Reply to Referee reports and list of changes

Let us thank the two referees for the throughout analysis of our lectures and their comments and suggestions. Hereafter, please find our replies and corresponding changes.

Report I

1) On page 5 in the 4th paragraph the authors write that the low energy response is "geometric", I wonder if this is the right word. After all also the dissipative thermal conductivity can be seen as a "geometric" response following Luttinger. The distinguishing feature is that it is robust (insensitive to microscopic details). So probably "topological" is a better word?

We added a remark to better relate "geometric" and "topological":

"Indeed, certain *global* quantities, like conductances, are topological and can only take discrete values."

2) I find the discussion of the gauge field on page 9 confusing. Is it really true that after fixing $A_0 = 0$ one can assume A_x to be constant in space? As the authors emphasize the gauge fields are external and do not have to obey any equations of motion (except the topological Bianchi identity, which is trivial in D=2). Why does one have to chose a gauge at all if only the fermions are quantized here?

Yes, we can choose a gauge in which A_x is constant in space. The explanation is added in the following footnote before (2.9):

"[4]This finite gauge transformation is $U(x) = \exp(-i \int^x dx' A_x(x')$"

Note that for A_x constant, the spectrum (2.9) is easily derived.

3) In the footnote 3 the authors state that anti-periodic boundary conditions form fermions are standard. I think one could chose either one if one works strictly on a circle. Anti-periodic boundary conditions seem most natural if the circle is the boundary of a disc.

Yes, we extended the footnote for explaining this point

"[3]Antiperiodic (Neveu-Schwartz) boundary conditions are standard for fermions, since the minus sign follows by bringing the fermion around the circle. Periodic (Ramond) conditions can also be chosen, they introduce additional features to be discussed in Sect. 9.3."

4) Something went wrong in the normalization in (2.29)? It seems to contradict (2.28).

5) In (3.35) the eigenvalue of the charge operator should be "n".

6) Chapter 6: I think there are some misprints: -) below equation (6.13) the axial vector (gauge) field should be $(b_0, \vec{b})$. The normalization of (6.14) should be $1/2\pi^2$ (also (6.16) and (6.17))

All remarks 4,5,6 are correct: formulas have been fixed.

7) Section 6.4 on the Chiral Magnetic Effect: I think the discussion suffers from the problem that the authors only write the contribution due to the Bardeen-Zumino term. But also the covariant current contributes (that is really the non-trivial term). What happens is that the two contributions exactly cancel if $A_{5,0} = \mu_5$. In fact there is a theorem (due to Bloch apparently) stating that the electric (DC) current must vanish in equilibrium. (a modern field theoretic formulation is Phys.Rev.D 92 (2015) 8, 085011.) This cancellation is discussed in ref. [66]. Since this led to some confusion in the early discussions of the effect in Weyl semimetals it would be good to mention this.

We have slightly rewritten this section, emphasizing that we work in a frame where the covariant current is zero before the fields are applied. We have also added a reference to Bloch's result and the paper by Yamamoto. We thank the referee for remarking this, an forcing us to sharpen the logic.

8) Probably the authors can expand a bit what the dots mean in equation (7.13). I understand that this is an introductory lecture on Index theorems but still a few words on what the dots are should be added.

We added the following phrase at the end of the paragraph after (7.13)

"The dots in (7.13) are other pseudoscalar quantities that are higher-order polynomials of the curvature and its covariant derivatives."

9) In the discussion in the first paragraph on page 59 I would suggest to add a citation to the paper by Kimura Prog.Theor.Phys. 42 (1969) 1191-1205 which appeared already in 1969 and computes the gravitational contribution to the axial anomaly. This is remarkable and the paper has much less citations than it deserves.

Thank you for pointing out this reference. We have added it in the text after the result (7.15), before quoting the path-integral result by Fujikawa.

10) In the discussion of the 4D topological insulator the axial anomaly in Euclidean space is used in (9.3) and (9.4). It is stated that the partition function is a phase and therefore we have the 2π periodicity in the theta-angle. But then the authors discuss Time Reversal symmetry. It would be good if the authors could clarify what is meant here by time reversal when one is implicitly working in Euclidean signature.

Thank you for noticing this potentially confusing point. We added the following footnote at the end of the paragraph after (9.4):

"Note that the theta term (9.4) is purely imaginary both in the Euclidean formulation of Section 5.3 and in the Minkowskian version used for physical application, being subjected to TR invariance. Furthermore, this symmetry can also be directly formulated in Euclidean space, as described in Sect. 10.2"

Report II

1a - At times, the authors could be more concrete when referring to methods and results presented later in this manuscript. For example, the divergent term in Eq. (2.16) is simply subtracted without any explanation. I suppose this becomes more obvious when regularizing the integral, which is done much later in the manuscript.

We have added a footnote after (2.16) referring to section 3.3 for the complete discussion of the subtraction procedure (normal ordering):

"[6] A detailed analysis of the subtraction procedure, unambiguously determining the finite terms, will be presented in section 3.3."

1b - Similarly, on page 25, the authors promise to return to the anomaly inflow "at several occasions in the context of the so called bulk-boundary correspondence in topological states of matter."

We have added a footnote after this phrase:

"[16] See section 6.2 and appendix D.4, in particular."

2- In Sec. 2.2, the authors write that A5 is a "non-dynamical 'emergent gauge potential'," which does not need to be conserved. Maybe it would be good to specify at this point that it can often (generally?) be directly measured, e.g., as the Hall conductance in Weyl semimetals, i.e., its value has physical consequences, and it is thus fundamentally different from a gauge potential.

Section 2.2 is a simple first encounter with anomalies, so many issues are necessarily left unexplained, to be dealt with in the rest of the lectures. Nonetheless, we expanded the text of the following phrase:

"We will return to the physical interpretation of A_5 in Sect. 6 and App. D discusses aspects of the mixed anomalies where both vector and axial (background) fields are present."

changed to:

"We shall see in Sec. 6 that A_5 plays an important role in the physics of Weyl semimetals and the chiral magnetic effect, while in App. D we discuss aspects of the mixed anomalies where both vector and axial (background) fields are present."

3- In Eq. (6.10), the role of the density ρ is not clear to me: It is a first-quantized Hamiltonian, so the density should not appear here.

Correct: we removed ρ.

4- In Eq. (6.12) and above, it would make sense to switch to a vector notation of the Berry curvature. In the 2D example, the integral over the field strength is of course identical to the surface integral over the vector field, however, is it only the vector field integration that generalizes to the 3D case.

We introduced the vector notation in (6.12)

5- The authors emphasize that this paper "is not a review paper, but a set of lecture notes covering a wide range of topics" and that they thus "have not attempted to provide a comprehensive bibliography," which is fine. Nevertheless, I have two suggestions for additional papers the authors might want to cite: [A] and Ref. [70] by Pikulin and et al. appeared simultaneously and discuss similar ideas. [B] covers most aspects of Sec. 9.2.1 and could be maybe introduced as an additional reference for interested readers.

[A] Adolfo G. Grushin, Jörn W.F. Venderbos, Ashvin Vishwanath, and Roni Ilan Phys. Rev. X 6, 041046 (doi:10.1103/PhysRevX.6.041046)

[B] Pavan Hosur, Shinsei Ryu, and Ashvin Vishwanath Phys. Rev. B 81, 045120 (doi:10.1103/PhysRevB.81.045120)

Thank you for quoting these works. We added the references: [A] is put first in a list of WSM references [67-70]; [B] is added at the end of the first paragraph of Sect 9.2