH inequality" [3], if the experiment be properly designed. This inequality applies a test that itself is not invariant under rotations of the apparatus. For Bell-test angles, in the case of photon experiments, a rotation by  $\pi/8$  should see a drop in magnitude of the CHSH "quantum correlation"  $E$  from  $\sqrt{1/2}$  to zero, so  $|S|$  (in the ideal) adds up to  $\sqrt{2}$  instead of  $2\sqrt{2}$ . The reason is that  $E$ , in the CHSH formulation, is *not* actually the expectation of experimental outcomes, as claimed. Or, to put it another way, it is an expectation conditional on a particular coordinate system—but this is not what quantum mechanics gives, and it is not what one wants. Physicists have left out the steps that took *our* calculation from  $\rho'$  to  $\rho$  proper, and which gave us the *correct*, *rotation-invariant* correlation coefficient. Yet experimenters report positive results. Therefore not only is the inequality itself a failure of scientific methods, so must be the experiments.

Furthermore, any derivation of the CHSH inequality, like Bell's reasoning discussed earlier, merely mimics logical reasoning. For mathematics to be consistent, all methods must reach the same result. Thus, when the CHSH "quantum correlation" is not the same as the correlation coefficient derived by quantum mechanics, the conclusion should *not* be that quantum mechanics is "different" from classical physics. The *correct* conclusion is that there is an error in the CHSH derivation, and that the researcher must persist—even if it takes years, and an education in unfamiliar branches of mathematics—until the derivation is correct. Quantum physicists, instead, publish a paper "confirming quantum mechanics," then collect citation counts. It is a cushy job, insofar as intellect is concerned, but this kind of work is what our "scientific" community has become. It is a perversion of science.

At this point I shall state unequivocally that there is no such thing as "entanglement." I write not for the journals but directly to the reader and so may speak freely. I need not be a cheap philosopher and hedge with "There might be action at a distance even though we have proven conclusively that there need not be." No. Oppositely polarized photons in a Bell test experiment are simply oppositely polarized photons, not some kind of "mixture." It is obvious that a "superposition state" or any such verbally obfuscated stuff must always simply be different notation for some probability expression, wave coherence expression, or the like. The real reason anyone believes there is magic involved seems to be not that the magic exists, but that some Fathers of the early Quantum Church (although perhaps not Niels Bohr himself [4]) strictly forbade treatment of quantum mechanics in the classical fashion. Physicists have been true to that stricture since and simply do not *permit* the heresy of a solution by classical methods.<sup>5</sup> Our universities are cult indoctrination centers.<sup>6</sup> One is permitted *only* to reach some fake "solution" different from that of quantum mechanics, which then is used to "prove" that quantum mechanics is "different." Otherwise physicists would have discovered long ago what we discovered by example above: that there

<sup>4</sup>Which could be computer-animated in numerous interesting ways.

<sup>5</sup>Indeed, some Bell tests are soluble by *electromagnetic wave theory* [5], which is as classical a physics as one can imagine, but this fact is dismissed one way or another by the orthodoxy.

<sup>6</sup>As an engineering student in the early 1980s, the author himself encountered what apparently was a common practice in approximately the 1970s: the professor would *boom*: "Do not try to understand quantum mechanics! Just do the calculations!" This was an actual command. Being commanded by my physics professor shook me painfully and permanently—particularly because, in general, he was the best teacher I ever had, who gave instructive homework and never made the exams cruelly difficult.

actually *is no* “modern physics”—just “classical physics” that no one has been allowed to investigate.

Physicists, in their roles as researchers and professors, apparently do not teach themselves and their students *how* to reach solutions of random process problems, except by the “magic” of quantum mechanics. *I* knew how because *I* majored not in physics at all, but in the very closely related subject of *electrical engineering*—where mistakes are punishable by corporate bankruptcy, and sometimes even by prison for the engineer. In basic research there may be no consequences for simply repeating orthodoxy, be it right or wrong. Peer review further reinforces the orthodoxy. Orthodox papers in quantum physics are reviewed by orthodox quantum physicists for publication in orthodox journals. Peer review is useless, in this instance, to serve the scientific mission. The papers never are subjected to analysis by experts in random process analysis, who *are not* quantum physicists, and who would tear the arguments to shreds. The papers are reviewed instead by persons ignorant of random process analysis, who delight in conclusions that reinforce orthodox doctrine. Careers rise as the peers see each others’ papers published in the most elite of journals. Eventually, the Nobel Prize in Physics for 2022 is awarded for superlative achievements in the pursuit of what, in fact, is balderdash [6]. But the winners cannot see that it is balderdash. Few in the field have even the background to see that the achievement was nothing at all.

The author of *this* paper would tell a Nobel Committee to take their prize and dump it in *Östersjön*, and recommends the same for all Nobels in science and mathematics. He does not believe in prizes and awards, and thinks that no scientist should be labeled as privileged over another. An artificial reef of Nobel medals could form a home for fish and crabs. Prizes for science are unethical and destructive at best [7], [8], but now also shown to be awarded without distinction to both actual discovery and the pursuit of nothing. Which is better, to be insulted with a cash reward for success in a system that is a perversion of science—which rewards publication and citation-counting over self-education and stubborn persistence—or to be a humble pursuer of the *actual* scientific enterprise, which is to clear up mysteries [9]? Let the gold medals belong to the crabs.

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