Dear Editor,

Thank you very much for arranging to review our manuscript. I have addressed all the queries raised by the referees. Detailed response to the queries of the referees have been provided below (Referees' comments are highlighted in blue). In view of these changes, I believe that the manuscript will now be judged suitable for publication.

Sincerely,
Ipsita Mandal

I. REVIEWER 1

1. The paper is clearly written, and scientifically sound. I believe this work is worth publishing in Scientific Reports.

   I thank the referee for the positive comments and recommending publication.

2. I'd like to mention that there are some typos in the manuscript. For example, in the caption of Fig. 1, "$s = 1$(light blue)" should be "$s = -1$(light blue)", and for Fig. 1(b) "k_xk_y-plane" should be "k_xk_z-plane". The authors should carefully examine the details to avoid such mistakes.

   I apologise for the typos and thank the referee for pointing these out. These originated due to changing the figures multiples times to my satisfaction. I have now corrected them. Regarding checking for typos in detail, I usually do it during proof-reading investing a lot of time, which avoids time-investment in doing it multiple times (e.g., multiple revisions of the manuscript) at various stages.

II. REVIEWER 2

1. I'm surprised that the OMM does not depend on the band index. Is 'e' supposed to have different signs for the s=1 and s=-1 band? This matters, for instance, for the chiral vortical effect, which can be shown to be a sum of Berry curvature and orbital magnetization contributions (Shitade-Mameda-Hayata, PRB 2020; Nanda-Hosur, PRB 2023). For a Weyl fermion, it should only depend on the chemical potential squared and Weyl node chirality. However, if only the BC depends on the band index and not the OMM, the CVE would be the sum (difference) of the two contributions for the upper (lower) band. I wonder if the absence of any significant effect of the OMM in Figs 2-4 is because of this issue.

   There is no surprise element here because it arises from the formula and derivation set forth in the seminal papers [1–5]— e.g., Eq. (3.6) of Ref. [2]. It is a well-known fact in the literature and has been used in numerous papers before me as well as in many of my earlier papers. e is of course the magnitude electron charge everywhere in the manuscript. From the parent formula, one can derive the actual final forms for a given semimettalic Hamiltonian. For the Weyl case, it evaluates to the forms shown in our manuscript. Some older papers which contain and use the final expressions are: Eq. (72) of Ref. [6], Eq. (5b) of Ref. [7], Eq. (19) of Ref. [8], paragraph below Eq. (13) of Ref. [9], Eq. (15) of Ref. [10], and many many more. If the referee is not convinced, I request him/her to derive the final formula for Weyl case — it is simple and involves only a few lines of calculations.

   Interestingly, for two-band models, which are essentially of the generic form given by $\mathbf{d}\cdot\boldsymbol{\sigma}$, the relation of $m_i^s(\mathbf{k}) = e\,\varepsilon^s(\mathbf{k})\,\Omega_i^s(\mathbf{k})$ is satisfied (see the dicussion below Eq. (3.12) of Ref. [11]). This clearly shows that the s-dependence cancels out in the OMM vector. This was already mentioned in our manuscript:
   "For our two-band system, the expressions above simplify to [12]

$$\Omega_i^s(\boldsymbol{k}) = \frac{(-1)^s\,\epsilon_{ijl}}{4\,|\boldsymbol{k}|^3}\,\boldsymbol{k}\cdot\left[\partial_{k_j}\boldsymbol{k}\times\partial_{k_l}\boldsymbol{k}\right] \text{ and } \boldsymbol{m}^s(\boldsymbol{k}) = e\,(s\,v_0\,k)\,\boldsymbol{\Omega}\,. \tag{1}$$

   "
   Of course OMM's sign will matter in all response characteristics.

2. If the OMM sign is correct, what is the physical reason for the observed OMM-independence (almost) of the results? In general, I would like to see more physical insight into the results. E.g. what determines the coefficients of the linear-in-B and quadratic-in-B terms?

   The OMM's sign is completely correct. The explanation for this point was already provided in the original version of the manuscript, which I repeat here for the convenience of the referee:
   "One might argue that the possibility of having $\sigma_{zz}(B_z,\chi) = \sigma_{zz}(-B_z,-\chi)$ could make a linear-in-B term possible. But the issue is that, the BC-contributions and the band-velocity contributions from the OMM appear as factors proportional to $\chi\,\boldsymbol{k}\cdot\boldsymbol{B}/k^n = \chi\,k_z\,B_z/k^n$, with a surviving k_z factor from $\boldsymbol{v}^s$ (i.e., the zeroth order $\boldsymbol{B}$-independent band-velocity) giving an odd-in-k_z integral evaluating to zero. In other words, the answers there exclusively contain

terms like $\chi^2\,B^2 = B^2$, thus preventing a linear term. This is what was observed in Ref. [13], where we considered pseudospin-1 nodes with quadratic-in-momentum corrections."
Since it seems that the referee did not understand the essence of the above statements, I have added some elaborate explanations as follows:
"For the benefit of the reader, let us elaborate on the precise origins of the linear-in-B and quadratic-in-B terms. From Eqs. (33), (27), and (29), we find that

$$\begin{aligned}&-V\,\sigma^s_{zz}/e^2\\&=\int\frac{d^3\boldsymbol{k}}{(2\,\pi)^3}\,\tau_s(\mu,\theta)\,[w^z_s(\boldsymbol{k})+e\,B\,\{\boldsymbol{\Omega}^s(\boldsymbol{k})\cdot\boldsymbol{w}_s(\boldsymbol{k})\}]\left[\lambda_s-\mathcal{D}_s(\boldsymbol{k})\left\{w^z_s(\boldsymbol{k})+e\,B\Big(\boldsymbol{\Omega}^s(\boldsymbol{k})\cdot\boldsymbol{w}_s(\boldsymbol{k})\Big)\right\}+\frac{a_s\,k_z}{k}\right]\delta\big(\xi_s(\boldsymbol{k})-\mu\big)\,.\end{aligned}\tag{2}$$

For gaining the requisite physical insight into scaling of B in the results, it suffices to assume τ to be $\boldsymbol{k}$-independent, and expand $\mathcal{D}_s(\boldsymbol{k})$ as well as $\delta\big(\xi_s(\boldsymbol{k})-\mu\big)$ up to $\mathcal{O}(B^2)$. Now, we have to remember that $\xi_s(\boldsymbol{k}) = c\,k^2 + s\,v_0\,k + \frac{e\,v_0\,k_z\,B}{2\,k^2} + \frac{e\,g\,\mu_B\,s\,B}{2}$ and $\boldsymbol{w}_s(\boldsymbol{k}) = \frac{2\,c\,k+s\,v_0}{k}\,\{k_x,\,k_y,\,k_z\} - \frac{e\,v_0\,B}{k_F^4}\left\{k_z\,k_z,\,k_y\,k_z,\,k_z^2-\frac{k_F^2}{2}\right\}$. The only term in the integrand which can give rise to a nonzero linear-in-B term originates from the part of the integrand proportional to $[\boldsymbol{v}^s(\boldsymbol{k})]^2\,\boldsymbol{B}\cdot\boldsymbol{S}_s\,\delta'\big(\tilde{\varepsilon}_s(\boldsymbol{k})-\mu\big)$, because all terms odd in $\boldsymbol{k}$ vanishes on integration over $\boldsymbol{k}$. The remaining even-in-$\boldsymbol{k}$ parts of the integrand comprise B^2. This is how the Onsager-Casimir reciprocity relation is satisfied as well, as outlined in the preceding paragraph — the presence of a nonzero SMM is solely responsible for giving rise to a term $\propto B$."

3. Is this effect observable in any real material?

   The explanation for this point was already provided in the original version of the manuscript, which I repeat here for the convenience of the referee:
   "Experimentally, the smoking-gun properties of KWNs have been observed in materials like β-Ag_2Se [14], which crystallises in a nonsymmorphic chiral form. In this paper, we will consider isotropic KWNs, which arises at a node harbouring the high-symmetry cubic point group, $\{\mathrm{T},\mathrm{O}\}$. The candidate materials include $K_2Sn_2O_3$ [15], RhSi [16], CoSi [16], AlPt [17], and PtGa [18]."

4. There are many scenarios where linear and quadratic B-dependences appear in magnetoconductivity, for instance, if a material has other Fermi pockets or band crossings. How would one isolate the contribution of the KWN discussed here?

   The explanation for this point was already provided in the original version of the manuscript, which I repeat here for the convenience of the referee:
   "For the spinless WSMs, arising in achiral crystals (e.g., TaAs family [19]), such conjugate partners are typically (almost) degenerate in energy, due to the presence of mirror or other roto-inversion symmetries. $\cdots$ On the contrary, the oppositely-charged chiral nodes in chiral crystals need not be degenerate in energy, because the conjugate nodes are not related by crystal symmetries. In fact, they are observed to have discernible separations in energy and momenta, with an isolated KWN located at an intrinsic chemical potential [15, 20]. As such, internode-scattering induced charge pumping becomes unimportant, while enhancing other phenomena like quantised circular photogalvanic effect [15, 21–26] and circular dichroism [27, 28]."
   and
   "As pointed out earlier, a single node will be relevant here, when we tune the external chemical potential to lie near the intrinsic energy of the concerned nodal point, as the chirally-conjugate node is well-separated in energy and momentum (see, for example, Refs. [20, 29]). In other words, the energy-separation between the $\chi = \pm$ members of the pair is much larger than the temperature-scale of the experiments."

   The bottomline is that it is not a viable scenario for chiral crystals to have Fermi pockets or band crossings at the same global energy. As such, when the chemical potential is tuned to lie near the band-crossing point of the KWN, there will be no interference while probing response originating fro low-energy-processes. In particular, Ref. [20] (see also the bandstructure therein for more clarity) clearly states that "Below E = 0.7 eV, only the Weyl bands enclosing the Kramers Weyl point at H are involved ...".

5. Many calculation details can be moved to Appendices, especially those that recap previous work. This includes large parts of Sec II and Sec III A-C. Sec IV also has some lengthy expressions which may be better off in an Appendix, with only a simplified result or numerical plot in the main paper.

   This is a theoretical physics paper and some equations will be there. I have kept the essential minimal Boltzmann equations in the main text which are *required* to set the notations of the final expressions to be solved. Without them, the readability and continuity of the manuscript will be completely lost. Furthermore, in Scientific Reports, appendices are not allowed — only supplementary information pdf is allowed. In order not to sacrifice the readability of the manuscript, I disagree with the observation that "some lengthy expressions which may be better off in an Appendix, with only a simplified result or numerical plot in the main paper." In other words, I cannot comply with this suggestion without sacrificing the readability.

III. LIST OF CHANGES

The additions are marked in the marked-up version of the revised manuscript in the color red.

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