Comments on the referee’s reports for manuscript FOOP1369, submitted to Foundations of Physics, entitled
Quantum weak values are not unique
What do they actually measure?

by Stephen Parrott

1 Notation

Most of the comments on the referee’s reports will not be meaningful to someone who has not read the paper, so I may as well use the notation of the paper. For the reader’s convenience, it is summarized below, but most readers will probably want to skip the summary, referring back to it only if needed. Symbols in the summary (such as $A$, $s$, $f$, etc.) will be used in the comments without further definition.

We are given a quantum system $S$ (the “system of interest”) in a given initial state $s$, and an observable $A$ (like the spin of a particle) whose value is to be measured, with the object of determining its average value $\langle s, As \rangle$. If these measurements are performed normally within $S$, each measurement may significantly disturb the initial state $s$ of the system $S$ (by “collapsing the wavefunction” to an eigenstate of $A$). Such measurements are called “strong” measurements.

If we want to measure $A$ without significantly disturbing the initial state $s$ of $S$, it is possible to “weakly couple” $S$ to an auxiliary “meter” system $M$, and measure a “meter observable” as a proxy for the measurement of $A$, in such a way that the average of the meter observable is the same as the average $\langle s, As \rangle$.

For sufficiently weak coupling, it can be arranged that a measurement of the meter observable will not significantly affect the state $s$ of $S$. The tradeoff is that the meter observable will have a large dispersion, i.e., the individual meter measurements are likely to be very inaccurate approximations to $\langle s, As \rangle$, though a large number of meter measurements will average to $\langle s, As \rangle$. Such measurement are called “weak measurements” of $\langle s, As \rangle$.

All this is preliminary to the situation with which the “weak value” literature is typically concerned. Suppose that after making each of “weak measurements” of $A$ in the state $s$ (i.e. measuring the meter observable), we “postselect” the system $A$ to a “final state” $f \in S$. This means that we perform another measurement to answer the question “Is $S$ in state $f$ (“yes” answer) or a state orthogonal to $f$?” If the answer is “yes”, we say that the postselection is “successful” and record the value of the meter observable; if “no”, we discard it. The average of all retained meter measurements is called a “weak value” of $A$. Mathematically, this weak value is the conditional expectation of the meter observable (not of
A) given successful postselection.

*A priori*, one would expect a “weak value” of \( A \) to depend on the details of the way that \( S \) is coupled to the meter system, i.e., on the details of the experimental setup. But surprisingly, nearly all of the “weak value” literature calculates “the” weak value *independently of these details* as what I call the “traditional weak value”:

“The traditional” weak value of \( A \) with initial state \( s \) postselected to final state \( f \) := \( \Re \langle f, As \rangle \langle f, s \rangle \).

They do this by assuming a particular abstract model of the weak measurement process which seems to have no obvious relation to actual laboratory measurements. My paper gives a mathematically rigorous proof that different (in fact, arbitrary) “weak values” can be obtained by different weak measurement setups.

References below to “AAV” refer to the seminal paper [2] of Aharonov, Albert, and Vaidman.

2 Report of Reviewer #1

Report of Reviewer #1:

“The author apparently misinterpret the AAV proposal regarding weak values. He introduces his definition for ‘weak values’ and shows that such values are not unique. He sees ‘logical fallacy’ of the standard approach which supposedly views weak value of \( A \) as conditional expectation of \( A \). However, the celebrated property of the weak value as it usually presented is that it might lie outside the range of the eigenvalues of \( A \), so it clearly cannot be the expectation of \( A \)”

This review is so superficial that it’s hard to identify precisely what the referee objects to. A “weak value”, as defined in the “Notation” section above, *is* a conditional expectation, namely, the expectation of the meter observable conditional on successful postselection.

It does seem to me that nearly all the “weak value” literature implicitly identifies (or confuses) this with the conditional expectation of \( A \) given successful postselection. This would be a natural confusion given that the (unconditional) expectation of the meter observable *does* equal the expectation of \( A \).

As the referee points out (as does my paper), such an identification would be obviously incorrect. Perhaps the referee thinks that the eminent physicists who have published so many papers on “weak values” could never make such a mistake.

The “weak value” literature, beginning with AAV, is typically vaguely written.\(^1\) I have made my best guess as to the motivation for the significance of

\(^1\) Indeed, AAV obtains via vague and dubious mathematics a complex “weak value” \( \langle f, As \rangle / \langle f, s \rangle \) which is in general non-real for a quantity which must manifestly be real. How-
weak values presented by AAV and other literature. This guess is a personal opinion which is clearly identified as such in my paper. Even if the guess should be a misinterpretation, that would not affect the mathematics or substance of the paper.

It is disturbing that the referee doesn’t even comment on the substance of the paper. He (or she, of course) doesn’t even remark on the significance of its conclusion that “weak values are not unique”, nor on whether this conclusion is new.

Reports like this leave an author frustrated. They leave the impression that the reviewer may have been looking for an easy way to avoid the tedious and unpaid professional responsibility of refereeing by seizing on any pretext, however superficial, to reject the paper without having to read it carefully.

3 Report of Reviewer #2

Report of Reviewer $2:

“The main conclusion of this paper is that weak values are not uniquely identified through a weak measurement with post-selection. However, this result is already known. For example, it has been shown in PRA 76, 044103 (2007). The author also concludes that weak values cannot correspond to any ”physical attribute” of the system. This is not proven. In fact, the author does not define what he means by a ”physical attribute”. Thus, this conclusion must be rejected.

I will elaborate briefly on the latter. The author correctly points out that the non-uniqueness of weak values may be attributed to non-uniqueness of the meter state. However, he does not seem to be aware that such problems arise also if the system and meter is described by classical statistical mechanics. In such systems, the weak value reduces to the classical conditional expectation value (see e.g. PRL 93, 120402 (2004))). For example, a ‘standard’ weak measurement of momentum post-selected on position gives the conditional expectation of momentum given the position. I suppose any physicist would agree that this is a ”physical attribute”. Nevertheless, by choosing an unorthodox meter state, a weak measurement in classical statistical mechanics may yield something else than the conditional expectation. But surely, this cannot imply that the classical conditional momentum is not a ”physical attribute”. In conclusion, there is no direct connection between the ambiguity of a weak measurement with post-selection and the possible ‘physical attribute’ of weak values.”

ever, the questionable aspects of this paper should not be allowed to overshadow its genuinely new and interesting ideas. The mere possibility of weak measurements (without postselection) seems surprising.
This report does raise substantial issues which should be addressed.

The referee states that one of the main observations of the paper, that “weak values” are not unique (in the sense of the paper’s definition), previously has been “shown” in PRA 76, 044103 (2007), by Richard Jozsa. I am very grateful for this reference, which I had not previously seen, and will cite it in future versions. However, without in any way deprecating the Jozsa paper, it should be noted that the argument of that paper is not mathematically rigorous and probably cannot easily be made rigorous. It relies on uncontrolled approximations similar to those which Section 5 of my paper (“Obtaining non-traditional ‘weak values’ in the context of AAV”) characterized as mathematically “deeply flawed”.

The Jozsa paper does algebraically motivate why “weak values” (as defined in my paper, not Jozsa’s) should not be expected to be unique. However, it does not discuss the implications of such nonuniqueness. In particular, Jozsa expresses no uneasiness at the non-uniqueness of weak values.

This brings us to the referee’s second (and last) objection, which seems to be to my paper’s statement that

“... weak values are not unique. They therefore cannot correspond to any physical attribute of the system being ‘weakly’ measured, contrary to impressions given by most of the literature on weak measurements.”

(The referee does not specifically cite this statement, but it seems to be to what he objects.)

I see now that this statement is worded in a way that is open to misinterpretation. I did not mean that no weak value can possibly correspond to a physical attribute of a system (which seems to be how the referee interpreted it). I meant that given a weak value (in the sense of my definition) for an observable, there need be no simple and obvious relation between that weak value and what one would normally think of as physical attributes of that observable.

Different measurement procedures can produce different weak values, so any physical meaning attributed to a weak value has to be interpreted within the context of a particular measurement procedure. For example, a weak value associated with a spin observable need have no obvious relation to a spin. To take a more graphic (though fantastic) example, if one obtained a weak value of 200 km/hr for the speed of a runner, that would not necessarily have any relation to the distance which he could run in a given time.

The “weak value” literature gives a very different impression. I know of no paper other than Jozsa’s [1] which even suggests that there could be “weak values” other than the traditional weak value $\Re(\langle f, As \rangle/\langle f, s \rangle)$ (or the same without the real part, if one believes that nonreal weak values are physically meaningful).

To elaborate, consider the title of the seminal AAV paper [2]: “How the result of a measurement of a component of the spin of a spin-1/2 particle can turn out to be 100”. This paper describes a weak measurement protocol which can measure the (average) value of the spin of a particle without (appreciably)
disturbing the system containing the particle. It accomplishes this by measuring the average value of a “meter observable” in a “meter system” which is weakly coupled to the original system containing the spin-1/2 particle.

This average value could be measured by normal (“strong”) measurements within the original system containing the particle, but that would usually disturb the state of that system. If so measured, the average would have to be between -1/2 and 1/2; for definiteness, suppose that average is 0.3. Any weak measurement protocol (without postselection) would produce the same average of 0.3 of the meter observable without (appreciably) disturbing the state of the original system containing the particle.

Now suppose we follow the weak measurement protocol by postselection to a final state \( f \in S \); that is, we calculate the average meter measurement conditional on successful postselection to \( f \). This time, the (conditional) average of the meter observable need not lie between -1/2 and 1/2; it could be 100 as AAV says. But what is the significance of that average of 100? Precisely, what does it have to do with the spin of the particle? AAV does not tell us explicitly, but it leaves the strong impression that there is some clear and direct connection. (Consider its title!)

I would question that. If the measurement procedure corresponds exactly to the mathematics of AAV, then the answer to the question just posed is that the number 100 obtained is the traditional weak value \( \Re(\langle f, A_s \rangle/\langle f, s \rangle) \), where \( A \) is the spin observable. But in what sense can this be taken as a proxy for the spin of a particle?

I agree with the referee that it is conceivable that some particular weak measurement protocol might happen to produce a weak value which would coincide with the average spin of the spin-1/2 particle conditional on successful postselection to \( f \), as measured by strong measurements entirely within the original system containing the particle. I agree that an unrecognized ambiguity in the original language would be consistent with an opposite impression, and I am grateful to the referee for pointing this out. The language will be changed in subsequent versions. However, it should be noted that this unrecognized ambiguity in no way affected the substance of the paper.

In summary, the referee’s report raises two valid objections to the paper: (1) that the Jozsa paper [1] has already given an argument (though not a rigorous proof) suggesting that weak values are not unique, and (2) that the language of a few passages should be reworked to avoid misinterpretation.

There remain substantial parts of the paper to which these objections do not apply, and I was disappointed that the referee ignored these parts. For example, the paper gives a mathematically rigorous proof that arbitrary weak values can be obtained with a meter space of only two dimensions. (The Jozsa paper [1] requires an infinite-dimensional meter space.)\(^2\) And the “Remarks” section makes observations which I think important, and do not appear elsewhere in the

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\(^2\) In case one believes that measuring weak values is likely to yield useful information (a question on which I am agnostic), the possibility of using a meter space of the smallest possible dimension suggests experimental simplifications.
literature, so far as I know. I was disappointed that the referee ignored these remarks.

References
