Unuploaded experiments have no result

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Abstract

The aim of this note is to attract once again attention of the quantum community to statistical analysis of data which was reported as violating Bell’s inequality. This analysis suffers of a number of problems. And the main problem is that rough data is practically unavailable. However, experiments which are not followed by the open access to the rough data have to be considered as with no result. The absence of rough data generates a variety of problems in statistical interpretation of the results of Bell’s type experiment. One may hope that this note would stimulate experimenters to create the open access data-base for, e.g., Bell tests. Unfortunately, recently announced experimental loophole-free violation of a Bell inequality using entangled electron spins separated by 1.3 km was not supported by open-access data. Therefore in accordance with our approach “it has no result.” The promising data after publication is, of course, a step towards fair analysis quantum experiments. May be this is a consequence of appearance of this preprint, v1. But there are a few questions which would be interesting to clarify before publication (and which we shall discuss in this note).

1 Introduction

By publishing the previous version of this preprint I hoped that it would have some impact to experimenters performing quantum foundational experiments and first of all experiments on violation of the Bell inequality. For the latter problem, availability of rough data for independent statistical analysis is very important. During the last
years a few leading experimental groups demonstrated that they are very close to close all possible (or at least basic) loopholes in the Bell test, we mention, e.g., Zeilinger’s group [1] and Kwiat’s group [2]. The rough data for such “finalizing experiments” is till not approachable. Here the main problem is the statistical significance of the announced results, see [3]–[5] for discussions.

Without rough data, it may happen that there will be declared that the loophole free test has been successfully performed. After this further experimental studies would be considered as meaningless and the result which may be not justified from the viewpoint of an independent statistical expertise would be everywhere cited as the final result closing the Bell-violation problem.

It seems that v1 of this preprint had some impact to experimenters. For example, the team announced “experimental loophole-free violation of a Bell inequality using entangled electron spins separated by 1.3 km” [6] promised open-access to rough data after publication. This is definitely an important step towards fair analysis of quantum experiments. However, there are a few questions which would be interesting to clarify even before publication. We shall mention some of them in new section, section 4, completing v1 of this preprint. Thus the reader who have already read v1 can jump directly to section 4.

One of the basic principles of the scientific methodology is that experimental data have to be approachable by researchers independent from the group performed a reported experiment. Statistics is a very delicate area of research and it is worse to provide a possibility to check statistical conclusions which are typically presented in the compact form in articles of experimenters (PhD-thesis are better, but there are still no rough click by click data, although nowadays data can be easily uploaded to the corresponding website containing the pdf-file of the PhD-thesis). Unfortunately, to put data on lab’s website is not a tradition in the quantum community, opposite to others, e.g., molecular biology and genetics as well as psychology. This situation has to be improved, as soon as possible. A. Peres famously pointed out that “Unperformed experiments have no results” [7]; I would add that “Unuploaded experiments have no result.”

Nowadays experimental testing of violations of Bell’s inequality [8], [9] is one of the most important foundational projects. It also plays the determining role for some quantum technologies, especially quantum random generators [10][1]. Therefore impossibility of open access to click by click data for such tests is really a problem. Quantum foun-
dational community really needs such data. However, even data from other quantum foundational experiments (and not only with photons) is badly demanded.

In spite of my very wide network of contacts with experimenters, I was able to approach only one set of rough data, click by click, from the experiment of G. Weihs [11], [12]. However, this experiment was done long ago. Since that time the quantum experimental technology was improved crucially. In particular, at that time there were used detectors of low efficiency and it might be that majority of “strangenesses in Weihs’ data” are simply consequences of the low detection efficiency. Another my source of data is even older, this is the PhD-thesis of A. Aspect [13]. These data suffers of the same problems which might be again simply consequences of low efficiency of detectors.

The absence of data or even worse consequences of the statistical analysis of the available data [14] in combination with impossibility to get new data lead to a number of problems which permanently disturb my mind and make me really suffering. Among them I would like to mention the following four problems which will be discussed in the section 2:

• statistical justification of no-signaling;
• “Aspect’s type anomalies” in data;
• the time window problem (coincidence loophole);
• statistical analysis of Bell’s experiment.

We also remark that the length of Bell’s experimenting and the strong wish to consider this problem as “practically closed” made the quantum community tiered; majority of the community lives with the very strong belief that the problem was completely clarified – depending on a person it can be said that it was done “already by Aspect” or “by Weihs”, “by Zeilinger’s team”, by “Kwiat’s group”. Yes, the sudy is much longer than one could expect at the beginning, but it is still far from to be completed. And the problem is not only to close all loopholes (as many naively believe), but to perform a proper statistical analysis of data for experiments declaring closing of particular loopholes. For a moment, I can mention only three papers which authors attempted to perform such an analysis, [14], [10], [3].

After the recent experiments closing the detection loophole\(^2\), the Vienna experiment [1] and the Urbana-Champaign experiment [2], it

\(^2\)These were really great experiments closing one of the most important loopholes (in my opinion the most important) and I propose to assign them the names, e.g., by the places were they have been done.
is commonly accepted that there is only one step to complete Bell’s project: to close in a single experiment both locality and detection efficiency loopholes. I agree that such an experiment will of the great value, both for quantum foundations and technologies. However, it would be still not the end of the Bell-story. It has to be completed by the detailed statistical analysis of data and I expect that it will take a long time to approach consensus on the results of this analysis. I repeat again that statistics is a delicate science.

One can imagine a kind of EU project on “certification” of violation of Bell’s type inequalities: each experiment closes the concrete loophole or a group of loopholes and the complete data are in open access. It would be not so bad to double such a data-base by certifying violations of Bell’s inequality by some leading group, e.g., in USA. Such certification is really needed if we plan to proceed to quantum technologies, especially based on quantum random generators and (less) on quantum cryptography. Moreover, the majority of the basic quantum foundational experiments have been done long ago and the rough data is neither available for the open access. Where can one find data for the two slit experiment with photons? with electrons? data of Weihs’ three slit experiment \cite{15}? experiments of neutron interferometry? experiments on “photon existence” – estimation of the coefficient of second order coherence $g^2(0)$ (with heralded photons and with a genuine single photon source)?

Besides to secure the users of the quantum technologies that in reality everything matches theory, such a data-base would simplify essentially the foundational debates and restrict the opposition to the conventional interpretation of violation of Bell’s inequality: only conclusions of people educated in statistics would be considered seriously. Nowadays the absence of the rough data provides possibilities for practically everybody to speculate and say: “well, there is no data, it might be that, in fact, ...”

2 Three issues disturbing me

2.1 Signaling

My impression is that experimental groups reporting a violation of Bell’s inequality do not check the condition of no-signaling. They are fine by violating the Bell inequality by as many $\sigma$ as they can. At the same time, the data which I was able to get always violate the condition of no-signaling. Of course, the main problem was that these data were really old. And there are practically no possibility to get new rough data.
I start with the remark that, in fact, the terminology no-signaling is ambiguous. In reality we want just to check that the marginal probabilities on side $S_1$ obtained by summation with respect to the results on the other side $S_2$ do not depend on settings on $S_2$:

$$p_{i;L}(+) = p_{ij}(++) + p_{ij}(+-), p_{i;L}(-) = p_{ij}(-+) + p_{ij}(--)$$  \hspace{1cm} (1)

where $i, j$ encode experimental settings, the angles $\theta_i, \theta_j$ of the orientation of polarization beam splitters at the left-hand and right-hand sides labs. In the same way

$$p_{j;R}(+) = p_{ij}(++) + p_{ij}(-+), p_{j;R}(-) = p_{ij}(+-) + p_{ij}(--)$$  \hspace{1cm} (2)

This is one of necessary conditions for existence of the classical probability distribution serving for the experiment, see, e.g., [16], [4] for details. This is simply the condition of additivity of probability\(^\text{3}\).

However, by following the physical tradition we shall keep to the terminology (no-)signaling.

The presence of “signaling” in experimental data is very disturbing for me. In Aspect’s experiment [17], the assumption of no-signaling is violated (this can be extracted from his PhD thesis [13].) This assumption is violated by Weihs’ data as well [14]. We shall discuss later possible sources of violation of no-signaling.

Now I want to make a point [19]:

Formally, experimental data violating both Bell’s inequality and no-signaling either cannot be used against local realism or such data have to be used to argue that both local both local realism and QM have to be rejected. The latter predicts no-signaling. You got signaling? Then you have to reject QM. One of, course, does not want to proceed in this way and experimenter would come with detailed explanation of all technicalities which lead to signaling. I do not question these explanations; experts know technicalities perfectly. So, the role of these technicalities have to be taken seriously. However, in this situation it is logically reasonable to accept that violations of Bell’s inequality might be explained by other technicalities and take seriously the arguments of such a type, e.g., [20], [21], [22].

Even by having data one confronts another extremely difficult problem, namely, the problem of selecting of proper statistical test for no-signaling. Perhaps the most complete analysis of a Bell-type

\(^{3}\)Violation of this condition in the two slit experiment, where one of indexes is used to label a slit, and another to label a point at the registration photo-emulsion screen, was discussed by Feynman [18]. The general approach to interference as violation of additivity of probability was elaborated in a series of my works, e.g., [16].
The experiment is the one given by Pironio et al. [10]. They performed statistical tests to check for signaling problems (as we defined it as consistency condition for marginal distributions), see the supplementary information of this paper. However, I cannot point to other publications on this problem and of such a level of statistical analysis. At the same time applicability of the statical test used in [10] to data from Bell’s type experiments can be questioned, and it was questioned in Yanbao Zhang et al. [3], where a detailed analysis of specialties of such data. Therefore it is important to have open access data, click by click, which can be used by experts in statistics.

2.2 “Anomalies” in Aspect’s data

A few years ago Alain Aspect told me about some strange anomalies which he founded in his data, data from the pioneer experiment [13], and these anomalies can be found in his thesis. That his data exhibited a strange behavior. If we take the joint probabilities

\[ p_{ij}(+,+),...,p_{ij}(-,-), \]  

where \( i, j \) encode experimental settings, then these probabilities differ from the prediction of QM for maximally entangled state, i.e., from

\[ p_{ij}(++) = p_{ij}((--) = 1/2 \cos^2(\theta_i - \theta_j)/2, \] \[ p_{ij}(++- = p_{ij}(-++) = 1/2 \sin^2(\theta_i - \theta_j)/2. \]  

However, at the same time the expressions for correlations

\[ E(ij) = p_{ij}(++) - p_{ij}(+-) - p_{ij}(-+) + p_{ij}((--) \]  

match very well with ones calculated from theory, for the maximally entangled state. Probabilities in \( E(ij) \) compensate each other in a mysterious way. The same we have seen in Weihs’ data [14]. Our attempt to solve the latter problem by considering non-maximally entangled states was not successful (calculations and simulation were performed by Adenier [14]); we were neither satisfied by Weihs’ explanation in terms of mixed states [23].

I have a plenty of discussions about these anomalies with the top experts; they pointed to a variety of technicalities which might lead to the anomalies. The main issue is that it might be that these experiments were calibrated for correlations and not for states. The experimenters also pointed to possible instabilities in the state production, pair production rates and measurement settings. I suppose that in modern experiments these characteristics were essentially improved.
However, I am not completely sure, because by attacking new loopholes experimenters met tremendous new challenges and they often ignore smallnesses as, e.g., stability of the state production or the pair rate production.

For an expert in quantum foundation who is not an experimenter by origin, the statement that these experiments were calibrated for correlations and not for states is really disturbing. Would it be in principle possible to “calibrate” an experiment in such a way that all loopholes including such “smallnesses” as no-signaling and the absence of the Aspect-type anomalies be combined? And not simply combined, but with a sufficiently high level of statistical confidence for all of them? Do there exist complimentary calibrations?

In any event, we are still far from an experiment which will not suffer of aforementioned technicalities which can be in short characterized as the problem of the stability of the experimental context in long run quantum experiments.

2.3 The time window problem (coincidence loophole)

I start again with a remark about the terminology. Although the terminology the “coincidence loophole” is standard, I would prefer to speak about the time window problem. The latter rightly emphasized the center role of an experimenter playing with the size of the time window in correlation experiments. Moreover, there are some tinny differences in treating the time-issue in the correlation experiments from the click-coincidence viewpoint and from time window selection viewpoint, see later discussion.

Regarding this problem, first of all I point to the contribution K. Hess and W. Philipp [25], [26] who actively emphasized the role of time in the Bell experiment, as an additional hidden (“forgotten”) variable. Their lovely debates during the Växjö series of conferences, first of all, with R. Gill and J. A. Larsson [28], [29] contributed a lot to clarification of the role of the coincidence/time window loophole. (Here I just emphasize the role of the debate, without to comment the validity of the arguments of the both sides and their mathematical constructions).

\[\text{4}\]Theoretical analysis of this type of problems was performed by the author and Volovich [24]; we came to the conclusion that some loopholes are complementary and they would be never closed jointly. May be we were wrong in our theoretical analysis, but the modern experimental situation seems to be supporting our claim.

\[\text{5}\]Their views matched well with my attempts to use the frequency von Mises theory to model violation of Bell’s inequality [27].
Typically experts point out that the pioneer experiments of Aspect and Weihs suffer of the coincidence loophole. For the latter experiment,

“Coincidences were identified by calculating time differences between Alice’s and Bob’s time tags and comparing these with a time window (typically a few ns).”

To avoid the coincidence loophole, one do not consider differences between time tags, but check the arrival time locally against an absolute trial time window referenced to when the settings choice was made.

Here we can see the difference between treating the time problem from the viewpoints of time coincidence and time window. For the latter, coincidence-identification procedure does not play any role; the crucial point is the presence of selection of time window. This is especially clear from the analysis of Weihs’ data performed by De Raedt et al. [21] who claimed that the selection of the time window can be treated as a post-selection procedure, that precisely by playing with the size of time window one violates Bell’s inequality. This is a very strong and even offensive statement. Therefore it is of the great interest to be able to check its validity for new sets of data.

3 Statistical analysis of Bell’s experiment

3.1 How many standard deviations?

In Bell’s experimenting essential efforts have been put to approach a violation of Bell’s inequality with so many standard deviations as possible. Is such an activity really meaningful? Should an experimenter be proud by approaching such a result? My private opinion (shared with a few my friends-statisticians) is that it is not so much meaningful to try to get more than 3-4 standard deviations. Simply mathematics says us that by increasing the number of trials $N$ we automatically increase the wanted number of $\sigma$. Roughly speaking, this is just a measure of patience of a PhD-student collecting data, how many hours she can spend in the lab.

3.2 Independence of trials, stability of experimental conditions

Of course, as everyone knows from the basics of probability theory, from the central limit theorem, the number of standard deviations can
be used to measure statistical significance of violation of Bell’s inequality only for a sequence of independent and equally distributed trials. If data contains some dependence-patterns (and this often happens in real experiments), then statistical significance cannot be simply reduced to a number of standard deviations.

One has to use other methods to estimate a confidence interval and one of the simplest approaches (although rough enough) is to use Chebyshev inequality as was done, e.g., in [4]; more advanced approaches were discussed in [3]. Thus the presence of dependences in data is not a big problem from the statistical viewpoint. Therefore may be for experimenters it is easier to apply more advanced statistical methods for data analysis, than to struggle with dependence-technicalities in the experiment.

4 Experimental loophole-free violation of a Bell inequality using entangled electron spins separated by 1.3 km

We turn to the recent experimental test announced in [6]. This is really an important step towards the final loophole free experiment, but the statistical significance of the result is questionable.

The biggest weakness is that the rate with which they perform their measurements is extremely low. With approximately one event per hour (!), it takes them about 9 days to record a total of 245 coincidences (“trials of the Bell test”). Even the first experiment with atomic cascades performed in the 1970s (before Aspect’s) had rates of pair productions that were still at least 100 times higher than this one.

Here just one CHSH measurement was made, and estimate the statistical significance of the violation is based on extra assumptions (independence, stability, normal distribution). Are these assumptions justified? Perhaps, but extra assumptions are of course loopholes. The estimation of the standard deviation itself could be underestimated, and when the violation is just two standard deviation away from the absence of violation, this is not a minor issue!

Quite simply, with such a low rate of detection, the experimental conditions are necessarily going to be different from one detection to the other. The authors don’t mention drifts at all in their paper, but there has to be some over the course of nine days and nights. Did

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6And if the rough data were public-available, we would see even more such dependence-patterns.
they correct for those drifts? If not, then the assumption of normal distribution can be questioned, and if they did, well, what good is a violation of Bell inequality in which the experimenter is allowed to re-calibrate/adjust/tune his experimental setup between each detection? Okay, the fact that they perform random measurements is in principle enough to protect them against such argument, but combined with the low number of counts, it remains quite problematic.

**Another issue is no-signaling.** As was emphasized in previous sections, signaling appears with strange regularity in all known experimental tests of Bell’s inequality...

## 5 Beyond quantum

Finally, I present another motivation to create an open access data base. This motivation may be not so important for the quantum community, but it has to be taken into account from the viewpoint of development of science in general. As the organizer of the longest series of conferences on quantum foundations, the Växjö series, I see that the stream of people questioning QM is not vanishing at all, may be nowadays even more people question QM than 30-40 years ago, Why? The answer is known to everybody: because, as Einstein, people are not satisfied that the greatest physical theory is foundationally just an operational formalism for calculation of probabilities. Many of these scientists are not “scientific outsiders”, they are qualified physicists. They are sometimes suspicious that there are no open access data, they suspect that quantum foundational experiments have deeper statistical complexity than it is typically claimed in the papers; they want to compare their theories with the real data.

Of course, experimenters by uploading data to websites can expect that their statistical conclusions may be criticized. And they expectations are correct, they would get more critical publications. But this is precisely as science has to work; this may stimulate experimenters to employ more experts in foundations who will be busy with the critical analysis of the critical publications. We all know that the chance that in future something useful would be found beyond quantum is not so high, cf. [30]. However, it is still nonzero. Recently one of the best experimenters working in quantum foundations told me: “Well, the most funny thing would happen if data from the final loophole free Bell experiment were not violate Bell’s inequality.”

And besides of the natural scientific unsatisfactoriness by the situation in which one simply should “shut up and calculate”, we have not forget about the real black cloud at the quantum horizon: the problem of unification of QM and general relativity. Nowadays many people
start to suspect that this problem is unsolvable without to go beyond, either of QM or general relativity. It seems that by playing with more advanced models of noncommutative mathematics one would not be able to come to the “greatest unification.”

But once again: the conventional quantum community can ignore this “beyond quantum problem”. My main motivation for creation of the open access data-base for the rough experimental data is to provide a possibility for a detailed statistical analysis of this data by independent quantum researchers.

The situation such that after about 20 years after Weihs’ experiment \[ \text{[11]} \] (closed the locality loophole) the data from this experiment is the only openly available for the quantum community (at least it was) is really unacceptable.

**Acknowledgment**

This note was written after the my recent visit to NIST and I would like to thank A. Migdall and S. Polyakov for hospitality. This visit stimulated me to write finally such a note. I was thinking to do this a few times, but was not sure whether it would be meaningful. When I found that even in such a place people suffer of the absence of good rough data on Bell’s test, I decided that something finally has to be done. I also would like to thank A. Migdall and S. Polyakov, Ya. Zhang, S. Glancy, and E. Knill for discussions on technicalities involved in Bell-type experiments and S. Ramelow, B. Wittmann, J. Kofler, and R. Ursin for similar discussions during my visits to IQOQI in 2013, 2014. I thank G. Adenier and I. Basieva for many years of free discussions on the Bell test.

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