Reviewers' comments:

Reviewer #1 (Remarks to the Author):

This paper studies theoretically the Hall effect induced by the canting of the spins in ferromagnet and antiferromagnet from the viewpoint of the vector spin chirality, giving the explicit model of graphene and also realistic calculation on thin film SrRuO3.
As mentioned in “Symmetry analysis”, the vector spin chirality is time-reversal even while the \sigma_xy-\sigma_yx is time-reversal odd, one cannot directly relate these two. Usually, the scalar spin chirality is considered to be related to the Hall effect, and hence the new aspect of this paper is that there is a category of Hall effect, chiral Hall effect, closely related to the vector spin chirality in noncollinear but coplanar spin structures.
I find the proposal novel and interesting, and hence basically support its publication in Communications Physics. I would suggest the following few points to improve the manuscript.

1.The distinction between the vector spin chirality and scalar spin chirality should be stressed more. In the presence of the uniform magnetization M of the conduction electrons, M \dot \chi, which is analogous to the scalar spin chirality, can be nonzero. It has been discussed that anomalous Hall effect is induced in this case although the localized spin moment is coplanar. As far as I understand, in this paper, Hall effect is induced even when M is perpendicular to the vector spin chirality, which is distinct from the conventional picture.
Furthermore, is it possible to separate the two contributions when M \cdot \chi is finite ?

2.When one define the axial vector b = (\sigma_yz, \sigma_zx, \sigma_xy), is there any relation between b and the uniform magnetization M or vector spin chirality \chi ?

3.Original paper for the anomalous Hall effect in antiferromagnet is

Orbital Ferromagnetism and Anomalous Hall Effect in Antiferromagnets on the Distorted fcc Lattice
Ryuichi Shindou and Naoto Nagaosa Phys.Rev.Lett.87,116801(2001)

Reviewer #2 (Remarks to the Author):

Kipp et al. investigate the nature of the Hall effect in magnetic crystals, emerging from the canting of the magnetic moments away from each other. This effect, tagged chiral Hall effect, arises from spin-orbit coupling due to the interplay between the spin chirality introduced by the canting and inversion symmetry breaking. To characterize this effect, the authors first investigate two model hexagonal lattices, develop a detailed symmetry and Berry curvature analysis and apply their theory to the case of SrRuO3 monolayer antiferromagnet. What is remarkable is that in antiferromagnets, the chiral Hall effect can be as large as the crystal Hall effect. This manuscript is well written, the arguments are very well developed and I believe it offers very interesting perspectives in the field of spintronics. I am therefore in favor of its publication in Communications Physics once the authors have considered the comments below:

(0) Based on the model system calculations, it seems that the chiral Hall effect is smaller in ferromagnets than in antiferromagnets. Is it a general rule or is it accidental?

(i) A point that wasn't clear to me is whether the chiral Hall effect only appears in antiferromagnets that already exhibit crystal Hall effect? Or is it possible to envisage antiferromagnets that exhibit chiral Hall effect without crystal Hall effect?

(ii) A followup on the previous question is how the chiral Hall effect translates to non-collinear antiferromagnets?

(iii) I do not understand expansions (4) and (5). Could you please make it more explicit?

(iv) It seems that band degeneracies and mixed Weyl points are crucial to observe large chiral Hall effect. These features are present in many metals and it might therefore be difficult to establish clear guidelines for materials search. Can the authors suggest concrete routes to identify antiferromagnets that might display large chiral Hall effect?

(v) "would allow to reconstruct the angular dependence of the chiral Hall conductivity and determine its nodal points, from which the
information about the details of the crystal symmetry can be deduced." I find this argument not convincing...identifying band structure peculiarities by measuring the Hall effect at a given energy would hardly give much information about nodal points away from Fermi level...could the authors provide a concrete example of application?

(vi) The idea of using the staggered torque is quite interesting, in particular in the context of antiferromagnets where chiral Hall effect seems to be particularly strong. Such a torque was computed in PRB 89, 174430 (2014) where it was explicitly mentioned that it competes with the exchange, leading to current-driven canting. It was also computed in the pioneering work of Nunez et al. PRB 73, 214426 (2006) (and, at that time, wrongly associated to current-driven switching).

Additional comments:

- After the sentence "its relation to the real-space Berry phases of electrons in winding spin textures is reflected in celebrated topological Hall effect of skyrmions", the authors cite Ref. 14. I don't see the relevance of this citation here. I suggest to rather cite papers specifically introducing the topological Hall effect in chiral textures (Neubauer PR 102, 186602 (2009), Nagaosa and Tokura Nature Nanotechnology 2013 etc.).

- There are several typos in the text:
"proprtional"-> "proportional"
"in geometrical terms which relate the geometry of Bloch electronic states in" -> "relates"
"in the vicity of the mixed Weyl point" -> in the vicinity of the mixed Weyl point
"the formal generalization of this logic to the situation" -> "the formal generalization of this rationale to the situation"
"in accordance to our calcualtions" -> "in accordance to our calculations"
"canting the we apply" -> "canting that we apply"
"presented in (Fig. 5),"-> "presented in Fig. 5,"
"One one hand" -> "On the one hand"
"Our analysis shows, that" -> "Our analysis shows that"
"external electic field" -> "external electric field"

"in terms of quanititave and qualitative"-> "in terms of quantitative and qualitative"
"such analysis" -> "such an analysis"

There are several sentences that are too long, improperly constructed or simply not English:

"On the other hand, this promotes the staggered mixed types of monopoles in the electronic structure, which give rise to a large chiral Hall response, as the new type of monopoles which can be “hidden” for the conventional, or, crystal Hall effect." Long sentence which lacks fluidity. Please rephrase.

"(especially in collinear in their ground state systems)" This sentence is very strangely constructed. I suggest "(especially in systems with collinear ground state)"

"The corresponding to the chiral Hall effect variation of the AHE with temperature T is expected to behave qualitatively...+ rest of the sentence" This sentence is not written in proper English.

"promising approach to induce canting between collinear ferromagnetic in the ground state spins". The expression "collinear ferromagnetic in the ground state spins" is not English at all, please reorganize it.

"an applied to a ferromagnet electric field"...another case of sentence written backward. It should read "an electric field applied
to a ferromagnet"...by the way, this sentence is too long and hard to read, I suggest to cut it.

"In the latter cases, the symmetry properties of the anomalous Hall effect can be scrutinized with respect to generalized AFM order parameters, in a way analogous to that employed in this work, various flavors of
spin and structural chirality can be singled out, and their role in mediating various contributions to the AHE can be identified." This sentence is too long and difficult to follow. Please rearrange it.

Reviewer #3 (Remarks to the Author):

The paper discusses the anomalous Hall calculation in response to staggered canting of spins on the two sublattices in the presence of the spin-orbit coupling. The main focus is the Hall conductivity \sigma_{xy}(\theta) dependence on the canting angle \theta. The even \sigma^s_{xy}(\theta) and odd \sigma^a_{xy}(\theta) components of that function are referred to as the crystal Hall and the chiral Hall conductivity. The authors evaluate both components for a model tight-binding system [Eq. (2)] as well as for the SrRuO_3 material. In addition, they evaluate the expansion of the Berry curvature to linear order in \theta [Eq. (9)] as well as evaluate the conductivity at finite frequency [Eq. (10)].

Overall, the paper has an overemphasizing and overselling vibe. Over the past decade, there has been a great number of papers which looked at similar issues. It is not obvious that this manuscript clearly stands among the rest of them and provides such a groundbreaking new perspective as the authors claim. Concretely, for example, the paper arxiv:1902:10650 looked at issues very similar in spirit.

To conclude the paper is elaborate but rather straightforward, and the novelty of obtained results in questionable. It should be published in some form although probably not in the current journal. Scientific Reports or PRB will be best fit.

More specific comments

1. The definition of chirality in the beginning of the “Results” section is misleading. The authors define the chirality as a vector quantity $\chi = (S_A \times S_B)$ using only two vectors S_A and S_B. In contrast, the textbook definition of chirality requires a minimum of three noncollinear vectors a, b and c, so the chirality is defined as a sign of the triple product $C = sign[(a \cdot (b \times c))]$. Perhaps, the authors imply that chirality may be properly defined once the spin-orbit coupling is taken into account. In any case, I recommend that the authors clarify the relation of vector quantity χ to the conventionally-defined chirality C, which is a pseudoscalar. This issue is going to confuse the readers.
2. Please elaborate on the statement “as the symmetry of the model dictates that the antisymmetric in θ part of the conductivity vanishes upon canting in the xy plane”. The reader does not have to do the extra work.
3. Provide the derivation of Eq. (9). Show that it is a gauge-independent quantity.
4. In Fig. 2 the authors showed the Berry curvature dependence on θ. In view of that, what is so “remarkable” about the appearance of mixed Berry curvature in Eq. (9)? After all, quantum metric also seems to appear there.
5. Language is a weak side of the manuscript. I also recommend to tone down the statements about the groundbreaking contributions of this manuscript.
6. Spins s_A and s_B are written in lowercase letters in Eq. (2). Perhaps capitalize them for consistency with the first paragraph of the Results section.
7. Caption of Fig. 1 describes the "$-\theta$" vectors as black. Aren't they red in the figure?

Summary of changes
and
Point-By-Point Response

Jonathan Kipp and co-authors

January 5, 2021

Summary of changes

1. The manuscript's language was revised in response to suggestions from the referees, and several points have been reformulated and expressed more clearly. Most drastic changes have been marked in blue.

2. Analysis of CHE in Mn_2Au has been performed and the corresponding extensive discussion has been added to the manuscript in order to provide answers to the question asked by referee 2. The general discussion of the chiral Hall effect in this part presents one of the quintessential parts of the revised manuscript.

3. Symmetry arguments, requested by the referees, have been added to the supplemental material. The additional symmetry analysis allowed to draw a sharper line between the chiral and topological Hall effects, as requested by referee 1.

4. A factor of two was missing in Eq. (9) and is now included.

Response to Referee 1

> This paper studies theoretically the Hall effect induced by the canting of the spins in ferromagnet and antiferromagnet from the viewpoint of the vector spin chirality, giving the explicit model of graphene and also realistic calculation on thin film SrRuO3. As mentioned in "Symmetry analysis", the vector spin chirality is time-reversal even while the $\sigma_{xy} - \sigma_{yx}$ is time-reversal odd, one cannot directly relate these two. Usually, the scalar spin chirality is considered to be related to the Hall effect, and hence the new aspect of this paper is that there is a category of Hall effect, chiral Hall effect, closely related to the vector spin chirality in noncollinear but coplanar spin structures. I find the proposal novel and interesting, and hence basically support its publication in Communications Physics. I would suggest the following few points to improve the manuscript.

We appreciate the referees positive evaluation of our manuscript and the recommendation to publish it in Communications Physics. The criticism and the constructive feedback is well-received. In the following we provide a response to all the points which were raised by the referee.

> The distinction between the vector spin chirality and scalar spin chirality should be stressed more. In the presence of the uniform magnetization M of the conduction electrons, $M \cdot \chi$, which is analogous to the scalar spin chirality, can be nonzero. It has been discussed that anomalous Hall effect is induced in this case although the localized spin moment is coplanar. As far as I understand, in this paper, Hall effect is induced even when M is perpendicular to the vector spin chirality, which is distinct from the conventional picture. Furthermore, is it possible to separate the two contributions when $M \cdot \chi$ is finite ?

A finite $M \cdot \chi$ indeed arises in certain non-collinear antiferromagnets (for example those which belong to the Mn_3X family). In this case, the symmetry of the coplanar, noncollinear spin state allows for the emergence of a pseudovector out-of-plane: orbital (and spin) magnetization and anomalous Hall effect are possible and $M \cdot \chi$ is finite. Since the net magnetization and the anomalous Hall effect share the same symmetry properties, one can try to identify a coupling of the proposed form using the symmetry expansion as defined in the manuscript. In response to the referee's question, we have outlined this possibility in detail in the Supplemental Material. The results show that true coupling term is more complex than $M \cdot \chi$ or other linear couplings of type $M_i \chi_j$ which one might guess. It is possible however, to interpret the chiral Hall effect in ferromagnets as an anisotropic coupling $\mathbf{n}_+^T \Xi \boldsymbol{\chi}$, where the matrix Ξ depends only on the direction of $\mathbf{n}_+$ and one could argue that this constitutes the proper generalization of the proposed mechanism.

> When one defines the axial vector $b = (\sigma_{yz}, \sigma_{zx}, \sigma_{xy})$, is there any relation between b and the uniform magnetization M or vector spin chirality χ ?

The relation of chirality and magnetization with intrinsic transport properties can be understood in a perturbative picture. At zeroth order, $b \propto M$. Changes in the electronic structure due to finite χ yield terms in b not proportional to M.

> Original paper for the anomalous Hall effect in antiferromagnet is
> Orbital Ferromagnetism and Anomalous Hall Effect in Antiferromagnets on the Distorted fcc Lattice Ryuichi Shindou and Naoto Nagaosa Phys.Rev.Lett.87,116801(2001)

We thank the referee for pointing out that we forgot to cite this reference, which we now cite in the revised version of the manuscript.

Response to Referee 2

> Kipp et al. investigate the nature of the Hall effect in magnetic crystals, emerging from the canting of the magnetic moments away from each other. This effect, tagged chiral Hall effect, arises from spin-orbit coupling due to the interplay between the spin chirality introduced by the canting and inversion symmetry breaking. To characterize this effect, the authors first investigate two model hexagonal lattices, develop a detailed symmetry and Berry curvature analysis and apply their theory to the case of SrRuO3 monolayer antiferromagnet. What is remarkable is that in antiferromagnets, the chiral Hall effect can be as large as the crystal Hall effect. This manuscript is well written, the arguments are very well developed and I believe it offers very interesting perspectives in the field of spintronics. I am therefore in favor of its publication in Communications Physics once the authors have considered the comments below:

We appreciate the referees positive evaluation of our manuscript and the recommendation to publish it in Communications Physics. The criticisms and the constructive feedback are well-received. Responding to sharp questions of the referee motivated us to significantly improve the manuscript. In the following we provide a response to all the points which were raised by the referee.

> Based on the model system calculations, it seems that the chiral Hall effect is smaller in ferromagnets than in antiferromagnets. Is it a general rule or is it accidental?

This is a very good question. Generally, we believe that the chiral Hall effect can be very large both in ferromagnets and antiferromagnets, and, in fact, our model calculations show that it is indeed the case as the largest values shown in Fig. 2(d) and (e) differ by a factor which is less than two. As we have emphasized in the manuscript, the chiral Hall effect in ferromagnets and antiferromagnets has entirely different symmetry properties. For example, in ferromagnets the chiral Hall effect requires inversion-symmetry breaking which lifts many degeneracies among the states. On the other hand, in antiferromagnets, the inversion symmetry breaking is not required for the chiral Hall effect, which keeps the electronic structure heavily degenerate, with the large amount of degenerate states serving as a platform for the emergence of a large chiral signal, see e.g. our analysis for Mn_2Au in the revised version of the manuscript. But again, the exact details which govern the subtle interplay between negative and positive contributions to the chiral Hall effect sensitively depend on specific electronic structure of a given ferro- or antiferromagnetic material, and have to be scrutinized in detail in each case.

> A point that wasn't clear to me is whether the chiral Hall effect only appears in antiferromagnets that already exhibit crystal Hall effect? Or is it possible to envisage antiferromagnets that exhibit chiral Hall effect without crystal Hall effect?

We thank the referee for a very relevant point, which we should have commented on in the initial version. According to our symmetry analysis, modified description of which can be found in the revised version of the manuscript, the inversion symmetry breaking is not required for the chiral Hall effect in antiferromagnets − in contrast to the crystal Hall effect which in antiferromagnets requires broken inversion symmetry. We demonstrate the result of this analysis by performing an explicit calculation of the chiral Hall effect in inversion-symmetric Mn_2Au in the revised version.

> A followup on the previous question is how the chiral Hall effect translates to non-collinear antiferromagnets?

We briefly discuss the possibility of generalization of our approach to the chiral Hall effect to multi-spin systems in the discussion section of the manuscript. While the chiral Hall effect itself, as an effect arising in response to canting of spin pairs, can be straight-forwardly accessed in multi-spin systems in a way that we introduce in our work, a more challenging issue here is, rather, what would be the general approach to naturally unravel the expressions for different contributions to the AHE arising upon canting as given by various flavors of chirality (i.e. generally defined as sensible products of several spin operators). The power of our methodology lies in the fact that we start with a clearly defined reference state (collinear or not) and consider a small deviation from this state, which can be analytically taken into account by involving torque operators. One of the advantages of this approach is that it lends itself to deriving analytical expressions for the Hall effect modifications brought by canting, as reflected for example in our Berry phase theory of the chiral Hall effect. Following this approach, the changes in the AHE can be traced back to products of combinations of order parameters of the system, the symmetrized combinations of which can be used to describe the spin deviations and formulate the AHE changes in terms of e.g. different types of chirality. We plan to formalize this logic to multi-spin systems in our future work.

> I do not understand expansions (4) and (5). Could you please make it more explicit?

The idea behind these equations is to expand the conductivity in powers of the magnetic order parameters $\hat{\mathbf{n}}_+$ and $\hat{\mathbf{n}}_-$, a procedure known in literature as the direction cosine expansion. In a nutshell, this procedure gives rise to a number of tensors which describe the coupling coefficients at different orders in the expansion. Eq. (4) and (5) were missing the symbol ":" for the tensor contraction which has now been added to the manuscript. Further, the Supplemental Material now contains an explicit example for the tight-binding model on the honeycomb lattice considered in the manuscript.

> It seems that band degeneracies and mixed Weyl points are crucial to observe large chiral Hall effect. These features are present in many metals and it might therefore be difficult to establish clear guidelines for materials search. Can the authors suggest concrete routes to identify antiferromagnets that might display large chiral Hall effect?

We thank the referee for this good question, the concise response to which we have partly included into the revised manuscript. Indeed, one of the main points of our work is that it is a special type of band degeneracies, or near band degeneracies – to which we refer as staggered mixed Weyl points – that can give rise to a large chiral Hall effect. The fact that this type of degeneracies does not necessarily coincide with the degeneracies which result in the large "normal", or, crystal Hall effect, has been shown by explicit calculations of $SrRuO_3$ and Mn_2Au. In fact, our calculations of Mn_2Au unambiguously show that the staggered spin-orbit torque (SOT), as given by the staggered mixed Berry curvature, and the chiral Hall effect, are strongly correlated with each other, implying that a large chiral Hall effect most often would manifest in a large staggered SOT and the other way around.

The type of degeneracies which we have considered in the past (see e.g. Refs. [21] and [26] in the revised version) and which give rise to the large non-staggered SOT, have been closely associated with the (local, atomic, often spin-conserving) spin-orbit interaction-mediated evolution of the electronic states as the magnetization direction is coherently changed in the crystal. The corresponding state evolution and corresponding Berry phase properties have been linked to the drastic evolution of the orbital character of the states across the degeneracy point upon tilting the collinear magnetization, see Ref. [21]. In contrast, the type of degeneracies which are giving rise to the chiral Hall effect − as follows from the Berry phase picture we developed − are related to the evolution of the states in response to a staggered variation of the magnetization, as mediated by the non-local spin-mixing part of the spin-orbit interaction which couples the states of opposite spin on two different atomic species. As we see on the example of Mn_2Au and $SrRuO_3$, such type of quasi-degeneracies can naturally occur in complex spin-orbit coupled transition-metal compounds. In addition, by sheer nature of staggered type of mixing points, they can be correlated with the strong response of the orbital magnetization exhibited upon canting (i.e. the chiral orbital magnetization, see Ref. [25] of the revised version). The latter consideration can provide another guiding principle in material design of the chiral Hall effect in canted magnets.

> ”would allow to reconstruct the angular dependence of the chiral Hall conductivity and determine its nodal points, from which the information about the details of the crystal symmetry can be deduced.” I find this argument not convincing...identifying band structure peculiarities by measuring the Hall effect at a given energy would hardly give much information about nodal points away from Fermi level...could the authors provide a concrete example of application?

We apologize that we have not been clear enough here. By nodal points we do not mean the nodal points in the bandstructure, but the specific angles at which the conductivity turns to zero. For example, in this context the nodal point corresponding to the AHE of a two-dimensional ferromagnet would be the azimuthal angle corresponding to the in-plane direction of the magnetization. We reformulate the statement slightly in the revised version of the manuscript.

> The idea of using the staggered torque is quite interesting, in particular in the context of antiferromagnets where chiral Hall effect seems to be particularly strong. Such a torque was computed in PRB 89, 174430 (2014) where it was explicitly mentioned that it competes with the exchange, leading to current-driven canting. It was also computed in the pioneering work of Nunez et al. PRB 73, 214426 (2006) (and, at that time, wrongly associated to current-driven switching).

The referee is absolutely correct. In fact, owing to the limitation on the number of references that the journal allows, we did not provide many references to the spin-orbit torque aspect of our findings, normally referring to the recent Reviews of Modern Physics article on the subject wherever we felt that a reference was needed. But in the revised version we have included the references to latter works among others.

Response to Referee 3

> The paper discusses the anomalous Hall calculation in response to staggered canting of spins on the two sublattices in the presence of the spin-orbit coupling. The main focus is the Hall conductivity $\sigma_{xy}(\theta)$ dependence on the canting angle θ. The even $\sigma^s_{xy}(\theta)$ and odd $\sigma^a_{xy}(\theta)$ components of that function are referred to as the crystal Hall and the chiral Hall conductivity. The authors evaluate both components for a model tight-binding system [Eq. (2)] as well as for the $SrRuO_3$ material. In addition, they evaluate the expansion of the Berry curvature to linear order in θ [Eq. (9)] as well as evaluate the conductivity at finite frequency [Eq. (10)]. Overall, the paper has an overemphasizing and overselling vibe. Over the past decade, there has been a great number of papers which looked at similar issues. It is not obvious that this manuscript clearly stands among the rest of them and provides such a groundbreaking new perspective as the authors claim. Concretely, for example, the paper arxiv:1902:10650 looked at issues very similar in spirit. To conclude the paper is elaborate but rather straightforward, and the novelty of obtained results in questionable. It should be published in some form although probably not in the current journal. Scientific Reports or PRB will be best fit.

While the reviewer clearly displays a very strong and emotional opinion about our manuscript, he/she does not provide the necessary evidence to substantiate the main argument. Taking a closer look at arxiv:1902:10650 (which we now cite in the revised version of our manuscript), we can certainly acknowledge interesting work which deals with canting in a very specific tight-binding antiferromagentic model. In fact, we are aware and completely agree that there is a number of works on the anomalous Hall effect in canted antiferromagnets available (some of which we mention in our manuscript), however, they all lack the perspective and generality offered by our approach. Specifically, our analysis is formulated in the language of symmetry, which unifies previous works on canted antiferromagnets but also covers an entirely novel aspect of the anomalous Hall effect in canted ferromagnets, thereby leading to a much more general viewpoint of the chiral Hall effect, emerging as a natural twin of the conventional (or, crystal) Hall effect. Further, we develop a Berry phase theory of the chiral Hall effect − which is expressed in terms of geometrical quantities not at all considered in the theories of the conventional anomalous Hall effect − which marks the chiral Hall effect as a playground for novel types of Berry phases. The novel aspects of the Hall effect in canted systems that we uncover are fully supported by model and ab-initio calculations. We therefore do not agree with the referee's opinion that the matters presented in our work have been dealt with before.

> The definition of chirality in the beginning of the "Results" section is misleading. The authors define the chirality as a vector quantity $\chi = (S_A \times S_B)$ using only two vectors S_A and S_B. In contrast, the textbook definition of chirality requires a minimum of three non-collinear vectors a, b and c, so the chirality is defined as a sign of the triple product $C = sign[(a \cdot (b \times c))]$. Perhaps, the authors imply that chirality may be properly defined once the spin-orbit coupling is taken into account. In any case, I recommend that the authors clarify the relation of vector quantity χ to the conventionally-defined chirality C, which is a pseudoscalar. This issue is going to confuse the readers.

By definition, a physical object can be considered chiral, if it can be distinguished from its mirror image. The concept as such is therefore much wider as it is suggested by the referee and the triple product is by no means the unique way of constructing a chiral quantity. The chiral object relevant to our discussion is prominently defined in Eq. (6). It has been intensively exploited in characterization of chiral magnetism since the 1950s.

> Please elaborate on the statement "as the symmetry of the model dictates that the antisymmetric in θ part of the conductivity vanishes upon canting in the xy plane". The reader does not have to do the extra work.

We now provide an extended symmetry analysis of the AHE in our model in the Supplementary Material.

> Provide the derivation of Eq. (9). Show that it is a gauge-independent quantity.

The derivation of Eq. (9) with the demonstration of gauge invariance is now given in the Supplementary Material.

> In Fig. 2 the authors showed the Berry curvature dependence on θ. In view of that, what is so "remarkable" about the appearance of mixed Berry curvature in Eq. (9)? After all, quantum metric also seems to appear there.

The anomalous Hall effect has traditionally been associated with Berry curvature in reciprocal k-space resulting in the anomalous velocity of Bloch electrons in response to an applied electric field. On one hand the study of the corresponding k-space Berry curvature has been one of the central pillars of topological condensed matter physics for the past 15 years. On the other hand, the study of the Berry curvature of electrons associated with the magnetization degree of freedom has been studied in parallel with respect to such effects as magnetic anisotropy (e.g. Nature Materials 6, 648), spin dynamics (e.g. PRL 83, 207) Gilbert damping and exchange interactions (e.g. PRB 95, 184428), as well as the effect of spin-orbit torque (e.g. PRB 90, 174423). In this light, we believe that our finding that the chiral Hall effect directly relates not only to the k-space Berry curvature but also to the magnetization-related geometry of electronic states, is remarkable, as it unites two – up to now formally distinct – areas of magnetic phenomena where Berry phase physics was shown to play an important role. In particular, in the revised version we show explicit calculations of Mn_2Au, where the k-space Berry curvature in the collinear state is suppressed by symmetry. On one hand, the close correlation in the electronic structure between the mixed Berry curvature in magnetization space and the chiral Hall conductivity clearly emerges even given a non-vanishing correction by the quantum metric term. On the other hand, the relation of the chiral Hall effect to the quantum metric of states in mixed space is also intriguing, and we assume that the correlation between the quantum metric and the chiral Hall effect can be promoted in some situations – an aspect which deserves further studies. Overall our main point is that the chiral Hall effect emerges as the marker of more complex geometric properties of Bloch states in magnetic solids well beyond the explored in depth k-space Berry curvature. We believe that this is a very important finding.

> Language is a weak side of the manuscript. I also recommend to tone down the statements about the groundbreaking contributions of this manuscript.

We have made an honest effort to comply with this request of the referee. The language has been improved also in response to referee 2.

> Spins s_A and s_B are written in lowercase letters in Eq. (2). Perhaps capitalize them for consistency with the first paragraph of the Results section.

The notation has been unified.

> Caption of Fig. 1 describes the "$-\theta$" vectors as black. Aren't they red in the figure?

The caption has been corrected.

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

I have looked at the responses and revision by the authors, and find all my comments are well responded. I now do recommend its publication.

Reviewer #2 (Remarks to the Author):

Kipp et al. have made remarkable efforts to answer all my questions and comments satisfactorily. The paper has been substantially extended and constitutes a rich study of the chiral Hall effect. I therefore recommend the manuscript for publication.

As a last remark though, I would advise the authors to cool down the claims of novelty. Is the chiral Hall effect really a novel effect, or simply another (intriguing) aspect of anomalous transport that was overlooked until now? This is a very good paper and the ideas developed by the authors will disseminate in the scientific community without the need for "overemphasizing" the results.

Reviewer #3 (Remarks to the Author):

In the revised manuscript, the authors have included more information in the supplementary materials. The language has been improved as well. I am now convinced by the discovery made in this work. I recommend the manuscript, as is, to Communications Physics