

Reply to “Comment on Aharonov-Bohm Phase Is Locally Generated Like All Other Quantum Phases”

Introduction—We reply to a Comment on our recent work regarding the locality of the Aharonov-Bohm (AB) phase [1]. We refute the misconceptions appearing in the comment and confirm that our results and conclusions are correct. In the comment [2] there are three criticisms, which we refute below.

The first point is irrelevant to our main claim regarding the open-loop phase. It is claimed that (1a) there is a discrepancy between the classical interaction energy and the QED Hamiltonian. The factor of 2 is due to the fact that the cross terms in $(E_c + E_s)^2$ have a 2 prefactor. We note that the classical interaction energy was derived in [3] and it is not new; we only cited it to give the open-loop phase difference a physical meaning in terms of the fields. This is why this point is irrelevant. It is also claimed that (1b) there is an incorrect -1 sign in the interaction Hamiltonian. We note that the -1 is relevant only for establishing whether the force between the charge and the solenoid is attractive or repulsive; but it cannot affect the value of a relative phase, which is what we propose to measure. This is why the Hamiltonian we use to derive the phase is an effective Hamiltonian, which approximates the charges with qubit operators. It is obtained from a well-known EM field Hamiltonian, which includes the correct prefactors and the correct sign. We also note that point about the -1 sign is part of a controversy about the AB phase, see, e.g., [4] for a summary, but it is not relevant to the main claim of our Letter, which is that the AB “open-loop” phase is detectable.

The key claim of the comment is (2) that the AB open-loop phase is gauge dependent, contradicting not only our result, but also a number of other works that are well known in the literature. This claim is false. The local mechanism for the phase indeed was already pointed out before Aharonov and Bohm in this Letter: Ehrenberg and Siday [5]. But those authors did not provide a full field-theoretic treatment. A simple recent account in terms of de Broglie matter waves is given here (and reaches the same conclusion as us) [6]. More formal accounts that agree with our conclusions can be found in [7] and [8,9].

Hereinafter we confirm that the open-loop relative phase is gauge independent. This can be easily demonstrated by considering that the relative open-loop phase which we propose to measure can be computed in two different gauges G_1 and G_2 , and it must be related as follows:

$$\langle \psi_{G_1} | \hat{O}_{G_1} | \psi_{G_1} \rangle = \langle \psi_{G_2} | \hat{O}_{G_2} | \psi_{G_2} \rangle,$$

where $|\psi_{G_2}\rangle = U|\psi_{G_1}\rangle$ and $\hat{O}_{G_2} = U\hat{O}_{G_1}U^\dagger$, for some unitary gauge transformation U . The author makes the

elementary mistake to compute the relative phase without considering that the transformation must be applied to both the states and the relevant quantum operators. A particularly insightful way to understand this from the quantized field perspective is to observe that the gauge change means that the vector potential changes from $a + a^\dagger$ to $a + a^\dagger + \nabla f$. This modifies both the path-dependent phase $\int_p A(x)dx$ as well as the phase in front of the coherent states in the field. The open-loop phase, therefore, ends up being gauge invariant.

The last erroneous claim in the comment is that (3) “entanglement does not play a role” in the phase generation because it does not exist in the semiclassical model and because its value is vanishingly small throughout the experiment. This conclusion, too, is an elementary mistake: our claim is that entanglement is necessary to explain the phase *with a local account*. This is true even though the value of the entanglement is vanishingly small. Moreover, nonlocal explanations do exist: the traditional semiclassical model is one of them; but this is in no contradiction to our claim.

We finally note that no one has yet performed our suggested experiment. The comment does not offer any prediction about the experiment: would the prediction be “no open-loop phase”? Without this element, the comment appears to be off the mark, as its three main claims are either incorrect or not relevant.

In conclusion, for the reasons listed above, the comment is invalid and does not contradict our prediction about the open loop phase. We hope that the experiment to test our prediction about the open-loop phase will be performed soon. We expect that it will show that the AB phase is a local phase like all other quantum phases.

Acknowledgments—This research was made possible by the generous support of the Gordon and Betty Moore Foundation, the Eutopia Foundation, and the John Templeton Foundation, as part of The Quantum Information Structure of Spacetime (QISS) Project (qiss.fr). The opinions expressed in this publication are those of the authors and do not necessarily reflect the views of the John Templeton Foundation.

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 Received 5 August 2025; accepted 11 August 2025;
published 27 August 2025
DOI: [10.1103/rtwt-g7y7](https://doi.org/10.1103/rtwt-g7y7)

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