I. SUMMARY OF CHANGES

In the second revision, we have made the following changes:

1. We have added discussions after Eq. (54) to show that steady-state solutions are also possible. But in addition to steady-state solutions, there are also non-steady-state solutions that are of interest to us. Hopefully this will convince the referee 1 that our theory does not miss the steady-state solutions.
2. Around newly added equations (65)-(68) we have added estimates based on the measurements done on graphene samples to estimate that for tilted Dirac cone samples that are $\sim 1\text{cm}$ wide, currents of $\sim 4\text{nA}$ are attainable.
3. We have corrected typos here and there.

For convenience of referees we have highlighted the latest changes by red color.

II. RESPONSE TO REVIEWER 3

Referee Comment 1:
1) Normally in hydrodynamics if you turn on an "external temperature gradient" as the authors did in Section 3.1.1, you could "cancel" the effect by having the actual temperature change so as to cancel the effect out; similarly, the electric field could in principle be cancelled off by a x-dependent chemical potential. For example taking the latter case (in the absence of tilt) I'd have

$$\partial_\mu T^{\mu i} = nE^i = \partial_i P = n\partial_i \mu \tag{1}$$

so taking $\mu = E^i x_i$ I could find consistency. It seems this cannot happen in Eq. (27) because there would be a $\zeta^j \partial_j \mu$ term on the left hand side of the equation but there is no similar term on the right hand side. This makes me worry that the calculation in Section 3 might in some way be inconsistent. It seems like a plausible argument on physical grounds that you wouldn't want the external electromagnetic fields to see the metric tilt...but that may have then led to this later issue, and I worry this is a serious enough point that it might mean this plausible assumption was wrong. I am not 100 sure if this is a mistake that needs to be fixed or just a funny feature of this system; however, at a minimum some discussion of this point should be had, and I do honestly lean towards thinking that the calculation is just not correct in some way.

Response:
In a finite system with boundary, at the equilibrium, an electric field induce a x-dependent chemical potential which cancel the effect of external electric field in the bulk of the system, but for a system without a boundary, it is possible that the effect of the external electric field gives rise to the velocity of fluid. In this case, a constant μ will be consistent and the terms such as $\partial_i \mu$ about which the referee is concerned will be zero.

Referee Comment 2:
In Eq. (33), why is there a τ_{imp} that appeared seemingly out of nowhere? I.e. it has nothing to do with $\partial_t T$, but was not included in Eq. (27).

Response:
We are thankful to the referee for careful reading. This was a typo that has been corrected in the present revision.

Referee Comment 3:
In Section 3.2 I am still concerned about a few points. If I use the memory matrix formalism (see e.g. the discussion in "Holographic Quantum Matter"), I would expect that if I have long-lived momentum that the conductivity (ignoring incoherent part for a moment) could be approximated as

$$\sigma_{ij} = n^2 \Gamma^{-1}_{ij}, \tag{2}$$

where Γ_{ij} relates to momentum relaxation and would encode the anisotropy, while n = χ_{JP} is a susceptibility which is fixed. Even in an anisotropic system, χ_{JP} does not become anisotropic in general. In this formalism it seems as if the structure that arises is rather different though. Since the derivation based on memory matrix methods would be rather formal and microscopics-independent, it should be valid for this system too. So the authors either ought to explain why the above expectation is wrong, or correct Section 3.2 (and possibly earlier sections too) in order to resolve the issue.

Response:
In the present work we have focused on the hydrodynamic approach, and have formalized the class of anisotropy that can be

encoded into our titlted Dirac materials metric. Of course, comparision with other methods such as memory matrix theory is interesting, and may constitute an interesting and independent research project.

Referee Comment 4:
Below Eq. (43), ω_2 can be arbitrarily large compared to ω_1. More importantly, I would expect that you would find in this anisotropic system that (if we align the tilt axis with a particular direction, let's say x) that σ_{xx} has a Drude peak with a different pole than σ_{yy} and σ_{zz}. If there are 2 poles visible in the same 'component' of sigma, it would simply be that the axes were not aligned but one could always choose 'smart' axes where sigma was diagonal and the pole structure was clearly separate. Is this what happens in this theory? Why or why not?

Response:
First thing to be noticed is that the absolute value of $\vec{\zeta}$ can not be greater than one and our study is confied to $|\vec{\zeta}|< 1$, so ω_2 can at most be three times greater than ω_1. The second point to be noticed is that the pole of the σ^{ij} depends on the **determinant** of $\mathcal{C}^i_j$ (see Eq.39) and hence the two poles structur is a property of the **entire conductivity tensor**. Assigning the pole structure to a particular component is not appropriate, simply because the precise values of the tensor depend on the "chosen" axes. To relate the tensor components obtained by different choices of axes, one needs to construct the isometries of the spacetime metric which are combination of ordinary rotations and ordinary boosts. The "smart" axis in fact is not smart, because it only diagonalizes the σ_{ij} matrix at the cost of introducing a coordinate transformation (i.e. standard rotation) *that brings us outside the spacetime given by metric (1) of our paper.*

Referee Comment 5:
I strongly object to calling this "Hall response" or "Hall-like response", it is simply anisotropic conductivity which is well-established in anisotropic materials. I also don't know why this response would be considered "anomalous" in a tilted Dirac material which is anisotropic?

Response:
Let us argue why the "anisotropic conductivity" in our case is misleading. One reason that we think (what we call) "Hall-like response" is different from "anisotropic conductivity" which is of course – as referee agrees– well established in anisotroic materials, is that in those materials one can find a "rotation" that brings any symmetric matrix into diagonal form in the bases of principal axes. In those cases, the rotation is an isometry of the spacetime metric (e.g. Galilean or Minkowsi) of the underlying material. In the new bases now one has a nice diagonal components $\sigma^{xx} \neq \sigma^{yy} \neq \sigma^{zz}$ which allows us to interpret it as "anisotropic conductivity". In such simple case, any symmetric off-diagonal components are artifact of poorly chosen axes, and can be eliminated by rotation. That is why in such cases, the anisotropic conductivity is appropriate name.

But in our case, ordinary roations are not isometries of the spacetime metric (1). Therefore rotating the symmetric conductivity tensor to make it diagonal, yes makes it look like "anisotropic conductivity", but unfortunately kicks us out of the spacetime (1). To this extent the off-diagonal components of the conductivity are inherent to the spacetime (1) and are not artifact of a poorly chosen axes. Therefore we have to choose a name for it that differs from "anisotropic conductivity" to emphasize the above fact.

Of course finding a name is a subjective matter, but still we feel that the name "Hall-like" response emphasizes the fact that (1) it is more than "anisotropic conductivity" and (2) it is a genuine property of the spacetime in Eq. (1).

Referee Comment 6:
I believe the argument at the top of page 12 is almost certainly wrong: if there were an emergent B-field that was giving rise to Hall-like transport, then the conductivity would become antisymmetric?

Response:
We appreciate the referee's intuition and revise our intuitive statement accordingly.

Referee Comment 7:
I worry the fact that the system becomes heated uniformly in a uniform temperature gradient in Eq. (54) might be a consequence of an incorrect implementation of temperature gradient in the earlier section 3.1, as per my previous point above. Typically *within linear response* one can always find steady-state solutions to the equations, and that seems not to be true here, which is a bit concerning!

Response:
We appreciate the referees intuition and insight. In fact, mathematically there exists both steady-state as well as non-steady-state solutions. Below Eq. (54) of the main text, we have added some new calculations that shows both steady-state and non-steady-state solutions. There are two sets of solutions: $(\partial_t \delta p = 0, \partial_i \delta p \neq 0)$ corresponding to steady-state solution, and another solution with $(\partial_t \delta p \neq 0, \partial_i \delta p = 0)$. Therefore Eq. (54) is mathematically correct equation and relies on $\partial_i \delta p = 0$ assumption. It expresses the fact that when $\partial_i \delta p = 0$, the temporal derivative of δp has to adjust itself in such a way to remain consistent with energy-momentum equations. Corresponding to this comment of the referee, we have added new discussion below the Eq. (54).

III. RESPONSE TO REVIEWER 2

Referee:
In the last two sentence of the abstract, I'm not sure what the term "electric energy" means. Perhaps, the authors would like to use a more precise term such as electric current.

Response:
In the revised version we have replaced it with "electric current".

Referee:
On paragraph below equation (1), the hyperlink is broken i.e. " are fermionic or bosonic [?, 20]"

Response:
We are thankful to the referee for his careful reading. The broken link is now fixed, and the Ref. [21] has also been replaced by the published version.

Referee:
On the second paragraph of Section 2, "The later is valid for the emergent spacetime" where it should be "The latter..."

Response:
Thanks for this important point. We have corrected the above typo with two other instances of the same mistake. In the revised version they appear in red.

IV. RESPONSE TO REVIEWER 1

Referee:
I still find the discussion on experimental detection lacking. While in their submission form, they state that "the vector transport coefficients ... will be comparable to thermoelectric coefficients," I still do not see numerical estimates of this in the paper. What *is* present are estimates of temperature gradients, which are (in theory) controlled by the experimenter. In other words, for a given temperature gradient/temperature fluctuation, how much current do I expect to measure in these tilted Dirac fermion systems? Without such estimates, I find claims of experimental feasibility hard to substantiate.

Response:
Around equations (65)-(68) of the revised text, based on typical numbers for spatial gradients applied to graphene, we have added estimations that show for an industrial heating rates of the order $0.4 \times 10^3\mathrm{Ks}^{-1}$, a sample of width $\sim 1\mathrm{cm}$ will give currents on the scale of $4\mathrm{nA}$ that is accessible to experimental measurements.