nature portfolio

Peer Review File

Manifestation of the quantum metric in chiral lattice systems

Reviewers' comments:

Reviewer #1 (Remarks to the Author):

The authors look at the mean-chiral displacement for initial state wave packet spread over multiple unit cells. The authors find that the mean-chiral displacement in the long time limit corresponds to the convolution of the Berry connection with the initial wavepacket. What is interesting is that Berry connection is a gauge-dependent quantity, but the authors yet find effects due to the Berry connection. The authors draw analogy from the Aharonov-Bohm effect about the reality of the Berry connection. The authors develop both theoretical framework and experimental realization of this idea, and observed the effect in a quantum walk experiment.

I agree with mathematically what is written, and I have no objection to the experiment that the authors realized. My only concern about the paper is the interpretation of the results in terms of the Berry connection, which is gauge dependent.

I feel not very sure about calling what they observed an effect from the “Berry connection”. As the authors are very well aware of, the Berry connection is gauge dependent. The authors claim that what the authors observed is the Berry connection under a specific gauge. I fully understand by looking at their expression that this is the case, but I do not think Berry connection under a specific gauge has any physical meaning.

In fact, I believe it is possible to give a proper gauge-independent geometrical meaning to what the authors observe. The Berry connection under the gauge authors use is nothing but the square root of the quantum metric. The quantum metric (and its square root) is a fully gauge invariant (and thus gauge independent) quantity. I note that this alone does not give a proper sign; one should assign the sign, I think, according to either the map from momentum space to the complex project space (which is the space obtained by identifying different states of the Hilbert space differing by an overall factor) is orientation preserving or not. (This sign part should be checked more carefully.)

Since we can assign a gauge invariant quantity to the authors’ observable, I find it misleading to say “This is an example of a physical effect of a gauge field in a theory where gauge-invariant fields are not present”.

I do think that the effect the authors find is interesting. It is a new type of geometrical effect that people have not looked at before. Dual presentation of both theory and experiment is also of ingenuity. However, with the current presentation of the paper claiming the observation of the Berry

connection under a specific gauge, I believe, is misleading and I cannot support publication of the paper in its form.

Reviewer #2 (Remarks to the Author):

Please see the attached file.

Reviewer #3 (Remarks to the Author):

In the submitted manuscript “Manifestation of the Berry connection in chiral lattice systems” by Di Colandrea et al, the authors proposed and experimentally verified a method for measuring the Berry connection with a specific gauge in chiral lattices. The relation between the Berry connection and the time-averaged mean chiral displacement (MCD) was derived with numerical demonstrations for 1D and 2D chiral lattices. An experiment was performed in a 1D photonic quantum walk, in which the Berry connection was measured, and the results agree with the theoretical predictions. The topological invariant was extracted from the integral of the Berry connection over the BZ. The experimental data are convincing, and the proposed method benefits from its simplicity compared to existing methods. I think the manuscript can be published in Communications Physics depending on their answers to the following questions.

1. Could the authors specify the meaning of the chiral symmetry operator Γ? What is the basis for this operator, which the authors choose to be σ_z. Is that the pseudospin operator of the two sublattices?

2. What is the correspondence between the integration over the BZ and the integration of light intensity on the Fourier plane?

3. The MCD relates to the Berry connection with an extra oscillating term which averages to zero in time. In the experiment, however, the average has been taken only over two discrete times t=10 and 11. I’m confused with the units the authors used. Does it mean that in Eq. (5) $E=pi/2$ the two cosine terms cancel each other? If that is what makes t=10 and 11 work, will any two times with difference 1 do the same job? I expect a more convincing and physical explanation of the times adopted other than numerical simulations.

4. I don’t quite understand Fig.2(b) and what do the colored lines stand for?

The paper would be more self-contained if the authors elaborate more about the lattice encoding in the main text, although details have been introduced in Ref. [26]. The jump from theory to experiment is too huge to follow.

In “Manifestation of the Berry connection in chiral lattice systems”, the authors demonstrate a direct association between the Berry connection and MCD in processes exhibiting chiral symmetry. They support their theory with both numerical simulations and experimental data. The article is well-written and substantiated, making it of interest to the community and deserving of publication in Communications Physics. However, before I recommend the manuscript for publication, I have the following minor suggestions, comments, and questions (in no particular order of priority), which I hope will help enhance the article’s clarity and appeal.

1. In parts of the text, the unitaries $\hat{\Gamma}$ and $\hat{U}$ are written without the hat (i.e., they appear as Γ and U.
2. The authors introduce the Aharonov-Bohm effect as being induced by the vector potential on the relative phase between the electron wavepackets. I understand the authors phrased it this way to relate the vector potential with the Berry connection. However, the more precise statement is that the relative phase between the electron wavepackets is shifted by an amount that depends on the magnetic flux within a region encircled by them. As the authors acknowledge, the role of the vector potential has sparked an ongoing debate, which can be traced back to Aharonov and Bohm’s seminal article and DeWitt’s alternative perspective a few years after that [Phys. Rev. 125, 2189 (1962)].
3. The observability of the Berry connection through the MCD is intriguing since the latter is gauge-dependent. The mathematical origin of this seems to be related to the treatment of the Aharonov-Bohm effect with a quantized magnetic flux [Phys. Lett. A 160, 493 (1991)]. Can the authors clarify if this is indeed the case? Can this provide new insights into their analysis?
4. Although MCD is proportional to the Berry connection almost everywhere, is there any physical meaning about the region where they diverge? For instance, in Fig. 1, the difference appears in the proximity of the Dirac cones. Can the proximity be characterized in terms of the parameters of the configuration? Does its area decrease with t?
5. In the “Discussion and conclusions”, the authors write “These findings can help address the fundamental question of the physical nature of gauge potentials”. Can the authors elaborate on what they mean by this? Can their answer to question 2 help give a more concrete meaning to this sentence?

We would like to thank all the Reviewers for reading our manuscript and sharing their valuable comments. We have carefully revised the manuscript in accordance with their comments and queries. In particular, prompted by the crucial remarks by Reviewer #1, we deeply investigated the connections between the mean chiral displacement (MCD) and the quantum metric. Importantly, we find that the MCD can be used to fully reconstruct the quantum geometric tensor of chiral-symmetric systems, which is real when chiral symmetry holds. The main result of the article is now captured by equation (9), which reads $4\eta_{ij} \sim \tilde{C}_i \tilde{C}_j$, where η_{ij} is the quantum geometric tensor and $\tilde{C}_{i/j}$ is the time-averaged MCD along the i/j direction. Reflecting our focus shift from the Berry connection to the quantum metric, we also decided to change the paper title to "Manifestation of the quantum metric in chiral lattice systems."

Reviewers' comments:

Reviewer #1 (Remarks to the Author):

The authors look at the mean-chiral displacement for initial state wave packet spread over multiple unit cells. The authors find that the mean-chiral displacement in the long time limit corresponds to the convolution of the Berry connection with the initial wavepacket. What is interesting is that Berry connection is a gauge-dependent quantity, but the authors yet find effects due to the Berry connection. The authors draw analogy from the Aharonov-Bohm effect about the reality of the Berry connection. The authors develop both theoretical framework and experimental realization of this idea, and observed the effect in a quantum walk experiment.

Reply: We thank the Reviewer for their comments and feedback. In the revised version of the manuscript, we have further clarified the relation between the Berry connection and mean chiral displacement. We state that no observable can be affected by the gauge choice. We are grateful to the Reviewer for pointing out the concept of quantum metric, which is indeed the right quantity to describe our results.

I agree with mathematically what is written, and I have no objection to the experiment that the authors realized. My only concern about the paper is the interpretation of the results in terms of the Berry connection, which is gauge dependent.

Reply: The mean chiral displacement is connected to a quantity that measures the geometry of the system's eigenstates: $\gamma := (\hat{n} \times \partial_q \hat{n}) \cdot \hat{v}_\Gamma$. This quantity, which is gauge invariant, is what the mean chiral displacement measures. It has a simple geometric interpretation as the differential solid angle spanned by the system's eigenstates. It can be associated with the Berry connection *in a specific gauge.*
As also pointed out by the Reviewer in the following comments, our results can be more appropriately described in terms of the quantum geometric tensor – a gauge invariant quantity. The manuscript has been modified to highlight the connection with this quantity. In the revised version, the title reads: *Manifestation of the quantum metric in chiral lattice system*. The

following added paragraph clarifies the relationship between the quantum geometric tensor, the Berry connection, and the quantity measured by the MCD:

I feel not very sure about calling what they observed an effect from the "Berry connection". As the authors are very well aware of, the Berry connection is gauge dependent. The authors claim that what the authors observed is the Berry connection under a specific gauge. I fully understand by looking at their expression that this is the case, but I do not think Berry connection under a specific gauge has any physical meaning.

Reply: As also noted by the Reviewer, in the chosen gauge, the Berry connection is proportional to the square root of the quantum metric. As the latter is a gauge-invariant quantity, we agree that it is more appropriate to refer directly to the metric as the quantity being measured.

In fact, I believe it is possible to give a proper gauge-independent geometrical meaning to what the authors observe. The Berry connection under the gauge authors use is nothing but the square root of the quantum metric. The quantum metric (and its square root) is a fully gauge invariant (and thus gauge independent) quantity. I note that this alone does not give a proper sign; one should assign the sign, I think, according to either the map from momentum space to the complex project space (which is the space obtained by identifying different states of the Hilbert space differing by an overall factor) is orientation preserving or not. (This sign part should be checked more carefully.)

Reply: We thank the Reviewer for bringing to our attention the role of the quantum metric in our experiment. Indeed, we verified that the quantity we measure is proportional to the square root of the diagonal component of the quantum geometric tensor. The sign ambiguity is not a problem here, since we demonstrate that it is the square of the MCD that converges to the diagonal elements of the metric (cf. Eq. (8)). Moreover, also the off-diagonal elements of the quantum geometric tensor can be obtained from the product of the time-averaged MCDs along independent directions, namely $4\eta_{ij} \sim \tilde{C}_i\tilde{C}_j$, where again what matters is the relative sign of C_i and C_j.

Since we can assign a gauge invariant quantity to the authors' observable, I find it misleading to say "This is an example of a physical effect of a gauge field in a theory where gauge-invariant fields are not present".

Reply: We agree with this point and thank the Reviewer for helping us clarify our result, by rephrasing it in the proper context. In the revised version, we have modified the original statement. In particular, we highlighted that our results demonstrate that the real part of the quantum geometric tensor also impacts quantum dynamics, in addition to what is already known for its imaginary part (the Berry curvature).

I do think that the effect the authors find is interesting. It is a new type of geometrical effect that people have not looked at before. Dual presentation of both theory and experiment is also of ingenuity. However, with the current presentation of the paper claiming the observation of the Berry connection under a specific gauge, I believe, is misleading and I cannot support publication of the paper in its form.

Reply: We hope that, thanks to the Reviewer's crucial observations, our results have now been enriched with a more clear and more general argument, supported by an appropriate physical discussion.

Reviewer #2 (Remarks to the Author):

In "Manifestation of the Berry connection in chiral lattice systems", the authors demonstrate a direct association between the Berry connection and MCD in processes exhibiting chiral symmetry. They support their theory with both numerical simulations and experimental data. The article is well-written and substantiated, making it of interest to the community and deserving of publication in Communications Physics. However before I recommend the manuscript for publication, I have the following minor suggestions, comments, and questions (in no particular order of priority), which I hope will help enhance the article's clarity and appeal.

Reply: We thank the Reviewer for their positive comments and feedback. We applied the suggested modifications as detailed below.

1. In parts of the text, the unitaries $\hat{\Gamma}$ and $\hat{U}$ are written without the hat (i.e., they appear as Γ and U.

Reply: We thank the Reviewer for spotting this inconsistency. We fixed it in the revised version of the paper.

2. The authors introduce the Aharonov-Bohm effect as being induced by the vector potential on the relative phase between the electron wavepackets. I understand the authors phrased it this way to relate the vector potential with the Berry connection. However, the more precise statement is that the relative phase between the electron wavepackets is shifted by an amount that depends on the magnetic flux within a region encircled by them. As the authors acknowledge, the role of the vector potential has sparked an ongoing debate, which can be traced back to Aharonov and Bohm's seminal article and DeWitt's alternative perspective a few years after that [Phys. Rev. 125, 2189 (1962)].

Reply: We fully agree with the Reviewer's description of the Aharonov-Bohm effect. What we believe is the crucial analogy between the AB effect and our observation of the MCD is that both effects are related to (gauge invariant) geometrical features of the space: the first is an obstruction induced by a localized magnetic flux (or a singularity in the Berry connection in experiments on graphene-like structures – Science 347, 288 (2015)), the second is associated with the non-zero quantum metric in the eigenstate space. Despite being mathematically different, both phenomena can be presented as examples of measurable effects of quantum geometry on particle dynamics. The alternative perspective provided by DeWitt's work has also been added to the references (now Ref. [16]).

3. The observability of the Berry connection through the MCD is intriguing since the latter is gauge-dependent. The mathematical origin of this seems to be related to the treatment of the Aharonov-Bohm effect with a quantized magnetic flux [Phys.Lett. A 160, 493 (1991)]. Can the authors clarify if this is indeed the case? Can this provide new insights into their analysis?

Reply: The MCD measures a gauge-independent quantity, i.e., the scalar $(\hat{n} \times \partial_q \hat{n}) \cdot \hat{v}_\Gamma$. As observed by Reviewer #1, this quantity can be associated with the real part of the quantum geometric tensor, which provides a metric for the eigenstates space. On the other side, the imaginary part of the tensor is the well-known Berry curvature. This insight enforces the statement that the MCD captures the geometry (or, more precisely, the metric) of the system. The manuscript has been modified to highlight this aspect.

4. Although MCD is proportional to the Berry connection almost everywhere, is there any physical meaning about the region where they diverge? For instance, in Fig. 1, the difference appears in the proximity of the Dirac cones. Can the proximity be characterized in terms of the parameters of the configuration? Does its area decrease with t?

Reply: The divergence in the proximity of the Dirac cones is due to the breakdown of the stationary phase approximation. As analytically shown in the Supplementary Material, the sin(tE)^2 factor scales quadratically (for relatively small times) with the distance from the Dirac point while the Berry connection diverges linearly, bringing the MCD to zero in this region. Numerical simulations show that the mismatch area does not decrease with *t*. However, this result strongly depends on the width of the wavepacket. The narrower the wavepacket's width in the BZ, the smaller the mismatch area. Intuitively, the larger deviations can be ascribed to a larger region occupied around the singularity.
To gain some physical insight, one can consider a Gaussian wavepacket initialized on the Dirac point and occupying only one sublattice. The dynamical evolution changes the shape of the wavepacket in a ring-shaped distribution with a zero on the Dirac cone (associated with a point singularity in the wavefunction's phase). This symmetric distribution induces a zero MCD. Conversely, the presence of a singularity in the quantum metric can be detected from the wavefunction characteristic shape. Intriguingly, this process is exactly analogue to what we observe in optical experiments with patterned waveplates with point singularities in the optical axis distribution (e.g., q-plates: Phys. Rev. Lett. **96**, 163905).

5. In the "Discussion and conclusions", the authors write "These findings can help address the fundamental question of the physical nature of gauge potentials". Can the authors elaborate on what they mean by this? Can their answer to question 2 help give a more concrete meaning to this sentence?

Reply: We thank the Reviewer for encouraging us to provide more concrete arguments in the conclusions of our paper. We have now modified the corresponding section, also supported by the observations of Reviewer#1, which now reads: *In analogy with the AB effect, the MCD can be non-zero in regions where the Berry curvature vanishes. In the particular case of 1D systems, where the Berry curvature is not defined, this effect is still observed and proportional to the square root of the quantum metric. Our work offers a new method to measure the quantum metric of unitary processes on lattice systems, from which geometrical and topological features can be extracted.*

Reviewer #3 (Remarks to the Author):

In the submitted manuscript “Manifestation of the Berry connection in chiral lattice systems” by Di Colandrea et al, the authors proposed and experimentally verified a method for measuring the Berry connection with a specific gauge in chiral lattices. The relation between the Berry connection and the time-averaged mean chiral displacement (MCD) was derived with numerical demonstrations for 1D and 2D chiral lattices. An experiment was performed in a 1D photonic quantum walk, in which the Berry connection was measured, and the results agree with the theoretical predictions. The topological invariant was extracted from the integral of the Berry connection over the BZ. The experimental data are convincing, and the proposed method benefits from its simplicity compared to existing methods. I think the manuscript can be published in Communications Physics depending on their answers to the following questions.

Reply: We thank the Reviewer for their positive feedback. We address their comments in the following.

1. Could the authors specify the meaning of the chiral symmetry operator Γ? What is the basis for this operator, which the authors choose to be σ_z. Is that the pseudospin operator of the two sublattices?

Reply: The Reviewer correctly identifies Γ as the pseudospin operator. With a proper rotation of the reference frame, one can always find a basis where $\Gamma=\sigma_z$. In the SSH or graphene model, this basis is given by states distributed in only one of the two sublattices. In the quantum walk implementation, instead, the two sublattices are related to left and right circular polarizations via Equations (8) and (9). In general, we prefer to adopt this representation of \Gamma because it simplifies the description of the eigenstate structure and the required calculations (i.e., in this basis, we can always set n_z=0).

2. What is the correspondence between the integration over the BZ and the integration of light intensity on the Fourier plane?

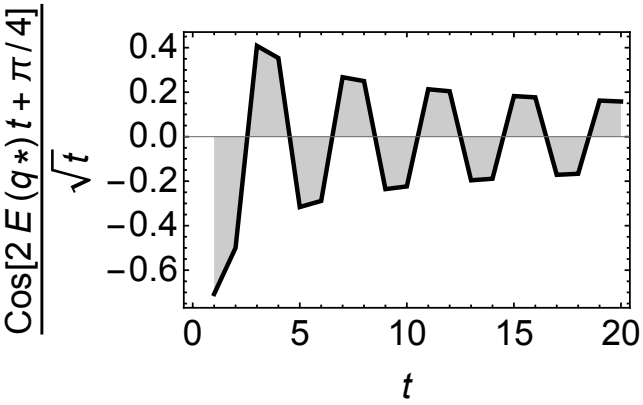
Reply: The integration in the Fourier plane, which corresponds to the model lattice space (the walker space in the experiment), is a direct implementation of the definition of mean chiral displacement given in Eq. (4). The theoretical results reported, for instance, in Eq. (5), are obtained by simply expanding the evolved states \psi(t) in terms of the system eigenstates, from which the integration over the BZ in the reciprocal space. This allows connecting a measurement performed in the real space with the quasi-momentum space. A more detailed mapping between the two spaces is provided in the Supplementary Material.

3. The MCD relates to the Berry connection with an extra oscillating term which averages to zero in time. In the experiment, however, the average has been taken only over two discrete times t=10 and 11. I’m confused with the units the authors used. Does it mean that in Eq. (5)

$E=pi/2$ the two cosine terms cancel each other? If that is what makes t=10 and 11 work, will any two times with difference 1 do the same job? I expect a more convincing and physical explanation of the times adopted other than numerical simulations.

Reply: The Reviewer raises an important point. In general, the energy E is a function of the quasi-momentum coordinate, therefore the cancellation of the subleading term is not exact. This can also be noticed from the small difference in Fig 2e between the dashed black curve (expected MCD) and the red curve (asymptotic value). Thus, in general, a t=1 difference is not sufficient to erase the subleading contributions. However, in the case of our specific quantum walk protocol, two consecutive time steps are sufficient. The mathematical proof is shortly provided in the following (and has been further detailed in the revised Supplementary Material).

If the Gaussian wavepacket is sharply peaked in the BZ, the wavefunction can be approximated with a Dirac delta function. Accordingly, $C(q) = 2A(q)(1 - cos(2E(q)t))$. The oscillating contribution $2A(q)\{cos[2E(q)t]\}$ has a semi-period $T = \pi/(2E(q))$. As the Reviewer observes, in the case E=\pi/2, averaging over two successive time steps does the job. However, if we take E=\pi/4 or 3pi/4, the average over two successive time steps features the most relevant deviation from zero. Notably, in our protocol, these two values of the energy are achieved in the maxima and minima of the energy band. Accordingly, if the wavepacket is broad enough in the BZ, the stationary phase approximation can be used to evaluate the oscillating contribution, which becomes proportional to $cos[2E(q*)t \pm \pi/4]/\sqrt{t}$ (where the sign is + for q*=pi or - for q*=0, with E=\pi/4 and E=3pi/4, respectively). This quantity is plotted in the following:



It is evident that the contributions at t=2n and t=2n+1 almost cancel each other (the difference being proportional to (1/\sqrt(t)-1/\sqrt(t+1)), which decreases for long times). Conversely, an average over three successive steps would have been necessary for very narrow wavepackets. The oscillating contributions in all other points of the BZ are negligible, as they are not stationary.

Our experimental configuration satisfies the settings described above. However, we emphasize and acknowledge that this choice can be justified only thanks to preliminary knowledge of the energy band dispersion.

For more general chiral-symmetric evolutions, the standard approach would be averaging over multiple steps for relatively large t (or directly performing a single measurement at very long times). However, we would also like to stress that a nonzero MCD for finite times is a direct consequence of the quantum metric. Accordingly, if one is only interested in detecting the physical effect of the metric, then only t=10 data would be sufficient, while a temporal average of the MCD is required for a precise reconstruction of the metric itself.

Finally, we note that our ability to calculate explicitly the subleading contributions for our protocol allowed us to verify and compare our experimental results with exact theoretical predictions, thus providing an ideal theoretical benchmark.

4. I don't quite understand Fig.2(b) and what do the colored lines stand for?

Reply: Many thanks for this comment, as we realized that a figure legend was missing. The different curves correspond to the optic-axis distribution of the liquid-crystal plates L_1, L_2, and L_3 employed in the experimental setup. In the revised version, the added legend clarifies the visual correspondence.

The paper would be more self-contained if the authors elaborate more about the lattice encoding in the main text, although details have been introduced in Ref. [26]. The jump from theory to experiment is too huge to follow.

Reply: The mapping between experimental and model parameters has been further expanded in the revised version of the paper. The following paragraph has been added to clarify the encoding: *Each lattice site thus corresponds to an optical mode which can be approximated as a plane wave with a slightly tilted wavefront. In other words, a position x on the lattice maps into a transverse wavevector equal to $k=2\pi x/\Lambda$ -- where Λ is a constant related to the inverse of the lattice spacing.*

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have re-interpreted their results in terms of gauge invariant quantum metric, and I am now fully convinced by and satisfied with their results. I have no further comments on the paper, and I can happily recommend the publication of the paper in Communications Physics.

Reviewer #2 (Remarks to the Author):

I have read the revised version of the manuscript "Manifestation of the quantum metric in chiral lattice systems" by Francesco Di Colandrea, Nazanin Dehghan, Filippo Cardano, Alessio D'Errico, and Ebrahim Karimi. The extensive revisions made to the text, particularly the new interpretation of the results in terms of the quantum metric, have significantly improved the presentation and clarity of the work.

As a minor comment, I suggest that the authors define $\gamma_i(q)$ immediately after Eq. (4) since this is the quantity that explicitly appears in the equation. The current phrasing might lead readers to mistakenly think that the chiral vector $v\Gamma$ has been introduced prior to this point.

Overall, the manuscript is of high quality and will be a valuable contribution to the field. As such, I recommend the article for publication in Communications Physics.

Reviewer #3 (Remarks to the Author):

I have read the revised manuscript and the response letter. I think the authors have satisfactorily answered the questions and made proper revisions. Therefore, I support the publication of the paper in Communications Physics.