In the new version of the manuscript, the Authors have added a number of clarifications regarding the scope of their calculations as well as the broader context of the study. However, I believe that the most important comment from my previous report ("Are any physical quantities of the system uniquely determined by this invariant?") has not been addressed properly. Let me elaborate on it to avoid misunderstanding.

While the Chern number of quantum Hall states or topological superconductors can be related to the Euler characteristic, the crucial property of these states is the existence of a physical observable (Hall or thermal Hall conductivity) that is determined by the topology only (and the universal constants), i.e. "quantized". A recent example, where Euler characteristic has been related to physical observables can be found in arXiv:2108.05870. The authors suggest the number of gaps in a system under consideration" as such a quantity - however, no way of actually measuring it is suggested. One can think of measuring DOS at ω=0 via STM; however, it is known that in actual experiments (see e.g., Phys. Rev. 137, A557 (1965); Phys. Rev. Lett. 53, 2437 (1984)) a non-zero density of states at ω=0 appears routinely even where it is not expected from an Abrikosov-Gor'kov theory standpoint. While the origin of this behavior is not fully clear (see e.g., Phys. Rev. Lett. 105, 026803 (2010), Phys. Rev. B 94, 144508 (2016)), it effectively rules out STM. Spectroscopic measurements, such as optical conductivity, would be contaminated at finite temperatures by quasiparticles. Taking into account in addition that the gapless superconductivity is expected to occur in an extremely narrow parameter regime, it does not appear that the topological characteristic can manifest itself robustly in experiments, severely limiting the impact of the present work.

We thank the Referee for the clarification of his/her statement and below will try to clarify our vision of the problem and, where possible, to account for his/her criticism.

1. The main concern of the Referee is related to the difficulties in the unique determination by the Euler characteristics of the physical quantities of the system undergoing the transition from the gap-to the gapless state. *We see the main value of our work in the discovery of the fact of the topological nature of the sixty years ago established by Abrikosov and Gor'kov transition between the gap and the gapless states of a superconductor itself*. We calculate the Euler characteristics just for its formal topological description, but this is a nuance. Nevertheless, its change tells us about the appearance (disappearance) of the number of gaps. The latter can probably be detected by tunneling measurements (the essential effect of the Lifshitz transition on the tunneling resistance in the limit of low temperatures was predicted in the paper "Anomalies in the properties of tunnelling and Josephson junctions in the vicinity of a Lifshits topological transition" (Sov. Phys. JETP 66, 396 (1987), http://jetp.ras.ru/cgi-bin/e/index/e/66/2/p396?a=list)).
2. The value of the seminal paper of Abrikosov and Gor'kov consists in the refusal of the straight requirement of the presence of the gap in quasi-particle spectrum for the existence of the phenomenon of supercurrent itself (assumed in the BCS theory). *We attract attention to the topological nature of such transition.* And it does not matter, how narrow is the corresponding domain of parameters. The existence of the Anderson-Brinkman-Morel phase is also very restricted at the phase-diagram of Helium-3, yet it became the object of the intensive study during decades (see nice reviews of Volovik

and his book "Universe in a Helium Droplet"). The requirement of the low temperatures for experimentalists today is not restrictive at all, since in the case under consideration the low temperature means only $k_B T \ll \min\{\Delta, \hbar\tau_s^{-1}\}$.

3. The next point in our appeal to the Lifshitz transition consists in the attraction of the attention of experimentalists to the number of observables, which should undergo anomalies close to the gap-gapless transition:
   - thermoelectric coefficient,
   - jump in the derivative of the specific heat capacity,
   - the current-voltage characteristics of the tunnel and the Josephson junction in the vicinity of the topological transition.
4. The Referee refers to the paper of Michael A. Woolf and F. Reif (Phys. Rev. 137, A557 (1965)) as the routine experiment, while this paper is cited as the *evidence* of the existence of the gapless phase in the textbook of De Gennes "Superconductivity Of Metals And Alloys" in the Chapter 8. The citation of Phys. Rev. Lett. 53, 2437 (1984) is not relevant, because the Authors of this paper studied granular aluminum that can exhibit properties of unconventional superconductivity and does not correspond the case of the pure s-wave state considered in our manuscript (see, e.g., N. Bachar et al., "Signatures of Unconventional Superconductivity in Granular Aluminum", https://link.springer.com/article/10.1007/s10909-014-1244-z).

The calculation of the thermoelectric coefficient, the second major result of the work, as the Authors acknowledge in their Reply, has been considered previously for a more general case: "the more general consideration in Ref. 34". Taking the points above into account, I can not recommend the current manuscript for publication in SciPost Physics, given that the criterion for this journal is to provide details on groundbreaking results". However, as the paper does explore interesting theoretical connections and provide guidelines for developments in thermoelectric effect, I believe that this paper is appropriate for publication in SciPost Core.

5. We would like to emphasize that our appeal to the study of the Seebeck coefficient in the vicinity of the transition under discussion does not pretend to be the first in literature but is called up to show the affinity of the transition between the gap and the gapless states with the standard Lifshitz transition, characterized in the first hand by the anomalies in the thermoelectric coefficient.
6. Finally, our results shed light not only at the Abrikosov-Gor'kov theory from the topological point of view, but also at the other subjects that we mention in the text. Namely:

- the $s_{\pm}$-s_{++} transition induced by impurities in a two-band superconductor,
- color superconductivity,
- the new interpretation of experimental results like lightwave-driven gapless superconductivity (see Ref. 6) as the light-induced topological transition in a superconductor.

As one can see the impact of the present of work can be extended to another hot topic phenomena. Moreover, we believe that this work renews experimental interest to the gap-gapless transition from the topological point of view. That's why in our opinion this manuscript can be published in SciPost.

Additionally, there is a technical comment I would like to make. I believe that the statement that "at ω=0, ζ=1 the Gaussian curvature is not diverged and equal to zero" is not, strictly speaking, correct, when Eq. (17) for DOS is used.

We forgot to include the important expression for the special case ζ=1 (see also Ref. 4 in the current version of the manuscript)

$$N_s(\omega,\zeta=1)=N(0)\frac{\sqrt{3}}{2}\left[\left(\frac{2\omega}{\Delta}\right)^{1/3}-\frac{1}{24}\left(\frac{2\omega}{\Delta}\right)^{5/3}\right],$$

where there is no problem with divergence at ω=0, ζ=1.
As for the case $\omega=\Delta_g$, ζ→0 one should use the first string in Eq. (17) for which DOS is strictly equal to zero, and therefore, there is no singularity. We correct an inequality sign in Eq. (16).
Also, we will add an explanation to the manuscript (blue highlight).
These corrections do not change the main conclusions of our manuscript.

To conclude we thank the Referee for the criticism, and we believe that now the manuscript deserves publication in SciPost.