

Pseudo Point Nodal Superconducting Gap in Spin-Triplet UTe₂

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This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

The superconducting gap structure of unconventional superconductor UTe₂ is an important issue. This paper reports thermal conductivity measurements on high-quality UTe₂ single crystals down to ~50 mK, with the heat current along b axis and magnetic field along all three directions (a, b, and c), respectively. Their results suggest that UTe₂ exhibits a fully gapped state with a pseudo point-nodal structure — where gap minima approach but never reach zero.

While this work is interesting, I have some comments:

(1) In the caption of Fig. 2, "c, Calculated temperature dependence of κ/T " should be "c, Calculated temperature dependence of normalized κ/T (see text)".

(2) On page 4, the authors wrote "At 0.3K, zero-field κ_b/T exhibits strong suppression under a magnetic field of 1T, attributed to enhanced quasiparticle scattering by magnetic flux. Given that κ_b^{ph} is insensitive to magnetic fields," This is incorrect. In the normal state under high field, there is no magnetic flux, the phonon thermal conductivity is much smaller than that in zero field. The suppression of phonon thermal conductivity in magnetic field (and in the normal state under high field) is actually due to strong scattering of phonons by quasiparticles.

(3) Previously there are two ultra-low-T thermal conductivity measurements on UTe₂, with heat current along a axis. The first one is from current group (Science Advances 2024, ref. [29] of this manuscript), which concludes that UTe₂ has fully gapped pairing state. The second one is from Prof. Johnpierre Paglione's group (PRX 2025, ref. [30] of this manuscript), which claims the presence

of symmetry-imposed nodes in the superconducting gap of UTe₂. While these two previous works are still controversial to each other, I do not think current work along b axis is conclusive for identifying the superconductivity gap of UTe₂. I mean if the gap is nodal along a axis, the full gap along b axis seems not that important. If the Maryland group (ref. [30]) did similar measurements along b axis, the results may be also controversial.

Reviewer #2

(Remarks to the Author)

Reviewer #3

(Remarks to the Author)

Since the discovery of unconventional superconductivity in UTe₂ in 2018, few topics in condensed matter physics have been as hotly debated as the nature and symmetry of its superconducting gap. In this manuscript by Hosoi *et al.*, the authors present their latest measurements of *b*-axis thermal conductivity in UTe₂ down to dilution fridge temperatures and as a function of magnetic field (and field direction), from which they conclude that the superconducting gap structure has a "pseudo-point-nodal" structure: an energy gap that drops to a small value at a minimum along k_y but remains nonetheless finite. This contrasts with some other recent thermal conductivity work claiming true point nodes [Hayes *et al.*, Phys. Rev. X

15, 021029 (2025)] and will certainly elicit significant interest from the community.

I think the data presented here are sufficiently novel (given the b -axis results), the data appears to be reliable, and their overall interpretation seems reasonable, or at the very least plausible, and will certainly be of interest to the readers of Nature Communications. I do think the manuscript should eventually be published in this journal, however there are several issues I think should be addressed first.

The primary issue I have concerns the zero-field thermal conductivity data (as presented in Figure 2) and its discussion. The authors' main argument here is that a finite gap would better describe the data than would a true point node, because the point node (but not the finite gap) would produce a low temperature peak that is not visible in their data. The problem is that the evidence is not so clearly presented; for one, it is somewhat difficult to see the low temperature behavior in Figure 2b, given the scale and the point size. It would be good to zoom in below $T=0.1$ K (either in the main panel or just an inset) to better show the reader this important region, to make it more clear whether or not a small bump or residual linear intercept can truly be ruled out; at present it's too hard to see at this scale. One minor note is that the caption of Figure 2 refers to labels a, b, and c even though such labels are absent from the figure itself, although I'm sure the reader can guess.

In the supplementary information section regarding calculation of thermal conductivity for different scenarios, as shown in Figure S4, the top right panel shows the dependence on scattering phase shift for a particular scattering rate. Here it appears that $c=0.2$ and $c=0.15$ indeed have prominent low temperature peaks that were not observed in the measured data – however, for $c=0.1$, and presumably some range of similar scattering rates and small-but-finite c values, this peak is hardly visible. I would argue that their zero-field data presented in Figure 2b might not be inconsistent with such a scenario (true point node but small non-unitary phase shift c), so it would be good if the authors could explain and/or better show why this is ruled out. It is true that the field-dependence data which follows provides a more convincing argument, so the authors may wish to make a less conclusive statement for the zero-field data (for example, pointing out that the pseudo-point-node scenario fits well but that true point nodes with small but finite scattering phase shifts could also still be compatible, if that is in fact the case) but then emphasize that the field dependence data provides confirmation.

Figure S4 and its caption appear to have some labelling issues. Firstly, the caption says that true point nodes are in the left column and finite gap in the right column, but the figure itself has that reversed. Also, the labelling for the curves in each of the two bottom panels appears to be reversed: the unitary ($c=0$) and finite c curves seem to be swapped if I'm not mistaken. In short, I think the paper is worthy of publication in Nature Communications and should indeed eventually be published there, but these weak points regarding claims from the zero-field data alone should first be addressed.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

I read all the responses from the authors. While the situation for the thermal transport measurements of UTe₂ from the current group and the Maryland group is confusing, I'm not against the publication of this work in Nature Communications.

Reviewer #2

(Remarks to the Author)

Report on the paper by Hosoi et al : Pseudo Nodal Superconducting gap in Spin-Triplet UTe₂

In this resubmission, the authors tried to answer the different criticism addressed by the 3 referees, clarifying some of the statements and adding also new measurements under field presented in the supplementary data (Fig S2).

Focusing first on the point on the Wiedemann Franz law, for which the authors took the supplementary data at 11 T, the authors argue that at low temperature, under field, the agreement between the thermal and electric measurement is verified within 10%. This is fine. However, there is some misunderstanding and I believe still a problem for the "high temperature data (between 2 and 4 K)". I did not say that the phonon contribution was negligible in this range, but that the electronic contribution was large enough in the best samples of ref [29] so that the Lorentz number was below the Lorentz ratio: this is still the opposite in the present work, meaning probably that the electronic contribution for $J//b$ -axis remains smaller than the phonon contribution. I assume that in the normal state, the phonon mean free path is governed in UTe₂ by electron-phonon interactions, and should be weakly sample dependent (sample geometry and surface state might indeed matter, but only deep in the superconducting state and in very low field). However, do the authors have an explanation for why the measurement of the thermal conductivity at 11 T is below that in zero field between 2 and 4 K? The resistivity is lower at 11 T than in zero field above 2 K, so I would expect a larger electronic contribution under field, and a very similar phonon contribution (there is no reason for a large field-sensitivity of the phonon contribution in the normal state?). Does it mean that magnetic contribution also plays a role in the thermal transport of UTe₂? This might need still further investigations

Regarding the question on the temperature dependence of the thermal conductivity at low temperatures, also raised by referee 1, in the paper, the authors now discuss a substantial quasiparticle contribution above 0.1 K, which is fine indeed (indeed more appropriate than the "dominant quasiparticle contribution" from the rebuttal letter!). However, the main point of worry was not really addressed, and was probably not stated clearly enough in my questions: I asked for a quantitative estimate of the maximum predicted theoretically for point nodes because the authors claimed, and still claim in this new version (top of page 4), that such a maximum should be detectable. However, the theoretical minimum is at $0.05 T_c$ hence

0.1K, where the thermal conductivity is sample independent hence totally governed by phonons, and the maximum would be way below the minimum temperature of the measurements (for the chosen parameters of Fig 2c). Hence, if the authors can claim that the peak should be detectable, it means that they did make a quantitative estimate of the theoretical minimum and they should present it. Otherwise, wouldn't it be better to just say like in [29] that presently, with the large phonon contribution below 0.1K, it is not possible to make any conclusion from the T-dependence and indeed rely entirely on the field dependence?

Regarding these measurements under field (Fig 3), the authors made corrections on page 4 explaining the change of behavior under field and notably the crossing of the field data with the $H=0$ curve. The following point is not important for the conclusions of the authors, and of course, I completely agree that the electronic contribution increases under field due to the increase of thermal excitations with the doppler effect (much stronger for the perpendicular directions than for the $H//b$ configuration). However, isn't the crossing with the $H=0$ curve also the result of a much-suppressed phonon contribution (expected to go roughly as T^3 below 0.3K in zero field), due to the restauration of electron-phonon scattering with the presence of more electronic thermal excitations? I do not really understand why it demonstrates significant inelastic quasiparticle scattering? But again, this is a minor point, probably just misunderstanding.

Naturally, the key issue is the field dependence of the extrapolated residual term, for which the authors claim to observe a threshold (Fig 4), as predicted for a weak minimum. The authors have made new analysis presented in supplementary Figure R2, where they could demonstrate that different fitting procedures reproduced the same (weak) feature at around $0.02 H/H_{c2}(0)$. However, figure R2c also show precisely that a linear fit of the data would indeed lie within the error bars. By contrast, on Fig 4b or R2b, when the thermal conductivity for $H//b$ is subtracted from the data for $H//a$, the threshold appears very clearly. Hence, the claim for the existence of the threshold would be much stronger if the authors could justify better why subtracting the data for $H//b$ is justified.

In the paper, it is only said that the data for $H//b$ represents the field dependence without doppler shift, but it does not explain where it originates from and a fortiori, whether it should be similar for both field orientations.

In the rebuttal letter, the authors also explain that a small increase of κ (whatever the field direction) under weak field is expected even in conventional fully gapped superconductors once H is larger than H_{c1} ? However, in the same system, for $j_Q//a$, the same authors measured a completely flat κ far beyond H_{c1} ? Then why would it be so different below the threshold for $j_Q//b$? The same question holds also for theoretical explanations proposed (only in the rebuttal letter), through effects arising beyond the quasiclassical approximation: shouldn't all of these effects (relying on the contribution of core states, or variation of the local superfluid velocity in the vortex lattice unit cell) be very much sensitive to the relative orientation of field and heat current? They could then hardly justify the subtraction presented in Fig 4c?

Last the authors have (from my point of view), well clarified the sentence on the corrugation of the Fermi surface and the possible gap minimum. The question of citing non unitary states in the conclusion is a minor point, and the authors corrected also the reference to the first quantum oscillation measurements.

Again, the measurements are really important and should be published. But I still believe, despite the nice improvements done by the authors, that some key points on the data above 2K and on the analysis of the field dependence of the residual can and should be improved before publication: it should provide more convincing evidence or clearer status of the present state of what can be said on this hard issue of the gap nodes in UTe₂.

Reviewer #3

(Remarks to the Author)

Having reviewed the revised version of the manuscript, I am satisfied that the authors have fully addressed with this new version each one of my concerns raised for the previous version. In my opinion, they have also made a valiant effort (in the rebuttal and paper modifications) at addressing those of the other reviewers as well, although those reviewers will no doubt express their own opinions on this.

I now gladly recommend this revised version of the manuscript for publication in Nature Communications.

Version 2:

Reviewer comments:

Reviewer #2

(Remarks to the Author)

In this resubmission, the authors have tried to answer a few points requiring further clarifications. There are two issues which remain unanswered:

- The first is the high temperature behaviour of κ/T under field, presented in the supplementary for the discussion of the Wiedemann-Franz law. The authors have added a paragraph in the supplement explicitly pointing to this unsolved issue

(κ/T drops under field when it was expected to be larger than in zero field due to an increased electrical conductivity, suggesting a field suppression of the phonon contribution, which is not so easily understood). As correctly stated by the authors, this should not influence the analysis of the low temperature data, and will remain a point to be checked and investigated by future experimental work.

- The second is why there would be a background contribution of κ/T (for $T \rightarrow 0$) from the mixed state for heat current along b , when it is absent for heat current along a ? This point was not really discussed or answered by the authors, and personally, I remain very skeptical that figure 4c is meaningful. The presentation of the theoretical arguments for this subtraction has been improved, and for the experimental arguments, the authors insist even more that the raw data already present the "threshold" with field distinguishing a finite small gap from point nodes. I do not think that this point can be further improved with the present data, it will probably remain yet another challenge for future experiments to test if they can reproduce it.

Regarding all the other points the authors have answered very satisfactorily all the remarks:

- For the analysis of the temperature dependence, owing to the dominant phonon contribution below 0.1K, it is possible to state that the results are compatible with a gap minimum and do not show a small maximum that would be the hallmark of a point node, but not to consider the absence of a peak as strong evidence for the absence of nodes. This is very clearly written in this last version

- On the (minor point) of the crossing of the zero-field data of κ/T with field measurements (between 1T and 6T) between 0.15K and 0.3K, the authors have also clarified that it could also arise from the decrease of the phonon contribution due to enhanced scattering on electronic excitations.

- The authors have also removed in the conclusion the remark on non-unitary state, so that it remains focused on the main result of the paper.

Hence, even though not all questions are solved (but this is a general issue with all works on UTe₂ !), I do support publication of this important work in Nature Communication in its present state.

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[Reply to Reviewer #1]

The superconducting gap structure of unconventional superconductor UTe₂ is an important issue. This paper reports thermal conductivity measurements on high-quality UTe₂ single crystals down to ~50 mK, with the heat current along b axis and magnetic field along all three directions (a, b, and c), respectively. Their results suggest that UTe₂ exhibits a fully gapped state with a pseudo point-nodal structure — where gap minima approach but never reach zero.

We thank the Reviewer #1 for the careful reading of our manuscript and for the constructive comments, which have helped us improve the clarity of our paper. Below we address each point raised by the Reviewer.

While this work is interesting, I have some comments:

(1) In the caption of Fig. 2, "c, Calculated temperature dependence of κ/T " should be "c, Calculated temperature dependence of normalized κ/T (see text)".

Thank you for pointing this out. We have corrected it accordingly.

(2) On page 4, the authors wrote "At 0.3K, zero-field κ_b/T exhibits strong suppression under a magnetic field of 1T, attributed to enhanced quasiparticle scattering by magnetic flux. Given that κ_b^{ph} is insensitive to magnetic fields," This is incorrect. In the normal state under high field, there is no magnetic flux, the phonon thermal conductivity is much smaller than that in zero field. The suppression of phonon thermal conductivity in magnetic field (and in the normal state under high field) is actually due to strong scattering of phonons by quasiparticles.

We have revised the manuscript to remove the incorrect reference to "magnetic flux" and the assumption that κ_b^{ph} is insensitive to magnetic fields. We agree that phonon thermal conductivity can be suppressed through enhanced scattering of phonons by quasiparticles under magnetic field.

However, as shown in Figs. 3a and 3c, in the low-temperature regime ($T < 0.3$ K), our data clearly indicate that the thermal conductivity is dominated by quasiparticles rather than phonons. The field dependence of κ_b/T exhibits pronounced anisotropy between the $H \parallel a$ and $H \parallel c$ configurations, in both of which the magnetic field is perpendicular to the heat current. This anisotropy therefore originates from the quasiparticle contributions.

Since phonon thermal conductivity is expected to be essentially insensitive to the field orientation when the field is perpendicular to the heat current – because phonon-quasiparticle scattering depends primarily on quasiparticle density rather than the field direction—the pronounced anisotropy between $H \parallel a$ and $H \parallel c$ is unlikely to originate from a phonon-dominated mechanism, even after accounting for the H_{c2} anisotropy.

As suggested by Reviewer #1, one might argue that anisotropic quasiparticle populations could modify the phonon thermal conductivity through quasiparticle-phonon

scattering. However, this scenario appears quantitatively insufficient. At the same magnetic field, $\kappa_b(H||c)$ is suppressed by more than a factor of two compared to $\kappa_b(H||a)$, whereas the number of quasiparticles for $H||a$ is only expected to be larger by approximately 50 %, based on the difference in H_{c2} . It is therefore difficult to reconcile the magnitude of the observed thermal conductivity anisotropy with a purely phonon scattering by quasiparticles.

Furthermore, as shown in Figs. 3a–c, κ_b/T at $H = 0$ crosses κ_b/T at finite fields. This crossing is a hallmark of inelastic quasiparticle scattering and is difficult to explain within a phonon-only picture without invoking complicated scattering processes.

Taken together, these observations provide evidence that the measured thermal conductivity at low temperatures is dominated by quasiparticles. We also note that the observed sample quality difference is consistent with the dominant quasiparticle contributions at low temperatures (See also our response to Reviewer2).

(3) Previously there are two ultra-low-T thermal conductivity measurements on UTe₂, with heat current along *a* axis. The first one is from current group (Science Advances 2024, ref. [29] of this manuscript), which concludes that UTe₂ has fully gapped pairing state. The second one is from Prof. Johnpierre Paglione's group (PRX 2025, ref. [30] of this manuscript), which claims the presence of symmetry-imposed nodes in the superconducting gap of UTe₂. While these two previous works are still controversial to each other, I do not think current work along *b* axis is conclusive for identifying the superconductivity gap of UTe₂. I mean if the gap is nodal along *a* axis, the full gap along *b* axis seems not that important. If the Maryland group (ref. [30]) did similar measurements along *b* axis, the results may be also controversial.

We stress that Refs. [29] and [30] are in full agreement regarding the absence of nodes along the *a* axis. The only controversy concerns the possible existence of nodes along the *b* axis. Ref. [30] interprets their data as evidence for symmetry-imposed point nodes along the *b* axis, ruling out those along the *a* axis. In this context, the most critical direction for resolving the superconducting gap structure of UTe₂ is transport measurements along the *b* axis.

We wish to emphasize that this is fundamentally a question of symmetry. The presence or absence of symmetry-imposed point nodes along the *b* axis corresponds to distinct irreducible representations of the superconducting order parameter. Therefore, determining whether the gap along the *b* axis is nodal or finite is not merely a quantitative refinement but directly constrains the pairing symmetry of UTe₂. We respectfully disagree with the referee's suggestion that this distinction is not important. Indeed, after the submission of our manuscript, we note that the Maryland group has recently posted a preprint reporting magnetic field–angle-dependent thermal conductivity measurements along the *a* and *c* axes, inferring that the point nodes are located along the *b* axis (I. M. Hayes et al., arXiv:2512.13877). This further underscores that the *b*-axis gap structure is at the center of the current debate.

We emphasize that, as we pointed out in the manuscript, without measurements of thermal conductivity along the *b* axis, it is experimentally impossible to determine the true

point nodes or finite gap minima in this direction. The Maryland group, including their recent preprint, has not performed b -axis thermal conductivity measurements; their conclusions regarding b -axis nodes are inferred indirectly from κ_a or κ_c data. Our work provides the first direct b -axis thermal conductivity measurements, and through careful analysis — including the observation of a threshold field kink in κ_b^0/T for $H \parallel a$ (Figs. 4b, c), which is a hallmark of a finite minimal gap — we identify a very small but finite gap minimum along the b axis. While some studies, including the Maryland group's new preprint, have interpreted their data in terms of point nodes, our results instead indicate that the gap remains finite, though strongly anisotropic, along the b -axis direction. Our results are consistent with those of Maryland group regarding the overall shape of the gap, with the main difference in whether it is a true node or a gap minimum along the b -axis. We argue that we have experimental sensitivity to distinguish the two cases, and our results therefore have direct implications for the irreducible representation and thus the pairing symmetry of UTe_2 . This finding has direct implications for the irreducible representation and thus the pairing symmetry of UTe_2 .

[Reply to Reviewer #2]

In this work, the authors report new thermal transport measurements on high quality single crystals of the unconventional superconductor UTe_2 , together with a theoretical analysis of these measurements. Numerous measurements have endeavored to determine the nodal gap structure of this potential spin-triplet superconductor, often leading to contradictory results regarding the existence and the position of the gap nodes, as well as topological properties of this system. After an extensive review of the present debate on this matter, the authors present their new results on the thermal conductivity of molten-flux grown samples of UTe_2 with a heat current directed along the b -axis. Measurements on similar high-quality samples had already been performed by some of the authors (ref 29) and by competitors (ref 30) for the heat current directed along the a -axis, and concluded either to a fully gapped superconducting ground state (ref 29), or to a nodal gap, with point nodes along the b -axis (Ref 30). These new measurements were performed with a heat current directed along the b -axis, despite the small dimension of the molten flux crystal along this axis (0.36mm). Hence, they should be highly relevant to solve this issue.

We thank the Reviewer#2 for the detailed and thorough evaluation of our manuscript. The points raised have helped us critically re-examine our data and strengthen the presentation. Below we address each point raised by the Reviewer.

A pre-requisite for such measurements is to check the validity of the Wiedemann Franz law, comparing electrical and thermal transport. Such data is reported in the supplement (Fig S2), and the authors claim (second to last paragraph of page 2) a “stringent validation” of this test.

However, this claim is surprising. Indeed, in both previous work (ref 29 and 30) on similar high-quality samples, below 4K, the phonon contribution is small enough compared to the electrical one, and inelastic scattering is dominant enough, so that the Lorentz ratio was found to be always below the Lorentz number (or equivalently, estimate from electrical conductivity is higher than the actual thermal conductivity data). In the present work, it is the opposite, even when comparing to the electrical data at 6T which has a higher conductivity than in 0T (above 2K): this is a huge discrepancy with expectations. Moreover, it is quite unfortunate that the data at 6T in figure S2 are displayed over such a small temperature range, as it does not allow to estimate reliably whether extrapolation of the thermal transport data toward zero temperature would match the “electrical estimate”. With the present data, it appears that it would rather fall below by at least 10%, but it requires a very bold extrapolation. Hence, until this first check is secured, it is not possible to judge the reliability of the measurements.

We appreciate the Reviewer’s thoughtful concern. The primary focus of our study is the low-field and low-temperature regime, where very low-energy quasiparticle excitations are probed. In the low-temperature and low-field regime, the thermal signal is substantially larger—quantitatively, more than 50 times greater than that in the high-field, high-temperature region. We note that we measure the thermal resistance, not thermal conductivity. Smaller κ means greater temperature difference for the same power. Furthermore, Samples #A and #B exhibit very similar behavior in this regime. Such a large signal margin provides strong confidence in the data quality underlying our main conclusions. Even if we assume a 10% uncertainty in the application of the Wiedemann–Franz law in the small-signal, high-field region for Sample #A, as suggested by Reviewer #2, this would not significantly affect our main conclusions.

To further enhance the reliability of our measurements, we performed additional experiments on Sample B at higher fields and temperatures (Fig. R1). Measurements in the lowest temperature range were constrained by the limited sample dimensions and the intrinsically large thermal conductivity of the crystal, which produced very small temperature gradients, together with prolonged thermal equilibration associated with the enhanced specific heat. Despite these limitations, the thermal conductivity at 11 T approaches the electronic contribution estimated from the Wiedemann–Franz law within our experimental resolution. Even if the low-temperature data do not perfectly coincide with the estimated electronic value, a simple extrapolation to zero temperature (shown as dotted line) yields a value that is very close. This consistency supports the reliability of our thermal conductivity measurements.

The recovery of the Wiedemann–Franz law in the low-temperature region under high magnetic field is also consistent with the study by S. Suetsugu *et al.* (Ref. [29]). At 11 T in the higher-temperature regime, our measured thermal conductivity slightly exceeds the value expected from the electronic contribution estimated via the Wiedemann–Franz law, indicating the presence of an additional phonon contribution. While the quantitative behavior in this higher-temperature regime may differ somewhat from Ref. [29], such variations are expected, since phonon thermal conductivity is highly sensitive to sample-

dependent factors such as dimensions and surface conditions. Despite these differences, the phonon contribution must vanish in the zero-temperature limit. Therefore, the recovery of the Wiedemann–Franz law at low temperature provides an important internal consistency check and supports the reliability of our data.

Since the Reviewer refers to the results of Ref. [30] as a benchmark, we also note that the results reported by I. M. Hayes *et al.* (Ref. [30]) show that their thermal conductivity data fall significantly below the electronic thermal conductivity estimated from the Wiedemann–Franz law below 4 K, but only for their best quality sample. This has been interpreted, as noted by Reviewer #2, as evidence for a very small phonon contribution. However, this interpretation appears difficult to reconcile with the overall temperature dependence of their data. Above 4 K, their measured thermal conductivity substantially exceeds the electronic contribution, indicating that heat transport is dominated by phonons in that temperature range. For example, at 6 K, the phonon contribution is approximately twice as large as the electronic contribution. Therefore, assuming that the phonon thermal conductivity abruptly vanishes below 4 K does not provide a self-consistent explanation. If the electronic thermal conductivity were to increase sufficiently below 4 K to overcome such a large phonon contribution, the total thermal conductivity would be expected to increase accordingly. Instead, their data exhibit a decrease in this temperature range. The low-temperature data is difficult to reconcile with the Wiedemann–Franz law in the zero-temperature limit.

Finally, we agree that the phrase “stringent validation” was too strong in the context of the present results, and we have revised the manuscript accordingly to clarify that the Wiedemann–Franz law serves as a consistency check for the accuracy of our thermal conductivity measurements.

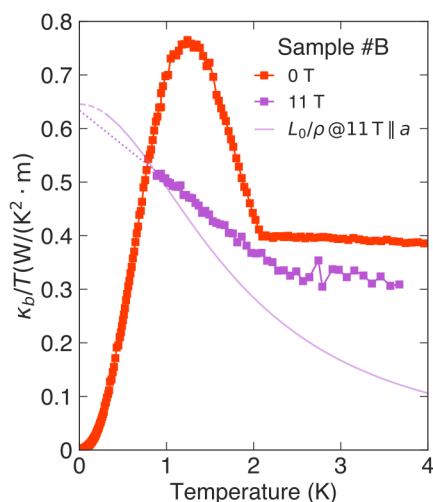


Fig. R 1 Checking Wiedemann-Franz law for sample B

Next, the authors present and discuss the temperature dependence of the thermal

transport at very low temperatures. They claim it to be “pivotal” regarding the presence or absence of line nodes. This could be true indeed, however, comparison to the results obtained in previous work for the heat current J_Q along the a -axis would have been very useful: in Ref 29, it was claimed that for J_Q/a , most of the thermal conductivity was arising from a phonon contribution, mainly because it was little sample (impurity content) dependent. Isn't it the same for these measurements below 0.1 K? It would be helpful to quantify the phonon contribution for these measurements.

We respectfully clarify that our discussion concerns point nodes rather than line nodes. According to Ref. [29], the a -axis thermal conductivity exhibits little dependence on impurity content below approximately 0.2 K, suggesting the absence of quasiparticle excitations and a dominant phonon contribution in that temperature range. In contrast, in our b -axis measurements, the temperature range in which such impurity independence is limited to below approximately 0.1 K. Above this temperature, thermally excited quasiparticles along the b axis contribute significantly to heat transport, indicating that quasiparticle excitations are more active along the b axis than along the a axis. This behavior is consistent with the presence of a very small but finite gap minimum along the b -axis direction. As discussed in our response to Reviewer #1, the anisotropic magnetic-field dependence of the thermal conductivity further supports the conclusion that quasiparticle heat transport dominates in the low-temperature regime (0.1–0.3 K). Furthermore, as discussed in the manuscript, the field-dependent κ_b/T exhibits pronounced anisotropy between $H \parallel a$ and $H \parallel c$ configurations. Since phonon thermal conductivity is isotropic with respect to the magnetic field direction normal to the heat current, this anisotropy can only arise from the quasiparticle contribution via the directional Doppler shift mechanism, confirming that quasiparticle heat transport dominates in the low-temperature regime (0.1–0.3 K). Furthermore, as shown in Figs. 3a–c, κ_b/T at $H = 0$ crosses κ_b/T at finite fields. This crossing is a hallmark of inelastic quasiparticle scattering and is difficult to explain within a phonon-only picture without invoking complicated scattering processes. Taken together, these observations provide evidence that the measured thermal conductivity at low temperatures is dominated by quasiparticles.

Moreover, as figure 2c shows an elaborate theoretical analysis checking the influence of impurities with and without a small gap, it would be also very helpful to give the order of magnitude of the gap Δ_0 required to get the overall temperature dependence of κ/T below 0.3 K: how does it compare to T_{sc} ? I may have missed it, but I also did not find quantitative values in the supplementary.

Also another comparison which could be useful is the effect of finite impurity scattering rate on a system with point nodes and a small gap: indeed, most heavy fermions are multigap superconductors: would the maximum shown for $\Delta_{min} = 0$ still be present in that case (in a model with point nodes for both the large and the small gap), or would it rather lead to a finite residual value (lowering the “threshold” for the appearance of impurity effects ?) How would this residual value (if any) compare with experimental data?

Throughout the manuscript we set the gap maximum $\Delta/k_B T_c = 2$. The exact value of this ratio does not affect our conclusions. As shown in the supplementary information, the low-temperature thermal conductivity is sensitive to the existence and the magnitude of the minimal gap in the near nodal regions as the quasiparticles exist only in those parts of the Fermi surface. We emphasize that we do not aim to reproduce the temperature dependence of the thermal conductivity. As mentioned above, the crossing point of the thermal conductivity data for $H=0$ and finite fields in Figs. 3a-c clearly indicates the importance of inelastic scattering at finite T . Although the phonon contribution is small at low temperatures, it is also non-negligible. Therefore it is impossible to reliably estimate the residual gap from the temperature dependence of κ/T . It is for that reason that we focus on the extrapolated value to $T=0$, where both the phonon contribution and inelastic scattering effects vanish, and the field dependence provides a direct probe of the gap structure.

In the second half of this question the reviewer suggests a calculation for a multiband system with different gap structure on different Fermi surface sheets. This is quite distinct from the model we consider, which consists of a single Fermi surface, sketched in Fig. S5. While the Reviewer correctly notes that many heavy fermion superconductors have complex multi-sheet Fermi surfaces, this is not the case for UTe_2 . Quantum oscillation measurements (Aoki et al., JPSJ 2022; Eaton et al., Nat. Commun. 2024; Weinberger et al., PRL 2024) show that in this material the Fermi surface consists of two quasi-2D cylindrical sheets (electron- and hole-type) with doubly degenerate cross sections. Our single-band model with a representative cylindrical Fermi surface, see Fig. S5, is sufficient for addressing the key question of whether the field dependence of κ_0/T can distinguish between a true point node ($\Delta_{\min} = 0$) and a pseudo-point node with a finite but small gap minimum ($\Delta_{\min} > 0$) on that Fermi surface. The main qualitative distinction between the two cases, the existence of a threshold field in Fig. 4d, is independent of the details in this case.

The last data set focuses on the field dependence of the thermal conductivity which is indeed well recognized as a powerful tool to probe the gap structure, through the effects (of absence of effects) of the Doppler shift of the excitation spectrum.

Here, the authors have measured these effects for the three main directions ($H//a, b, c$), and then extrapolate the results toward $T=0$. Their main finding, supporting a scenario of a fully gapped superconducting state but with a minimal gap along b is the existence of a threshold for fields along a for the appearance of a Doppler shift of the excitation spectrum. They also claim that the field dependence should be much stronger allow fields if a point node was present (Fig 4d with $\Delta_{\min} = 0$). Regarding the data analysis, there are several points which need to be clarified in order to judge the solidity of this claim.

First, the “threshold” appears weakly established on the “raw data” of figure 4b.

- There is very likely a large uncertainty on the extrapolated values as evidenced by figure 3a, where it can be seen that the (polynomial?) fit used to extrapolate the data has non-

systematic evolution of its coefficients (change of the curvature and even the derivative at $T=0$). So the weak kink pointed by the arrow of figure b is probably buried in the error bars, and both curves for $H//a$ of Fig 4b might well be fitted by straight lines within error bars?

We agree that the extrapolation procedure introduces some uncertainty and that different fitting forms may slightly modify the apparent curvature. Nevertheless, the presence of the anomaly is robust. In the original manuscript, polynomial fits were used to extrapolate the data to $T \rightarrow 0$. To reduce the number of fitting parameters and test the stability of the result, we additionally performed linear fits of κ/T as a function of T^2 (see Fig. R2). As shown in main text of Fig. 2b, the zero-field data exhibit a clear T^2 dependence at low temperatures, which provides a physical justification for adopting a linear fit in T^2 . Importantly, adopting a T^2 fitting form leads to nearly the same magnetic-field dependence of the extrapolated residual term. In other words, the qualitative field evolution of the residual thermal conductivity is insensitive to the choice of fitting function.

Regarding the uncertainty estimation, we find that the dominant contribution arises from the choice of the fitting range. Under applied magnetic fields, the temperature dependence deviates from the ideal T^2 behavior, leading to a slight variation in the effective exponent and hence in the extrapolated value. For this reason, we presented the polynomial fitting results in the original manuscript, as they are more robust against variations in the fitting temperature window.

Crucially, however, the variation of the extrapolated values with fitting range is systematic rather than random. As shown in Fig. R2(c), where error bars represent the variation arising from the choice of fitting temperature range, the threshold behavior remains visible even when accounting for this uncertainty. This stability further strengthens our confidence in the robustness of the threshold behavior.

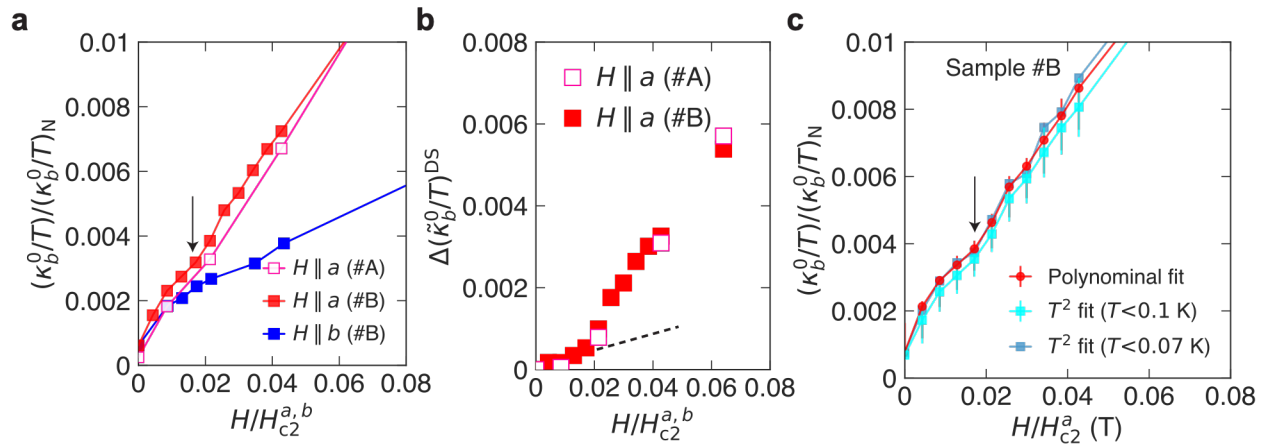


Fig. R2 Reanalysis of the field-dependent residual term using a linear-in- T^2 fit.

(a,b) Same plots as Fig. 4b,c, but obtained using linear fits of κ/T as a function of T^2 .

(c) Comparison of the fitting results for sample B with $H \parallel a$. The error bars represent the variation arising from the choice of fitting temperature range.

- Such a kink or threshold should also appear for $H//c$: at first glance, this is not the case?

Based on the relation $H^*/H_{c2} = a^{-2}(\Delta_{\min}/\Delta_0)^2$, the threshold field for $H \parallel c$ is expected to occur at a comparable value of H/H_{c2} as for $H \parallel a$. However, we did not achieve sufficient field resolution for $H \parallel c$ to resolve a possible threshold. The measurements on Sample #A were performed using a single-axis rotator, and the field steps along the c direction were not optimized to detect such a low-field feature. Despite these limitations, the available data do not contradict this expectation and appear broadly consistent with a similar onset scale within experimental uncertainty (Fig. R3). The slightly larger increase of the residual term for $H \parallel c$ compared with $H \parallel a$ could be related to the quasi-two-dimensional nature of the Fermi surface. Higher-resolution measurements for $H \parallel c$ would be a valuable future test of the pseudo-point-node scenario.

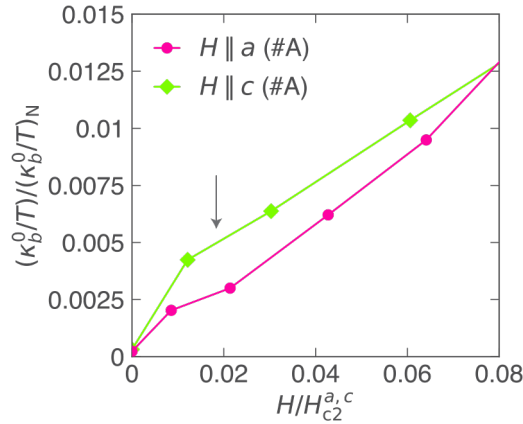


Fig. R 3 Field-dependent residual thermal conductivity for $H||c$. The gray arrow marks the threshold field determined for sample #B with $H||a$, shown here for comparison.

Second the curve 4c might be misleading:

- it subtracts the data for $H//b$ with a rather arbitrary renormalization of the x axis according to the very unusual behavior of $H_{c2}(T)$ for both $H//a$ and $H//b$.

We note that the data shown in Fig. 4c are taken at very low temperatures ($T \ll T_c$), where H_{c2} is essentially temperature-independent for both $H \parallel a$ and $H \parallel b$. In this regime, the normalization by H_{c2} amounts to a simple rescaling by a constant value and does not involve the unusual temperature dependence of $H_{c2}(T)$. The purpose of this normalization is to allow a meaningful comparison of the field response for the two field directions on a common scale. The ratio H/H_{c2} determines both the quasiparticle density and the density of vortices, and therefore is the control parameter for the measurements. The subtraction

in Fig. 4c serves to eliminate small background contributions and to emphasize the salient features arising from the near-nodal structure, as we elaborate below.

- it masks the very rapid increase of the residual term at very low fields in both directions, which the authors do not comment, but which could look like the theoretical effect reported on figure 4d for $\Delta_{\min}=0$ (there is no discussion of comparison of the vertical scales between figures 4b-c and 4d).

First, the increase of the residual term in the low-field region is very small. Even at $H/H_{c2} \sim 0.08$, it remains below approximately 1–2% of the normal-state value. Such a small increase is not unusual and can occur even in conventional superconductors; for example, similar behavior has been reported for V_3Si (E. Boaknin *et al.*, PRL 90, 117003 (2003)).

The referee's main concern appears to be that the initial increase of the residual term exhibits an upward field dependence. However, this behavior is entirely consistent with a finite gap minimum scenario for the following reasons. Once vortices enter a superconductor, the thermal conductivity generally begins to increase with magnetic field. Even in conventional fully gapped s-wave superconductors, nearly field-independent thermal conductivity is observed only in the immediate vicinity of H_{c1} . In the case of UTe_2 , H_{c1} is extremely small, on the order of 2–7 mT for all three crystallographic axes (K. Ishihara *et al.*, PRR 5, L022002 (2023)), which is much lower than the typical residual field present in standard superconducting magnets. In our measurements, the smallest applied field was 50 mT, well above H_{c1} . Therefore, a modest increase in thermal conductivity at low fields is expected.

Furthermore, if this upward feature were attributed to a nodal gap structure, the interpretation would be less straightforward. The upward feature is most pronounced for $H \parallel b$, yet this configuration is not expected to strongly excite nodal quasiparticles with momentum along the b axis. Moreover, this upward feature is present for all field directions, which is also difficult to explain by assuming point nodes in a specific direction. In contrast, in a pseudo-point-node scenario with a small but finite gap minimum along the b axis, the Doppler shift can suppress this minimal gap for all field directions, leading to a gradual increase in quasiparticle excitations and thermal conductivity. This interpretation is therefore more consistent with the observed isotropic low-field increase.

As discussed in the manuscript and in our response above, there are contributions to the thermal conductivity that are not accounted for in the quasiclassical approximation. The existence of discrete core states, whose overlap grows with increasing field, is one possibility. The field dependence of the vortex core size may also play a role. However, it is important to emphasize that even without these considerations, we expect differences between Figs. 4c and 4d. The Doppler shift method yields exponentially small thermal conductivity below the threshold field because the local values of $\kappa(r)$ nearly vanish near the edges of the vortex unit cell. The exact manifold where $\kappa(r)$ vanishes depends on the vortex lattice structure. Recall that the superfluid velocity vanishes only at high-symmetry points in such lattices. For realistic arrangements, there likely exist small regions of the vortex unit cell where the local superfluid velocity remains finite and sufficient to generate

non-zero thermal conductivity. An even more important reason is already described on p. 6 of the manuscript: the kernel of the correlation function for the thermal current response has a finite range. Therefore, even when the local value of the superfluid velocity is small, there is a finite probability of carrying heat current across this region. This effect, albeit small, lies completely outside the semiclassical approximation.

Therefore the subtraction in Fig. 4c serves to eliminate these small effects and emphasize the salient features due to near-nodal structure.

Last, regarding the final discussion, I could not understand what is meant by the necessity of a “certain degree of surface corrugation and pairing interaction strength” to have such gap minimums? All theoretical models predict a quite strong k -dependent orbital character of the electronic states on the Fermi sheets, which should be enough to induce “multigap” superconductivity? Could the authors precise what they mean here (last paragraph of page 6)?

We thank the referee for pointing out that this paragraph was unclear. What we intended to convey is the following: a purely cylindrical Fermi surface with an isotropic pairing interaction would yield a gap that is uniform around the Fermi surface. To produce the observed deep gap minimum along the b axis, one needs either a sufficient degree of corrugation of the Fermi surface in the b direction, or a momentum-dependent pairing interaction that suppresses the gap near the b axis, or a combination of both. We have revised this paragraph in the manuscript to clarify this point.

I also do not understand the need to discuss in the conclusion possible admixtures of non-unitary states at zero field, which have now long been dismissed for this system by other measurements?

We acknowledge that many recent studies favor non-chiral superconducting states in UTe_2 . However, proposals involving time-reversal symmetry breaking and non-unitary mixing still exist in the literature (e.g., Refs. [21, 47]). An important implication of our results is that non-unitary mixing—which would split point nodes rather than open a gap—is incompatible with our observation of a finite gap minimum. We believe briefly addressing this point in the conclusion is justified, as it helps clarify the constraints our findings place on proposed superconducting states in UTe_2 .

As a summary, despite a very challenging and long-awaited experimental work, I cannot recommend publication of the paper in its present state: presently, it is difficult to be convinced that the gap structure can be revealed by this new set of experiments. Several crucial points need to be checked (starting with the Wiedemann-Franz law) in order to strengthen the experimental analysis. Comparison with the theoretical analysis would also strongly benefit from more quantitative comparisons.

We appreciate the Reviewer's recognition of the challenging nature of these experiments. We believe that the additional measurements and analyses presented in this revision—including the improved verification of the Wiedemann–Franz law with additional data on Sample B (Fig. R1), the reanalysis of the field-dependent residual term with different fitting procedures (Fig. R2), and the more detailed comparison with theoretical predictions—substantially address the concerns raised. We hope that these revisions demonstrate the reliability of our measurements and strengthen the conclusions of our study.

Minor point: Initially quantum oscillation measurements were performed and published by D Aoki et al (JPSJ 2022) before reference 41 and 12, and without any claim of the existence of a 3D Fermi surface. This should be corrected in the last paragraph, first column of page 6.

We thank the Reviewer for pointing this out. We have revised the text to properly acknowledge the initial quantum oscillation measurements by D. Aoki et al. (JPSJ 2022) and have corrected the description accordingly.

[Reply to Reviewer #3]

Since the discovery of unconventional superconductivity in UTe_2 in 2018, few topics in condensed matter physics have been as hotly debated as the nature and symmetry of its superconducting gap. In this manuscript by Hosoi *et al.*, the authors present their latest measurements of b -axis thermal conductivity in UTe_2 down to dilution fridge temperatures and as a function of magnetic field (and field direction), from which they conclude that the superconducting gap structure has a “pseudo-point-nodal” structure: an energy gap that drops to a small value at a minimum along k_y but remains nonetheless finite. This contrasts with some other recent thermal conductivity work claiming true point nodes [Hayes *et al.*, Phys. Rev. X **15**, 021029 (2025)] and will certainly elicit significant interest from the community.

I think the data presented here are sufficiently novel (given the b -axis results), the data appears to be reliable, and their overall interpretation seems reasonable, or at the very least plausible, and will certainly be of interest to the readers of Nature Communications. I do think the manuscript should eventually be published in this journal, however there are several issues I think should be addressed first.

We thank Reviewer #3 for the positive assessment of our work and for the constructive suggestions, which have helped us strengthen the manuscript. Below we address each point raised by the Reviewer.

The primary issue I have concerns the zero-field thermal conductivity data (as presented in Figure 2) and its discussion. The authors’ main argument here is that a finite gap would better describe the data than would a true point node, because the point node (but not the

finite gap) would produce a low temperature peak that is not visible in their data. The problem is that the evidence is not so clearly presented; for one, it is somewhat difficult to see the low temperature behavior in Figure 2b, given the scale and the point size. It would be good to zoom in below $T=0.1$ K (either in the main panel or just an inset) to better show the reader this important region, to make it more clear whether or not a small bump or residual linear intercept can truly be ruled out; at present it's too hard to see at this scale.

We have added the low temperature part of thermal conductivity data for clarity (the inset of Fig. 2b).

One minor note is that the caption of Figure 2 refers to labels a, b, and c even though such labels are absent from the figure itself, although I'm sure the reader can guess.

We thank the Reviewer for pointing out this mistake. We have revised the manuscript accordingly.

In the supplementary information section regarding calculation of thermal conductivity for different scenarios, as shown in Figure S4, the top right panel shows the dependence on scattering phase shift for a particular scattering rate. Here it appears that $c=0.2$ and $c=0.15$ indeed have prominent low temperature peaks that were not observed in the measured data – however, for $c=0.1$, and presumably some range of similar scattering rates and small-but-finite c values, this peak is hardly visible. I would argue that their zero-field data presented in Figure 2b might not be inconsistent with such a scenario (true point node but small non-unitary phase shift c), so it would be good if the authors could explain and/or better show why this is ruled out. It is true that the field-dependence data which follows provides a more convincing argument, so the authors may wish to make a less conclusive statement for the zero-field data (for example, pointing out that the pseudo-point-node scenario fits well but that true point nodes with small but finite scattering phase shifts could also still be compatible, if that is in fact the case) but then emphasize that the field dependence data provides confirmation.

We sincerely thank Reviewer for the insightful comments. As correctly pointed out, the zero-field data by itself does not entirely exclude a true point-node scenario, although such a possibility is restricted to a limited parameter regime. Accordingly, we have revised the discussion of the zero-field data to present a more cautious and balanced interpretation. As the Reviewer suggests, we now emphasize that while the zero-field data is consistent with the pseudo-point-node scenario, the field-dependent data—in particular the threshold field kink observed in κ_b^0/T for $H \parallel a$ —provides the more definitive evidence for a finite minimal gap.

Figure S4 and its caption appear to have some labelling issues. Firstly, the caption says that true point nodes are in the left column and finite gap in the right column, but the figure itself has that reversed. Also, the labelling for the curves in each of the two bottom panels

appears to be reversed: the unitary ($c=0$) and finite c curves seem to be swapped if I'm not mistaken.

We thank the Reviewer for catching these labelling errors. We have corrected the column labels in the caption of Figure S4 and the curve assignments in the bottom panels accordingly.

In short, I think the paper is worthy of publication in Nature Communications and should indeed eventually be published there, but these weak points regarding claims from the zero-field data alone should first be addressed.

We appreciate the Reviewer's support for publication and hope that the revisions adequately address the concerns raised.

First of all, we thank the Editor and the reviewers for their careful and constructive evaluation of our manuscript again. We are grateful that Reviewers 1 and 3 expressed strong support for publication. We also thank Reviewer 2 for recommending publication while requesting further clarifications, which have helped us to improve the manuscript. We have carefully addressed all remaining concerns raised by Reviewer 2 in the present revision.

Reviewer 2's comments shown in blue text:

In this resubmission, the authors tried to answer the different criticism addressed by the 3 referees, clarifying some of the statements and adding also new measurements under field presented in the supplementary data (Fig S2).

We thank the reviewer for the careful reading of our manuscript and for the constructive comments. We have carefully reconsidered the points raised and clarify them below as follows.

Reviewer Comment 1

Focusing first on the point on the Wiedemann Franz law, for which the authors took the supplementary data at 11T, the authors argue that at low temperature, under field, the agreement between the thermal and electric measurement is verified within 10%. This is fine. However, there is some misunderstanding and I believe still a problem for the “high temperature data (between 2 and 4K). I did not say that the phonon contribution was negligible in this range, but that the electronic contribution was large enough in the best samples of ref [29] so that the Lorentz number was below the Lorentz ratio: this is still the opposite in the present work, meaning probably that the electronic contribution for $J//b$ -axis remains smaller than the phonon contribution. I assume that in the normal state, the phonon mean free path is governed in UTe₂ by electron-phonon interactions, and should be weakly sample dependent (sample geometry and surface state might indeed matter, but only deep in the superconducting state and in very low field). However, do the authors have an explanation for why the measurement of the thermal conductivity at 11T is below that in zero field between 2 and 4 K? The resistivity is lower at 11T than in zero field above 2K, so I would expect a larger electronic contribution under field, and a very similar phonon contribution (there is no reason for a large field-sensitivity of the phonon contribution in the normal state?). Does it mean that magnetic contribution also plays a role in the thermal transport of UTe₂? This might need still further investigations.

Our Response

We thank the reviewer for raising this interesting point. We first note that the high residual resistance ratio (RRR) of our sample makes it unlikely that impurities or cracks are responsible

for this behavior. In this temperature range, the observed T^2 dependence of the resistivity is consistent with Fermi-liquid-like inelastic quasiparticle scattering, likely influenced by magnetic fluctuations. The observed reduction in electrical resistivity under magnetic field suggests suppression of magnetic fluctuations and the associated inelastic scattering processes. One possible interpretation of the simultaneous reduction in thermal conductivity is that the phonon contribution is reduced through a field-dependent modification of phonon scattering processes. We cannot, however, exclude that an additional field-dependent magnetic contribution to the thermal transport may also play a role. At present, the detailed mechanism remains unresolved and requires further investigation. We emphasize that this issue concerns finite-temperature scattering processes and does not affect our main conclusions regarding the superconducting gap structure, which are based on the field dependence of the residual thermal conductivity in the limit $T \rightarrow 0$.

We have clarified this point in the revised SI.

Reviewer Comment 2

Regarding the question on the temperature dependence of the thermal conductivity at low temperatures, also raised by referee 1, in the paper, the authors now discuss a substantial quasiparticle contribution above 0.1K, which is fine indeed (indeed more appropriate than the “dominant quasiparticle contribution” from the rebuttal letter!). However, the main point of worry was not really addressed, and was probably not stated clearly enough in my questions: I asked for a quantitative estimate of the maximum predicted theoretically for point nodes because the authors claimed, and still claim in this new version (top of page 4), that such a maximum should be detectable. However, the theoretical minimum is at $0.05 T_c$ hence 0.1K, where the thermal conductivity is sample independent hence totally governed by phonons, and the maximum would be way below the minimum temperature of the measurements (for the chosen parameters of Fig 2c). Hence, if the authors can claim that the peak should be detectable, it means that they did make a quantitative estimate of the theoretical minimum and they should present it. Otherwise, wouldn't it be better to just say like in [29] that presently, with the large phonon contribution below 0.1K, it is not possible to make any conclusion from the T-dependence and indeed rely entirely on the field dependence?

Our Response

We thank the reviewer for this important clarification.

Based on the parameters used here, the temperature at which the thermal conductivity is expected to peak in the presence of point nodes lies well below our lowest measurement temperature. However, the temperature at which the thermal conductivity should start to increase upon cooling

is expected to be around 0.1 K, which falls within our measurement range. The absence of such an upturn in our data is consistent with the absence of point nodes. However, since this argument depends on the choice of parameters, we cannot draw a definitive conclusion, and we agree with the reviewer's suggestion to rely primarily on the analysis of the field dependence. We have clearly stated these points in the revised manuscript.

Reviewer Comment 3

Regarding these measurements under field (Fig 3) , the authors made corrections on page 4 explaining the change of behavior under field and notably the crossing of the field data with the $H=0$ curve. The following point is not important for the conclusions of the authors, and of course, I completely agree that the electronic contribution increases under field due to the increase of thermal excitations with the doppler effect (much stronger for the perpendicular directions than for the $H//b$ configuration). However, isn't the crossing with the $H=0$ curve also the result of a much-suppressed phonon contribution (expected to go roughly as T^3 below 0.3K in zero field), due to the restauration of electron-phonon scattering with the presence of more electronic thermal excitations? I do not really understand why it demonstrates significant inelastic quasiparticle scattering? But again, this is a minor point, probably just misunderstanding.

Our Response

We thank the reviewer for this comment and for recognizing that this point is not central to our conclusions.

We agree that the crossing behavior may reflect a combination of an increased quasiparticle contribution under field and a suppression of the phonon contribution due to enhanced electron–phonon scattering in the presence of thermally excited quasiparticles. Such effects can indeed influence the thermal conductivity at finite temperatures.

However, we emphasize that electron–phonon scattering is an inelastic process and becomes negligible in the limit $T \rightarrow 0$. The central quantity in our analysis is the residual thermal conductivity κ_0/T , which is insensitive to such inelastic effects.

Therefore, while the detailed temperature dependence and crossing behavior may involve multiple mechanisms, they do not affect the conclusions drawn from the zero-temperature limit. We have clarified this point in the revised manuscript.

Reviewer Comment 4

Naturally, the key issue is the field dependence of the extrapolated residual term, for which the authors claim to observe a threshold (Fig 4), as predicted for a weak minimum. The authors have

made new analysis presented in supplementary Figure R2, where they could demonstrate that different fitting procedures reproduced the same (weak) feature at around $0.02 H/H_{c2}(0)$. However, figure R2c also show precisely that a linear fit of the data would indeed lie within the error bars. By contrast, on Fig 4b or R2b, when the thermal conductivity for $H//b$ is subtracted from the data for $H//a$, the threshold appears very clearly. Hence, the claim for the existence of the threshold would be much stronger if the authors could justify better why subtracting the data for $H//b$ is justified. In the paper, it is only said that the data for $H//b$ represents the field dependence without doppler shift, but it does not explain where it originates from and a fortiori, whether it should be similar for both field orientations.

In the rebuttal letter, the authors also explain that a small increase of κ (whatever the field direction) under weak field is expected even in conventional fully gapped superconductors once H is larger than H_{c1} ? However, in the same system, for $j_Q//a$, the same authors measured a completely flat κ far beyond H_{c1} ? Then why would it be so different below the threshold for $j_Q//b$? The same question holds also for theoretical explanations proposed (only in the rebuttal letter), through effects arising beyond the quasiclassical approximation: shouldn't all of these effects (relying on the contribution of core states, or variation of the local superfluid velocity in the vortex lattice unit cell) be very much sensitive to the relative orientation of field and heat current? They could then hardly justify the subtraction presented in Fig 4c?

Our Response

We thank the reviewer for this detailed and important comment. We first emphasize that our main conclusion does not rely on the subtraction procedure. In the raw data, a clear kink-like feature is observed in κ_b for $\mathbf{H} \parallel a$ near $H^*/H_{c2} \approx 0.015$, whereas no corresponding anomaly is observed for $\mathbf{H} \parallel b$. This anisotropic behavior is already evident without subtraction and supports our conclusion regarding pseudo point nodes.

The reviewer raises an important question concerning the justification of the subtraction procedure. We agree that the background field dependence need not be strictly identical for the two field orientations, since effects such as Fermi-surface anisotropy, field and temperature dependence of the vortex-core size, and details of the vortex-state structure may all contribute. The exact weak field dependence of the residual thermal conductivity far below H_{c2} in a fully gapped superconductor is therefore itself a difficult problem, and we are not aware of a fully consistent theoretical treatment that accounts for all of these effects on equal footing.

Nevertheless, below the threshold field, where the Doppler energy remains smaller than the gap minimum and nodal quasiparticles are not yet significantly activated, the dominant field response is expected to arise mainly from generic mixed-state effects rather than orientation-selective nodal excitations. In this regime, it is therefore reasonable as a first approximation to expect a similar

smooth low-field background evolution for the two field orientations. Under this assumption, subtracting the $H \parallel b$ data provides a useful phenomenological way to suppress the common background contribution and highlight the anisotropic onset feature associated with nodal quasiparticles more clearly. We stress again that this subtraction is used only as a visualization tool and not as the basis of our conclusion.

By contrast, the field response associated with nodal quasiparticles is expected to be more robust, both theoretically and experimentally, across different unconventional superconductors. We have revised the manuscript and SI to clarify both the purpose and the limitations of the subtraction procedure.

Final remark

Last the authors have (from my point of view), well clarified the sentence on the corrugation of the Fermi surface and the possible gap minimum. The question of citing non unitary states in the conclusion is a minor point, and the authors corrected also the reference to the first quantum oscillation measurements.

Again, the measurements are really important and should be published. But I still believe, despite the nice improvements done by the authors, that some key points on the data above 2K and on the analysis of the field dependence of the residual can and should be improved before publication: it should provide more convincing evidence or clearer status of the present state of what can be said on this hard issue of the gap nodes in UTe₂.

Our Response

We thank the reviewer for the positive assessment of the importance of our measurements.

Regarding the data above 2 K, we have provided a physical interpretation for the observed reduction in thermal conductivity under field, attributing it to an enhancement of electron–phonon scattering, while acknowledging that the detailed mechanism remains an open question for future work. We emphasize that this issue concerns finite-temperature scattering processes and does not affect our conclusions based on the residual thermal conductivity in the limit $T \rightarrow 0$.

Regarding the field dependence of the residual term, we have demonstrated that the kink structure in κ_b is clearly observed in the raw data for $H \parallel a$ but not for $H \parallel b$, establishing our conclusion regarding quasi-point nodes independently of the subtraction procedure. We have also provided a clearer justification for the subtraction procedure and explicitly stated its limitations.

In addition to the above, for a time-reversal-symmetry-breaking order parameter, a state of the form $A_u + i\alpha B_{3u}$ with α slightly larger than 1 could, in principle, generate pseudo-point nodes

distributed around the b axis. However, the absence of time-reversal-symmetry breaking has already been strongly suggested experimentally, for example by the absence of a split superconducting transition in uniaxial specific-heat measurements. Moreover, realizing a state with $\alpha \approx 1$ would require fine tuning and is therefore unlikely. For these reasons, we have removed the discussion of such a time-reversal-symmetry-breaking gap function from the manuscript.

We believe that these revisions have strengthened the manuscript and improved the clarity of our conclusions.

Response to Reviewer #2 (Remarks to the Author):

In this resubmission, the authors have tried to answer a few points requiring further clarifications. There are two issues which remain unanswered:

- The first is the high temperature behaviour of κ/T under field, presented in the supplementary for the discussion of the Wiedemann-Franz law. The authors have added a paragraph in the supplement explicitly pointing to this unsolved issue (κ/T drops under field when it was expected to be larger than in zero field due to an increased electrical conductivity, suggesting a field suppression of the phonon contribution, which is not so easily understood). As correctly stated by the authors, this should not influence the analysis of the low temperature data, and will remain a point to be checked and investigated by future experimental work.
- The second is why there would be a background contribution of κ/T (for $T \rightarrow 0$) from the mixed state for heat current along b , when it is absent for heat current along a ? This point was not really discussed or answered by the authors, and personally, I remain very skeptical that figure 4c is meaningful. The presentation of the theoretical arguments for this subtraction has been improved, and for the experimental arguments, the authors insist even more that the raw data already present the “threshold” with field distinguishing a finite small gap from point nodes. I do not think that this point can be further improved with the present data, it will probably remain yet another challenge for future experiments to test if they can reproduce it.

Regarding all the other points the authors have answered very satisfactorily all the remarks:

- For the analysis of the temperature dependence, owing to the dominant phonon contribution below 0.1K, it is possible to state that the results are compatible with a gap minimum and do not show a small maximum that would be the hallmark of a point node, but not to consider the absence of a peak as strong evidence for the absence of nodes. This is very clearly written in this last version
- On the (minor point) of the crossing of the zero-field data of κ/T with field measurements (between 1T and 6T) between 0.15K and 0.3K, the authors have also clarified that it could also arise from the decrease of the phonon contribution due to enhanced scattering on electronic excitations.
- The authors have also removed in the conclusion the remark on non-unitary state, so that it remains focused on the main result of the paper.

Hence, even though not all questions are solved (but this is a general issue with all works on UTe₂ !), I do support publication of this important work in Nature Communication in its present state.

We thank the reviewer for carefully reading our revised manuscript and for supporting publication in its present state. As acknowledged by the reviewer, the revised manuscript clarifies the limitations of the present data and the remaining open issues. We have therefore retained the scientific content of the revised manuscript, apart from the editorial and formatting revisions requested by the journal.