Reply to the comment by D. Kreimer and E. Mielke.

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Abstract

We respond to the comment by Kreimer et. al. about the torsional contribution to chiral anomaly in curved spacetimes. We discuss their claims and refute its main conclusion.

In the article by D. Kreimer and E. W. Mielke \cite{1} the existence of a chiral anomaly in spaces with torsion as reported by us in \cite{2} is challenged. Their result is presumably based on standard diagramatic techniques and regularization prescriptions. Our results, on the other hand, are derived using functional methods to evaluate formally the expectation value of the divergence of the chiral current as the regularized trace of the chiral operator.

Their claim that the Nieh-Yan four-form has been shown to be irrelevant for the anomaly is contradictory with subsequent confirmations by Obukhov et al. (second article in \cite{3}) who obtain the same anomaly in the heat kernel approach, Soo \cite{4} and Chang and Soo \cite{5}, who apply the Pauli-Villars technique in a standard diagramatic approach, and our own \cite{6,7}, where the anomaly is obtained as the Atiyah-Singer index using supersymmetry. So, apart from the puzzling fact that one of the authors of the comment is also coauthor of the paper by Obukhov et al., it is not at all clear that our result was demonstratedly incorrect.
In our view there are a number of unproven and false statements in the comment which we would like to point out. As a result, the main claim of the comment cannot be substantiated. Below we analyze those statements one by one as they appear in the text.

1)(Section I, second paragraph) “The fact that the anomaly is stable against radiative corrections guarantees that it can be given a topological interpretation.”

**Response:** Here the order should be reversed: the anomaly is stable under perturbative corrections because it is a topological invariant.

2)(Third paragraph) “...there is no doubt that the NY term can be possibly generated, as demonstrated previously [3,8]...”

**Response:** It should be clarified that the second article in Ref. [3] did not precede ours. As stated in the abstract of Ref. [3], “Following Chandia and Zanelli, two spaces with non-trivial translational Chern-Simons forms are discussed. We then demonstrate, firstly within the classical Einstein-Cartan-Dirac theory and secondly in the quantum heat kernel approach to the Dirac operator, how the Nieh-Yan form surfaces in both contexts, in contrast to what has been assumed previously.”. Moreover, although the NY form can be found in the first article of Ref. [3] and in Ref. [8], it is not identified as a topological invariant and mixed with dozens of other terms which are collectively discarded as irrelevant. So, to say that “in Refs. [3,8] it was demonstrated that the NY term is undoubtedly generated” is a gross overstatement. To set the record straight, we were the first to point out that the topological NY-four form is generated in Einstein-Cartan spaces.

3)(Third and fourth paragraphs) “In rescaling the tetrad, the authors of Ref. [2] ignore the presence of renormalization conditions and the generation of a scale upon renormalization. Rescaling the tetrad would ultimately change the wave function renormalization Z-factor...This factor creeps into the definition of the NY term at the quantum level, and thus a rescaling of the tetrad does not achieve the desired goals... With no renormalization condition available for the NY term, and other methods obtaining it as zero, we can only conclude that [the anomaly] delivers no NY term. Or, saying it differently, its finite value is zero after renormalization.”
Response: Anomaly calculations assume a given background –which is not necessarily quantized– and a quantized chiral field. Pertubative radiative corrections cannot change the a topological density, which is a function of the background field [Adler-Bardeen-Zumino theorem]. Then, it is sufficient to check that the result is a topological invariant (including scale invariance, of course), in order to be sure it does not renormalize. Kreimer et al. do not show –in this paper or elsewhere– that the rescaled Nieh-Yan four form, $l^{-2}[T^{aT}a - R_{ab}e^ae^b]$ is not a topological invariant, nor have they shown that renormalization does what they claim. Hence, there is no ground to claim that our result is spurious and will be erased by renormalization.

4) (Section II, third paragraph) “Since the coframe is the translational part of the Cartan connection...”

Response: That identification rests on the assumption that the tangent space be flat and with a Poincaré fiber. Since Minkowski space has no scale, identifying the vielbein with the “connection” associated to translations is a slippery issue: the vierbein has dimensions of length, while the connection is dimensionless. Hence, one is forced to introduce an artificial length scale in a scale invariant space. Instead, we would prefer to identify the tangent space with a manifold of constant curvature with (A)dS local invariance. This has the advantage of having a naturally defined length scale $l$ (radius of curvature).

5) (Fourth paragraph) “The corresponding CS term $\hat{C}$ spplits via $\hat{C} = C_{RR} - 2C_{TT}$ into the linear one and that of translations, see the footnote 31 of Ref. [9]. This relation has recently been “recovered” by Chandia and Zanelli [11].”

Response: Here the wording and the use of quotation marks is meant to imply that we have appropriated an idea found in Ref. [9] without acknowledging it. This splitting was “recovered” by the present authors as much as by the authors of Ref. [9]. It is first mentioned in the original papers by Nieh and Yan [10], and further discussed also, several years prior to Ref. [9], in a paper by A. Mardones and J. Zanelli [11]. What was not at all clear in the literature before our paper, was the topological nature of the NY invariant or its relationship with the Chern classes of SO(5) and SO(4), and much less, its relevance for
6) (Section III, second paragraph) “The decomposed Lagrangian (3.1) leads to the following form of the Dirac equation

\[ i \ast \gamma \wedge \bar{D} \psi + \ast m \psi = i \ast \gamma \wedge \left[ D^{(1)} + i \frac{m}{4} \gamma + i \frac{A}{4} \gamma_5 \right] \psi = 0 \quad (3.2) \]

in terms of the Riemannian connection \( \Gamma^{(1)} \) with \( D^{(1)} \gamma = 0 \) and the irreducible piece (2.3) in the torsion. Hence, in a RC spacetime a Dirac operator does only feel the axial torsion one-form \( A \). This can also be seen from the identity (3.6.13) of Ref. [9] which specializes here to the “on shell” commutation relation

\[ [\bar{D}, \bar{D}] = \Omega^{(1)} + \frac{i}{4} \gamma_5 dA - \frac{i}{8} m^2 \sigma. \quad (3.3)^{\prime \prime} \]

**Response:** Although the “on shell” Dirac operator \( \bar{D} \) is not clearly defined, it is not true that because other components of the torsion do not appear in the Dirac equation (3.2), they will not contribute to the interaction. The expression for the commutator is incorrect. The correct form is found in the literature and can be easily checked to be

\[ [D, D] = \frac{1}{4} J^{ab} J^{cd} R_{abcd} - J^{ab} e_\mu^a e_\nu^b e_\lambda^c T_{\mu \nu}^c D_\lambda. \]

Clearly, if one uses the “on shell” relations, one could cancel some part of the second term in the RHS. The problem is then how to justify using “on shell” relations which don’t go through in the quantum regime. It is precisely this last term that gives rise to the Nieh-Yan form through the Fujikawa method, which obviously could not be reproduced starting from (3.3).

7) (Fifth paragraph) “From Einstein’s equations ... and the purely algebraic Cartan relation... one finds

\[ dj_5 \approx 4 dC_{TT} = \frac{2}{l^2} \left( T^\alpha \wedge T_\alpha + R_{\alpha \beta} \wedge \vartheta^\alpha \vartheta^\beta \right) \quad (3.4) \]

which establishes a link to the NY four form, but only for the massive fields.”

**Response:** There are three remarkable points here: the first is that in spite of the above relation, the authors continue to believe that “in a RC spacetime a Dirac operator does only
feel the *axial torsion* one-form $A$" (see 6, above). How can this be if at the same time the violation of the fermionic chiral current is due to the interaction with torsion through the NY torsional term? The second point is their use of the Einstein equations. Why should the Einstein equations be at all relevant to this problem? In fact, as is well known, the integrability condition for the Dirac operator is precisely Einstein’s quations (in particular, that is why local supersymmetry requires gravity, and that is one way in which supergravity arises). Thus, had they computed the integrability condition correctly, they would have found Einstein’s equations and the rest of the argument would follow naturally. The third remarkable point is their claim that the result be valid for massive fields only. This is puzzling because there is no mass parameter in (3.4) and nothing seems to prevent taking the limit $m \to 0$. This last observation shows that there must be something fishy about their next claim:

8) (Sixth paragraph) “in the limit $m \to 0$, we find within the dynamical framework of ECD theory that the NY four-form tends to zero “on shell”, i.e. $dC_{TT} \cong (1/4) dj_5 \to 0$”.

**Response:** Again, the suspect “on shell” relations are invoked to justify an otherwise irreproducible result, because the only relation that links $dj_5$ with the mass are “on shell” equations.

9) (Section IV, second and third paragraphs) “[in order to calculate the anomaly, we] concentrate on the last term $[-\frac{1}{4} A \wedge \bar{\psi} \gamma_5 \gamma \psi]$ in the Lagrangian... this term can be regarded as an *external* axial covector $A$ ... coupled to the axial current $j_5$ of the Dirac field in an *initially flat* spacetime. By applying the result (11-225) of Itzykson and Zuber..., we find that only the term $dA \wedge dA$ arises in the axial anomaly, but *not* the NY type term $d \ast A \cong dC_{TT}$ as was recently claimed [2].”

**Response:** The calculation in Itzykson and Zuber (I-Z) would be valid for commutation relations of the form (3.3), but unfortunately, as we said in 6), this is not the case. Eq. (3.3) is valid only on shell, but that is insufficient to apply the I-Z result, especially because this is supposed to be a quantum calculation. So it is not that the I-Z approach is wrong, it is just not designed to handle a Dirac operator that satisfies a more complicated relation such
as that in RC spaces, so it should be rederived in order to apply it to this case, something Kreimer and Mielke didn’t do.

For the purposes of our work, the $U(1)$ anomaly is completely standard and cannot yield anything new which was not there already, say, in electrodynamics. In particular, since $\pi_3[U(1)]$ is trivial [12], its presence can be gauged away.

10) (Fifth paragraph) “Whereas in $n = 4$ dimensions the Pontrjagin type term $K_4$ is dimensionless, the term $K_2 \sim 2l^2dC_{TT}$ carries dimensions. It can be consistently absorbed in a counterterm, and thus discarded from the final result for the anomaly.”

Response: Kreimer and Mielke provide no proof of this claim. They do not exhibit the counterterm or the radiative corrections that can give rise to it. Although they often refer to renormalization and counterterms, there is not a single one-loop calculation to be found anywhere in the paper.

11) (Seventh paragraph) “In Ref. [2] it is argued that such contributions can be maintained by absorbing the divergent factor in a rescaled coframe $\tilde{\vartheta}^\alpha := M\vartheta^\alpha$ and propose to consider the Wigner-In"on"u contraction $M \to \infty$ in the de Sitter gauge approach [6], with $Ml$ fixed.”

Response: This is not true. We did not propose that rescaling. All we did was to observe that if one replaces $\tilde{\vartheta}^\alpha$ by $(Ml)^{-1} \vartheta^\alpha$ [Eq. (30) in our paper], the result reads,

$$A(x) = \frac{1}{8\pi^2} \left[ R^{ab} \cdot R_{ab} + \frac{2}{l^2} (T^a \cdot T_a - R_{ab} \cdot e^a \wedge e^b) \right].$$  \hspace{1cm} (1)

This, in the language of Kreimer and Mielke is equal to $dC_{RR} + dC_{TT}$, which is just the Chern class for $SO(5)$.

12) (Ninth paragraph) “1. As the difference (2.1) of two Pontrjagin classes, the term $dC_{TT}$ is a topological invariant after all. Now, it is actually not this term which appears as the torsion-dependent extra contribution to the anomaly, but more precisely $-d \ast A = 2l^2dC_{TT}$. Thus, measuring its proportion in units of the topological invariant $dC_{TT}$, we find that it vanishes when we consider the proposed limit $M \to \infty$, keeping $Ml$ constant.”

Response: The redefinition $\vartheta \to (Ml)^{-1} \vartheta$, does not change the units (assuming $c$ and
The scaling properties of $C_{TT} = l^{-2} \vartheta^\alpha \wedge T^a$ depend on how $\vartheta$ and $l$ are supposed to scale. If the fields are properly defined, the one-form $\vartheta$ has dimensions of length and therefore $l^{-1} \vartheta$ is dimensionless, like any well-defined connection one-form. Then, $C_{TT}$, $dC_{TT}$ and the anomaly are scale invariant as they should.

13) (Tenth and eleventh paragraphs) “...consistently a renormalization condition can be imposed which guarantees the anomaly to have the [torsion-free] value. Even if one renders this extra term finite by a rescaling as in Ref. [2], one has to confront the fact that a (finite) renormalization condition can be imposed which settles the anomaly at this value.... From a renormalization group point of view, it is the scaling of the coupling which determines the scaling of the anomaly... a property which is [needed to satisfy the conditions of] the Adler-Bardeen theorem. Or, to put it otherwise, an anomaly is stable against radiative corrections for the reason that such corrections are compensated by a renormalization of the coupling. While, on the other hand, the topological invariant of Ref. [2] has no such property, its interpretation as an anomaly seems dubious to us.”

Response: As it is easily seen, Kreimer and Mielke’s comment is more of a warning about the problems one might encounter than a proof that something wrong has actually been done. As they produce no evidence in support of their contention, they end up in a sceptical remark. This is the most honest claim in the entire comment (even if it is incorrect because the topological invariant of Ref. [2] does possess the property they would like it to have). In our opinion, they should have limited themselves to just that last line.

14) (Fifth section) Here Kreimer and Mielke essentially repeat their claims without adding any new arguments.

In conclusion, we can summarize the following points:

A The authors of the comment do not argue against the fact that the NY term is present in the chiral anomaly as we had shown.

B They challenge the contribution of the Nieh-Yan topological invariant, on the grounds that they do not find it through manipulations in which they use “on shell” conditions.

C They furthermore claim that radiative corrections will renormalize the NY term to
zero, although they do not produce any evidence for this (e.g., loop corrections to the effective action, etc.). Their claim rests on a scaling argument, according to which the NY term scales with the mass, which is incorrect or at best arbitrary.

D They criticize a rescaling argument which they attribute to us but which is nowhere to be found in our paper.

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