This manuscript has been previously reviewed at another journal. This document only contains reviewer comments and rebuttal letters for versions considered at Communications Physics

Reviewers' comments:

Reviewer #1 (Remarks to the Author):

The resubmitted manuscript by Tsurumoto et al. has improved compared to the originally submitted manuscript and it has become more clear how the protocol relates to teleportation. I also find that the scope of the work fits well into communication physics.

I am still not convinced though that it is really necessary to frame this work as teleportation. I appreciate that the authors have toned it a little down and now call it "teleportation-based", but the main topic still seems to be teleportation and I would still agree with the comments of reviewer 1 to the original manuscript that "the interpretation in terms of teleportation is oversold". This interpretation was not necessary for their earlier work (reference 11) which is very similar, so I am not sure why it is featured here so prominently. I would recommend toning it further down with regard to teleportation.

Here are further comments:

1. Page 2, first sentence after "Principle": Again, here the description of teleportation is not quite accurate. The authors state that for teleportation entanglement has to be generated on the receiver's side while it should actually be shared between the sender's side and the receiver's side. Secondly, "measurement of an entanglement" should be a "Bell state measurement" or measurement in the Bell basis. Somehow, the way it is formulated now could lead the reader to believe that there is entanglement between the photon and the electron (which there is not) and also that the teleportation is over a long distance (while in fact it is over the distance from the electron to the nuclear site).

2. Throughout the manuscript the authors refer to "entangled absorption". I would recommend to call this "coherent absorption". I am not sure how an absorption process can be entangled. To me, two particles, or two degrees of freedom can be entangled. This is only semantics, but I think it would make the manuscript clearer.

3. Last sentence of caption for Figure 2. In my opinion this statement is wrong. After the electron is detected in |0>_e, there is no photon and |+1>_e/|-1>_e component any more. How could they then be projected into a bell state? To me, the detection of |0>_e post selects on a certain bell measurement outcome.

4. On page 2 the authors state the "…the number (of 13C) can be arbitrarily increased by isotope enrichment technology". In my opinion the word "arbitrarily" should be omitted. Clearly, this number cannot reach any value and on a more practical term, the number of actually usable 13C will probably be around 10-20.

5. Page 2: "centered NV electron", probably should be "the NV center's electron"

6. Page 2: wording: "measure an entanglement", "generate an entanglement", "an" should be left out.

Reviewer #2 (Remarks to the Author):

The authors have carefully considered my earlier comments on their manuscript -- which are very similar to those of referee 2 -- and they have responded with well-reasoned clarifications on their experiment and its interpretation. They have revised the manuscript accordingly, and I am generally satisfied that it is suitable for publication subject to their consideration of the following comments.

Regarding the key issue of the use of the concepts "teleportation" and "entanglement" in describing this protocol, the authors continue to walk a fine line between making useful analogies and overstating their results. Actually there are many subtleties in this protocol that can lead to confusion. This is why I do not think the authors do the readers any favors by drawing the mathematical analogies too far. The demonstration is interesting and impressive enough in itself, once it is clearly explained.

Specifically, I agree that the with the authors' interpretation that measurement of the spin having relaxed to the ms=0 spin state through the A1 excited state constitutes a Bell measurement of the incoming photon polarization state and the electron spin. The authors have experimentally proved that is the case by showing faithful transfer of the polarization information to a coherent state of the carbon spin. In that sense it is reasonable to call this a "teleportation-based state transfer," as the new title states, even though the incoming photon state is classical rather than quantum mechanical. However I agree with the other referee that it is better to avoid referring to "spin-orbit entangled states" when describing this measurement. There is never any entanglement between the photon polarization and the electron spin. I recommend the authors to remove "spin-orbit entanglement" from the manuscript and simply refer to this as a projective Bell measurement. They can call the A1 state a "spin-orbit-correlated eigenstate."

I also have the following minor technical comments:

* The abstract begins "Quantum teleportation is the key principle for quantum information technology." Since there are many important principles, probably "the" should be replaced by "a."

* On p2 appears the statement "the number [of 13C spins] can be arbitrarily increased by isotope enrichment technology." Probably "arbitrarily" should be removed, since there will be a tradeoff between the number of coupled spins and their usefulness due to increased decoherence that will present a limit.

* On p2, the description of the principle should be rewritten to remove "measure an entanglement between photon polarization and electron spin."

* In figs 1d and 2c, "Entangled absorption" should be replaced with "Bell state measurement." The captions should be similarly edited.

* The authors state a transfer fidelity of 78% that exceeds the classical bound of 67%. They should provide experimental uncertainties on this measurement.

* The authors consider various factors that reduce the transfer fidelity. Do differences in the hyperfine coupling parameters in the ground and excited states play a role?

* Error bars (or representative uncertainty levels) should be added to Fig 2a.

* Error bars should be added to Fig 3c.

* Uncertainties and error bars should be added to Fig 3d.

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Response: We thank the reviewer for positive comments.

I am still not convinced though that it is really necessary to frame this work as teleportation. I appreciate that the authors have toned it a little down and now call it "teleportation-based", but the main topic still seems to be teleportation and I would still agree with the comments of reviewer 1 to the original manuscript that "the interpretation in terms of teleportation is oversold". This interpretation was not necessary for their earlier work (reference 11) which is very similar, so I am not sure why it is featured here so prominently. I would recommend toning it further down with regard to teleportation.

Response: We further toned down with regard to "teleportation" in the revised manuscript by largely modifying the paragraph starting from "Principle" and Fig. 1. We removed "quantum teleportation" throughout the manuscript excepting for titles of the manuscript and figures, and general explanation of quantum teleportation. We replaced "quantum teleportation transfer" with "quantum state transfer" or simply "state transfer" in most cases. We also removed "entanglement" throughout the manuscript excepting for entanglement between electron and carbon.

Here are further comments:

1. Page 2, first sentence after "Principle": Again, here the description of teleportation is not quite accurate. The authors state that for teleportation entanglement has to be generated on the receiver's side while it should actually be shared between the sender's side and the receiver's side.

Response: We largely modified the paragraph starting from "Principle" and removed "sender's side" and “receiver’s side” throughout the manuscript and figures.

Secondly, “measurement of an entanglement” should be a “Bell state measurement” or measurement in the Bell basis. Somehow, the way it is formulated now could lead the reader to believe that there is entanglement between the photon and the electron (which there is not) and also that the teleportation is over a long distance (while in fact it is over the distance from the electron to the nuclear site).

Response: We replaced "measurement of an entanglement" with "measurement in the Bell basis" throughout the text and captions.

2. Throughout the manuscript the authors refer to “entangled absorption”. I would recommend to call this “coherent absorption”. I am not sure how an absorption process can be entangled. To me, two particles, or two degrees of freedom can be entangled. This is only semantics, but I think it would make the manuscript clearer.

Response: We replaced “entangled absorption” with “photon absorption” throughout the text, figures and captions.

3. Last sentence of caption for Figure 2. In my opinion this statement is wrong. After the electron is detected in |0>_e, there is no photon and |+1>_e/|-1>_e component any more. How could they then be projected into a bell state? To me, the detection of |0>_e post selects on a certain bell measurement outcome.

Response: We replaced “projects the photon polarization and electron spin states into one of four Bell states” with “post selects the photon polarization and electron spin states on a certain Bell state”.

4. On page 2 the authors state the “…the number (of 13C) can be arbitrarily increased by isotope enrichment technology”. In my opinion the word “arbitrarily” should be omitted. Clearly, this number cannot reach any value and on a more practical term, the number of actually usable 13C will probably be around 10-20.

Response: We removed “arbitrarily” in the sentence.

5. Page 2: “centered NV electron”, probably should be “the NV center’s electron”

Response: We replaced “centered NV electron” with “the NV center’s electron” in the sentence.

6. Page 2: wording: “measure an entanglement”, “generate an entanglement”, “an” should be left out.

Response: We removed “an” in the sentence.

Reviewer #2 (Remarks to the Author):

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Response: We thank the reviewer for positive comments.

Regarding the key issue of the use of the concepts "teleportation" and "entanglement" in describing this protocol, the authors continue to walk a fine line between making useful analogies and overstating their results. Actually there are many subtleties in this protocol that can lead to confusion. This is why I do not think the authors do the readers any favors by drawing the mathematical analogies too far. The demonstration is interesting and impressive enough in itself, once it is clearly explained.

Response: We further toned down with regard to "teleportation" and "entanglement" to avoid possible reader’s confusion in the revised manuscript as explained below. First of all, we largely modified the paragraph starting from “Principle” and Fig. 1 to remove sender and receiver scheme. We also removed “quantum teleportation” throughout the manuscript excepting for titles of the manuscript and figures, and general explanation of quantum teleportation. We replaced “quantum teleportation transfer” with “quantum state transfer" or simply "state transfer" in most cases. We also removed "entanglement" throughout the manuscript excepting for entanglement between electron and carbon.

Specifically, I agree that the with the authors' interpretation that measurement of the spin having relaxed to the ms=0 spin state through the A1 excited state constitutes a Bell measurement of the incoming photon polarization state and the electron spin. The authors have experimentally proved that is the case by showing faithful transfer of the polarization information to a coherent state of the carbon spin. In that sense it is reasonable to call this a "teleportation-based state transfer," as the new title states, even though the incoming photon state is classical rather than quantum mechanical.

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However I agree with the other referee that it is better to avoid referring to "spin-orbit entangled states" when describing this measurement. There is never any entanglement between the photon polarization and the electron spin. I recommend the authors to remove "spin-orbit entanglement" from the manuscript and simply refer to this as a projective Bell measurement. They can call the A1 state a "spin-orbit-correlated eigenstate."

Response: We replaced "spin-orbit entangled states" with "spin-orbit-correlated eigenstate" and "measurement of an entanglement" with "measurement in the Bell basis" throughout the text and captions.

I also have the following minor technical comments:

* The abstract begins "Quantum teleportation is the key principle for quantum information technology." Since there are many important principles, probably "the" should be replaced by "a."

Response: We replaced "the" with "a" in the sentence.

* On p2 appears the statement "the number [of 13C spins] can be arbitrarily increased by isotope enrichment technology." Probably "arbitrarily" should be removed, since there will be a tradeoff between the number of coupled spins and their usefulness

due to increased decoherence that will present a limit.

Response: We removed “arbitrarily” in the sentence.

* On p2, the description of the principle should be rewritten to remove ″measure an entanglement between photon polarization and electron spin.″

Response: We replaced “measure an entanglement between photon polarization and electron spin” with “measure photon polarization and electron spin in the Bell basis” on p2 and others in the text and captions.

* In figs 1d and 2c, ″Entangled absorption″ should be replaced with ″Bell state measurement.″ The captions should be similarly edited.

Response: We replaced "Entangled absorption" with "Bell state measurement" throughout the text and captions.

* The authors state a transfer fidelity of 78% that exceeds the classical bound of 67%. They should provide experimental uncertainties on this measurement.

Response: We mentioned uncertainty of the transfer fidelity as 78%±2% in the text.

* The authors consider various factors that reduce the transfer fidelity. Do differences in the hyperfine coupling parameters in the ground and excited states play a role?

Response: Our experiments indicate that the hyperfine coupling parameters in the ground and excited states does not play a significant role.

* Error bars (or representative uncertainty levels) should be added to Fig 2a.

Response: We mentioned in the caption that error bars are within 2%, which was defined by shot noise and within the points.

* Error bars should be added to Fig 3c.

Response: We mentioned in the caption that error bars are within 2%, which was defined by shot noise and within the arrows.

* Uncertainties and error bars should be added to Fig 3d.

Response: We mentioned in the caption that error bars are within 2%, which was defined by shot noise and within the bars.