nature portfolio

Peer Review File

MA: On the robustness of topological gap detection via transport & Reply to MA: "Interferometric single-shot parity measurement in InAs–Al hybrid devices"

This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version, for MA: On the robustness of topological gap detection via transport, followed by reviewer reports in order by version, followed by all author rebuttals in order by version, for Reply to MA: "Interferometric single-shot parity measurement in InAs–Al hybrid devices".

MA: On the robustness of topological gap detection via transport

Corresponding Author: Dr Henry Legg

Version 0:

Reviewer comments:

Referee #1

(Remarks to the Author)
I have reviewed the studied the Matters Arising from H. Legg and the Reply from Microsoft.

Is the criticism valid?
The criticisms do not deliver on their promise of calling into questions the core findings of Nature 638, 651–655 (2025). I will summarize my views briefly below.

(2) Is the criticism likely to be of interest to non-specialist readers?
The scientific content is not of interest. The criticism will be highly publicized and discussed on X, YouTube, and the like, and in this sense would be of high interest.

(3) Could it be stated more concisely?
Yes. A paragraph explaining the issues raised in Fig. 3 would be sufficient.

A short summary on my views of the criticisms is below. The criticisms focus on the transport data and TGP. Some of the technical issues raised by Legg are right, and some are wrong. Overall they do not undermine the main novelty of the work, which moves beyond transport signatures by performing parity readout. The work generates strong empirical evidence that there is a coherent, low-energy state with a parity degree of freedom. The field knows from experience that the transport data is difficult to interpret and will not be by itself conclusive.

Comment #1 essentially reiterates the prior observation that TGP outcomes depend on windowing. This is an unsurprising result that follows form how the protocol normalizes its data, and in general we should not be surprised that changing meta-parameters can change the results. For the current work, the TGP is used to identify candidate regions for parity readout. It does not cause a problem that altering meta-parameters removes candidate regions. Nothing of interest is raised here.

Comment #2, in contrast, does raise a problem of interest. It seems that a coding error made the authors miss several regions that would have passed TGP. This caused them to miss an opportunity to more broadly correlate parity readout with the TGP. Legg has found a mistake here and I am glad he points it out. This point is of interest, but does not undermine the study.

Comment #3 again deals with identifying candidate regions using the TGP. The only part that I see as definitely valid is that Microsoft claimed in their reply that there is no evidence of particle-hole symmetry breaking, and it is completely unclear what this statement could have meant. They are correct that antisymmetric conductance is allowed in a three-terminal device without breaking particle-hole symmetry, as has been extensively studied. They also correctly point out that Andreev reflection can lead to sub-gap local conductance, and voltage-divider effects can do the same. Given the measured conductances, which are in-between deep tunneling and the open regime, these effects are likely present. The data for Device A1 looks promising, A2 is less convincing, and B1 is in between. It would be interesting to study and debate these points in more detail, but ultimately the whole reason to move beyond arguments over transport data is that they do not tend to be conclusive.

Referee #2

(Remarks to the Author)
This Matters Arising raises substantive concerns regarding the methodology and interpretation of results in the original article by Microsoft Quantum. Specifically, it questions the reliability of the Topological Gap Protocol (TGP) used to identify gapped regions in proximitised nanowires, and presents compelling arguments suggesting that the regions where parity readout was performed may, in fact, be gapless and highly disordered—depending on the analysis parameters. These are not minor technicalities but go to the heart of the experimental validation of the parity readout.

The authors of the original article have submitted a detailed reply addressing several technical points and defending the validity of their main claims. However, in my view, the response does not fully resolve the core issues raised in the comment—particularly those concerning the transparency and reproducibility of the experimental methodology, and most importantly, the inconsistent TGP outcomes and their implications for the interpretation of the parity readout experiments.

The Microsoft Quantum article has generated significant attention and debate within the condensed matter and quantum computing communities. The issues raised in the comment are of broad relevance—not only for this specific experiment, but also for the interpretation of parity measurements and the future use of the TGP protocol in Microsoft's experimental program. I strongly recommend that this Matters Arising be published alongside the authors' reply. The comment raises valid and unresolved concerns that merit open discussion. Given the high profile and potential implications of the original work, it is essential that the scientific community has access to both the critique and the detailed author's response in order to evaluate the robustness of the claims and to treat the original findings with appropriate caution until further clarification is provided.

Referee #3

(Remarks to the Author)
The Matters Arising submission by Legg raises technical concerns regarding the Topological Gap Protocol (TGP) implemented in Supplementary Information (S4.3) of the original Nature paper (Microsoft Quantum, Nature 638, 651–655 (2025)). The author identifies potential inconsistencies in TGP outcomes—notably sensitivity to data parameters (e.g., magnetic field ranges, cutter pairs), coding errors affecting gap/gapless classifications, and unaddressed disorder features in conductance data.

I commend the author for his rigorous scrutiny of methodological details. His insistence on examining raw conductance data and probing TGP's parameter dependencies exemplifies the essential scientific practice of independent verification. However, in my view, Matters Arising should primarily address the central discovery/conclusion of a recently published paper, rather than focusing on points confined to Supplementary Information, Zenodo data, or review materials. The Microsoft Nature paper's core conclusion is the demonstration of single-shot interferometric fermion parity measurement, evidenced by flux-dependent bimodal random telegraph signals (RTS) in quantum capacitance. The author's critique primarily targets the TGP methodology—which supports the claim of topological superconductivity but is peripheral to the parity readout demonstration. Consequently, I do not recommend publication as a Matters Arising article with only these comments. These comments are more appropriately directed at the PRB paper (Phys. Rev. B 107, 245423 (2023)).

Additional comments:
1) The author think the nature paper relies the TGP to prove there is a superconducting gap. But the author didn't argue how the quantum capacitance readout work without if the wire is gapless. Is the model or the measurement wrong?
2) To substantiate alternative explanations (e.g., "fine-tuned mesoscopic disorder effects"), the author should provide detailed experimental or simulated validation—such as modeling a fine-tuned disordered system replicating the Nature paper's results.
3) When challenging the main discovery, the author must address the entirety of relevant evidence. The Nature paper's Supplementary Information and related publications establish a proof-chain; critiques should engage holistically. For instance, while noting "highly disordered" gapless conductance data, the author omits discussion of the symmetry around zero energy—a very common feature to see in superconducting gap in both tunneling regime or AR enhanced regime.
4) Greater attention to experimental nuances is needed: distinguishing tunneling conductance from enhanced Andreev reflection conductance, and addressing transport asymmetry of particle-hole partners (rather than particle-hole asymmetry itself).

I reiterate that the author's meticulous analysis provides valuable feedback for the Microsoft Nature authors to consider—potentially improving methodology, corrections, or evaluation (e.g., the pixel-level data cropping issue).

Version 1:

Reviewer comments:

Referee #1

(Remarks to the Author)
The author has replied extensively to my comments.

Unfortunately my fundamental view has not changed. Overall the criticisms are not of scientific interest.

I appreciate the value of this activity, and commend the author for taking the time to do this.

Certainly a mistake has been found which increases the candidate topological regions in Device B. As was pointed out in the comment, I wish that this had been known at the time of publication as it would have given more parameter space to explore.

The symmetry analysis of transport data is not compelling.

Referee #2

(Remarks to the Author)
I have reviewed the revised Matters Arising by Legg, the authors' Reply, and the associated correspondence. After this additional round of refereeing, it is clear that the central focus of the Matters Arising is on the transport data and the Topological Gap Protocol (TGP) used to tune up the devices into a purported topological superconducting phase. The critique does not directly address the random telegraph signal (RTS) capacitance measurement itself, but rather challenges the foundational assumptions and methodology used to select the operating regime in which the RTS was interpreted as a parity readout of a topological superconductor. According to the Matters Arising, if the TGP is unreliable or applied inconsistently, the interpretation of the RTS as a topological parity signal is substantially weakened, though the critique stops short of providing a definitive alternative explanation.

The authors are careful in their rebuttal, stating: "We show that the issues raised do not undermine our conclusions. The observed parity-switching signals are robust and incompatible with the 'gapless' scenario suggested by the author." They argue that the parity readout data itself is confirmation of a gapped system, as a gapless wire would wash out the clear interferometric signal, a point supported by theoretical models in their supplementary information (section S2.7). However, this defense relies on interpreting the RTS signal as a readout of a gapped system—an interpretation that the Matters Arising argues is predicated on the very TGP data now in question. This creates a circularity: the RTS is used to validate the TGP's conclusion, while the TGP was used to justify the RTS's interpretation.

The ball is now in the editors' court to decide the best way forward—whether that's publishing this Matters Arising and the Reply, or finding another suitable way to alert the community. From my perspective, I feel that simply posting a corrected TGP map on Zenodo doesn't fully resolve the situation. The core of the critique, in my view, is that these issues are quite fundamental; they call the original interpretation into question, and I believe the community needs to be aware of that context.

I would strongly recommend that, at a minimum, the authors be required to publish additional data—such as specific examples of parity readout performed in regions that do not pass the TGP. This would serve as the crucial experimental evidence to substantiate their claim that "a gapless wire would wash out the clear interferometric signal." Importantly, this would also help addressing concerns about reproducibility and the selectivity of the reported results, and would provide a more transparent basis for evaluating the robustness of the parity readout claim.

In summary, while the Matters Arising does not dispute the existence of the RTS signal, it compellingly argues that the interpretation of that signal rests on an unvalidated and potentially flawed transport-based TGP. The community deserves to see this critique and the authors' response, in the form the editors deem most appropriate, but the scientific record would be best served by additional experimental transparency—especially regarding non-TGP regions.

Referee #3

(Remarks to the Author)
I have reviewed the revised Matters Arising and the authors' response. While the revision provides additional detail, my position remains unchanged: the critique does not meet the bar for publication in Nature. The core issue is its failure to provide a compelling, self-consistent alternative explanation for the central finding—the flux-periodic random telegraph signal. Merely suggesting possibilities like disorder or quantum dots (which would be surprising on a micron-scale loop) is insufficient without experimental or simulated validation. Speculative questioning, which neither confirms nor definitively refutes the original claim, does not constitute a substantive challenge.

Response to Referees: Matters Arising
Nature 638, 651-655 (2025)

H. F. Legg, *University of St Andrews, Scotland*

I thank the referees for their detailed feedback on my Matters Arising. I am grateful for their constructive comments, including Referee 1's acknowledgement that a "problem of interest" was found, Referee 2's view that I raise "substantive concerns" that are "not minor technicalities but go to the heart of the experimental validation", and Referee 3's commendation that my Matters Arising represents "the essential scientific practice of independent verification".

I have now reformatted my comment to meet the space constraints of a Matters Arising, with certain sections moved to Supplemental Material and extended data figures.

In response to Referee 1 and Referee 3, as well as editorial suggestions, I have clarified that the Matters Arising focuses on the transport data used to tune up the devices into a purported (topological) superconducting phase, rather than directly on the random telegraph signal (RTS) capacitance measurement that is interpreted as the parity readout of a (topological) superconductor. I firmly believe it is important to make the community aware of the issues highlighted in my Matters Arising, as it raises significant questions about potential alternative explanations unrelated to (topological) superconductivity that are not apparent in the original manuscript. This transport data determines virtually all experimental parameters, including gate voltage and magnetic field, where the purported "readout" occurs, making it a core component that establishes the interpretation of the main result in Nature 638, 651–655 (2025).

To emphasize why the community must be made aware of these issues, I first highlight the problems that the Microsoft Quantum acknowledged in their reply:

1. **The TGP is sensitive to measurement parameters:** "It is not surprising that the TGP has some sensitivity to data ranges..."
2. **Bug(s) present in the TGP analysis code:** "...the published version of the TGP data in Subsection S4.3 of the Supp. Info. for Ref. [2] has a bug causing an incorrect cropping of the bias range (i.e. it is not symmetric around zero bias, with 1 extra pixel at negative bias), as pointed out in Ref. 1."
3. **The bug(s) can alter the outcome of the TGP:** "...due to the thresholding method used in the TGP, those changes could lead to a change in the status of a pixel (gapless to gapped or visa versa), which could lead, in turn, into a change of cluster overlap, and the subsequent (dis-)appearance of ROI2s."
4. **Purported parity readout measurements were confined to single TGP regions:** "It is true that parity measurements were not performed at every point in these devices' phase space since an exhaustive survey of phase space wasn't the goal of Ref. 2." .

This final point is particularly notable given that it contradicts the referees' explicit request about reproducibility elsewhere in phase space, e.g., "Are A1/A2 the only regions that passed TGP? Are there other regions where things didn't work out but, based on the TGP, should have? ... The authors need to give us some context to make the control experiments meaningful."

The structure of the remainder of this reply is as follows: I will first clarify certain key points raised by multiple referees, then provide point-by-point responses to each referee.

Andreev enhancement:

Both Referee 1 and Referee 3 noted that my Matters Arising should discuss Andreev enhancement and the strong coupling limit. I agree that this point should have been more clearly addressed in the original version of my Matters Arising. Notably, Microsoft Quantum's reply claims that several of their conductance measurements—with specific focus on G_{RR} for Device A2 cutter 2—were performed in the "strong coupling" regime rather than the tunnelling regime. I did not discuss this in the original version, because the claim of the strong coupling regime contradicts both the supplemental material (SM) of Nature 638, 651–655 (2025) and the stated regime of applicability of the topological gap protocol (TGP) outlined in the authors' previous papers:

Nature 638, 651–655, Section S4.1 : "SC1 and SC2 are set back to accumulation while TG1 and TG2 are set to the *tunneling regime* to enable transport measurements on the wire again."

arXiv:2103.12217 : "The cutter gates should be *set such that both junctions are in the tunneling regime*: their high-bias conductance is smaller than e^2/h and it stays within bounds defined as follows... The upper bound is for the system to be in the weakly conducting regime where the local conductance is a convolution of the local density of states with the tunneling matrix element."

PRB 107, 245423 (2023): "We further facilitate this by restricting the cutter gate voltages so that G_N at each junction is limited to the range 0.1–1 e^2/h as a compromise between sufficient visibility and remaining in the *tunneling regime*."

In Fig. R1, line-cuts of local conductance within regions of purported parity readout are presented. In several cases goes far above e^2/h (and in the case of G_{RR} for Device A2 cutter 2 even above $2e^2/h$). This leads to two possibilities:

1. If these data are in the tunnelling regime, where the conductance is proportional to the density of states (up to matrix element effects), then there is an abundance of states at low energy.

2. If these data are in the strong coupling regime, then the TGP in Nature 638, 651–655 (2025) has not been applied "as described in Ref. 35 [PRB 107, 245423 (2023)]" or in the SM of Nature 638, 651–655 (2025), since the regime of applicability is the tunnelling regime.

Importantly, there is also significant low-bias structure, supporting the view that there are numerous overlapping disordered states. Finally, the conductance is negative for certain bias values, which can be indicative of quantum dot physics (discussed further below).

Modification made: I have added a discussion of the possibility for Andreev enhancement, with a clarification that the strong coupling regime (in particular conductance above $2e^2/h$) is not compatible with the stated regime of applicability of the TGP according to the authors' previous work. I have also added the panels in Figure R1 to the colour plots of conductance (Fig. 2 and Extended Data Figs. 1 & 2) of the revised text, allowing readers to see the structure of the local conductance throughout the bias range.

Broken particle-hole symmetry and quantum dots:

Another point raised by both Referee 1 and Referee 3 relates to three-terminal particle–hole symmetry relations and, in particular, the fact that certain conductance symmetries are expected for three-terminal devices if particle-hole symmetry is present. Whilst I believe the considerable breaking of these symmetries can be seen by eye, I also agree that this could have been more clearly established in my Matters Arising. In the region of interest, if the wire were truly gapped—as claimed by the authors—then the low-bias nonlocal conductance should vanish, which would force a local conductance symmetry close to zero-bias. However, one can already see by eye in Fig. R1 that in many cases there is considerable asymmetry in the low-bias local conductance. Hence, if there is a gap, the claim of no particle-hole symmetry breaking that was made by the authors to the original referees is not supported by the conductance data.

To this point much more concrete: The main theoretical relation the authors' response (and referees) invoke for particle–hole symmetry in a three-terminal device is given by [see for instance

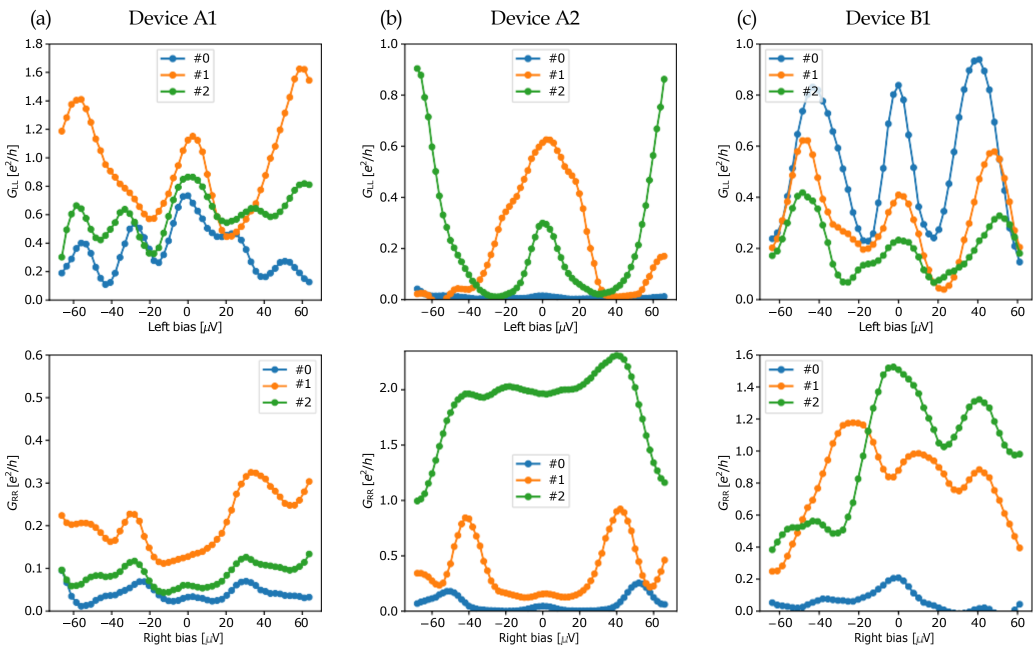

Figure R1: **Line-cuts of local conductance in purported parity readout regions that "passed" the TGP:** Considerable structure is apparent in all line-cuts and no well-defined SC gap is visible (different colours correspond to different cutters). Note: local conductance is negative for certain cutters, which can be a signature of quantum dots, which we discuss this below. Each line-cut corresponds to the voltage and magnetic field settings indicated in my Matters Arising (crossing of dotted lines in TGP plots).

Eq. (7) of Danon *et al.*, PRL 124, 036801 (2020)]:

$$A(G_{ii}) \equiv \frac{G_{ii}(V) - G_{ii}(-V)}{2} = -\frac{G_{ij}(V) + G_{ij}(-V)}{2} \equiv -A(G_{ij}), \quad i \neq j. \tag{1}$$

Figs. R3, R4, & R5 in the Appendix of this response (as Figs. S4–6 in Supplemental Note 2 of the revised manuscript) show that these relations do not hold for the authors' own data, even in the regions reported in the original manuscript. In particular, the antisymmetrised local conductance is often much larger in magnitude than the nonlocal conductance. The fact this relation is not satisfied strongly contradicts the claim of no particle-hole symmetry breaking.

Furthermore, perhaps the clearest indication of the breaking of particle-hole symmetry—and potential signatures of quantum dots—is the presence of *negative local conductance* that is also asymmetric in bias voltage. As shown in Figs. R1 & R2, the local conductance can dip (sometimes considerably) below zero and often on only one side of the bias voltage range. Such negative local conductance has previously been shown to indicate the presence of quantum dots [see, e.g., Lee *et al.*, PRL 109, 186802 (2012)] and the fact it is asymmetric in bias provides a strong indication of particle-hole symmetry breaking.

Modification made: I have modified the discussion of broken particle-hole symmetry in the main text to make it clearer that in a gapped system one expects the local conductance to be symmetric around zero-bias and further discussed that the expected symmetries of three-terminal devices do not hold for the underlying conductance data. In addition, I have added Figs. R3, R4, & R5 to Supplemental Note 2 of the SM. I have also included a brief discussion of the negative local conductance in both the main text and SM.

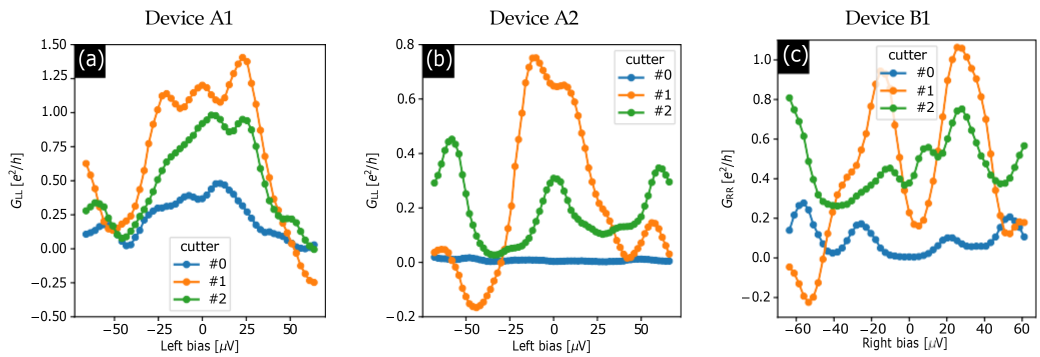

Figure R2: **Further examples of negative local conductance in the authors' released data.** Negative differential conductance occurs for all three devices in many portions of phase space. Here we show the line cuts at the following positions of phase space where large negative values occur: Device A1 (B, V_p) = (2.4 T, −1.82025 V), Device A2 (B, V_p) = (2.66 T, −1.845 V), and Device B (B, V_p) = (21.4 T, −1.676625 V).

Response to Referee 1

Referee 1: I have reviewed the studied the Matters Arising from H. Legg and the Reply from Microsoft.
Is the criticism valid?
The criticisms do not deliver on their promise of calling into questions the core findings of Nature 638, 651–655 (2025). I will summarize my views briefly below.
(2) Is the criticism likely to be of interest to non-specialist readers? The scientific content is not of interest. The criticism will be highly publicized and discussed on X, YouTube, and the like, and in this sense would be of high interest.
(3) Could it be stated more concisely? Yes. A paragraph explaining the issues raised in Fig. 3 would be sufficient.

Response: I thank the referee for their feedback. In line with their suggestion, the Matters Arising has been substantially condensed, with many aspects discussed by the referee moved to Supplemental Material.

Regarding the validity of my critique, I respectfully maintain that challenging the tune-up procedure calls into question the interpretation of the "core findings". The parity readout measurement was performed in a parameter region selected by the TGP. Virtually all key device parameters were set by this tune-up procedure. If the evidence for a (topological) superconducting gap—a prerequisite for protected parity—does not support this, then the confidence in the interpretation of the main result as the parity of a (topological) superconducting nanowire is significantly weakened. This is especially true given the lack of reproduction by the authors in other regions of phase space.

Finally, the referee suggests the critique is scientifically "not of interest" whilst simultaneously suggesting that it will likely be "highly publicised" due to public interest. I do not share this viewpoint and believe the two points are linked: significant public interest stems from the magnitude of the scientific claims made by Microsoft Quantum. However, it is important to state that the primary purpose of a Matters Arising is to ensure the integrity of the scientific record, such that the research is correctly represented in that record. To that end, I believe it is essential that the community is informed of issues with the conductance data identified in my Matters Arising, because these data—which were not presented in the original manuscript—strongly suggest the possibility of other, more trivial explanations for the main claimed result, *e.g.*, quantum dot physics. Such concerns were also raised by the original referees, but due to the errors highlighted in my Matters Arising, these referees' queries were not correctly answered by the authors.

Referee 1: A short summary on my views of the criticisms is below. The criticisms focus on the transport data and TGP. Some of the technical issues raised by Legg are right, and some are wrong. Overall they do not undermine the main novelty of the work, which moves beyond transport signatures by performing parity readout. The work generates strong empirical evidence that there is a coherent, low-energy state with a parity degree of freedom. The field knows from experience that the transport data is difficult to interpret and will not be by itself conclusive.

Response: I thank the referee for this assessment and wish to address their central point: that the work successfully "moves beyond transport signatures". I respectfully disagree and argue that the interpretation of the measurement in Nature 638, 651–655 (2025) is fundamentally inseparable from the transport data that is used to tune-up the devices.

The logic of the paper is as follows:

1. Use a tune-up procedure—in this case the authors use the TGP—to establish the existence of a prerequisite condition: a (topological) superconducting gap.

2. Perform the new measurement within the region where this prerequisite is supposedly met.

3. Interpret the new measurement's signal as a result of this prerequisite condition, i.e., the signal is interpreted as a parity readout of a (topological) superconductor *because* a (topological) SC gap is established according to the TGP/transport data.

My Matters Arising demonstrates that the tune-up in Step 1 gives inconsistent answers and the prerequisite is seemingly contradicted by the underlying transport data. The authors' rebuttal, as alluded to by the referee, argues that their interpretation of Step 3 should be taken as confirmation of Step 1. This inverts the logical flow of the paper into a circular argument: the measurement is interpreted as a parity readout of a (topological) superconducting nanowire because a (topological) SC gap is claimed to exist, but now the existence of this gap is inferred from the supposed parity readout measurement itself.

This is precisely why the work cannot "move beyond" the transport data: The core claim rests upon tuning up the devices with the transport data. The issues with the TGP and the underlying nature of the transport measurements seemingly removes the necessary foundation (Step 1) required to interpret the subsequent signal. Without a robustly established (topological) SC gap, the measurement is a random telegraph signal with certain features. Most importantly, for the Matters Arising, due to the fact this transport data was not presented (and incorrect statements to referees), these issues were not evident in the published manuscript.

Modification made: As above, I have emphasised that the existence of a (topological) SC gap is a prerequisite for the interpretation and also clarified that I do not assess the RTS capacitance measurement in my Matters Arising.

Referee 1: Comment #1 essentially reiterates the prior observation that TGP outcomes depend on windowing. This is an unsurprising result that follows form how the protocol normalizes its data, and in general we should not be surprised that changing meta-parameters can change the results. For the current work, the TGP is used to identify candidate regions for parity readout. It does not cause a problem that altering meta-parameters removes candidate regions. Nothing of interest is raised here.

Response: Based on the referees' comments and editorial advice, I have shifted the discussion in the Matters Arising away from this point, see Modifications Made below. However, I still believe it is important to provide a full answer to the referee. Whilst I agree that a diagnostic tool may show some sensitivity to its parameters, I disagree that this issue is nothing but an "unsurprising" technicality. The referee is correct that I have also highlighted the sensitivity of the TGP in my recent comment on the authors' PRB manuscript; however, I do not believe that makes this sensitivity any less relevant to other papers that utilise the TGP.

In particular, the core issue is that only TGP outcomes that suggest the system is gapped were reported in Nature 638, 651–655 (2025), whilst contradictory outcomes were not reported. This is not simply a case of "altering meta-parameters removing candidate regions"; the evidence for a gapped system being contradicted by other data from the same measurement was not presented. For example, in Device A2, the TGP reports the readout region as gapless when using cutter_value=2. However, with no discussion of this gapless outcome for cutter_value=2, the authors reported only the gapped outcome for cutter_value=1 in Nature 638, 651–655 (2025). For the other devices (A1 and B1) cutter_value=2 was reported.

Furthermore, I respectfully disagree with the referee's assessment that "nothing of interest is raised here". The other referees specifically commented on the parameter dependence of the TGP. Referee 2, for instance, described these concerns as "substantive" and going "to the heart of the experimental validation". Referee 3 also highlighted the "the pixel-level data cropping issue".

Finally, although I will not comment on the specifics of the new manuscript, it is notable that in their latest preprint (arXiv:2507.08795), Microsoft Quantum present *all* cutter outcomes and seemingly use the same magnetic field ranges in their TGP analysis. This was not the case in Nature 638, 651–655 (2025) or previous TGP papers.

Modifications Made: As stated above, I have now modified the comment to focus on the specific issues raised that are directly related to the Nature 638, 651–655 (2025) publication, rather than the wider issues with the TGP.

Referee 1: Comment #2, in contrast, does raise a problem of interest. It seems that a coding error made the authors miss several regions that would have passed TGP. This caused them to miss an opportunity to more broadly correlate parity readout with the TGP. Legg has found a mistake here and I am glad he points it out. This point is of interest, but does not undermine the study.

Response: I thank the referee for identifying this as a "problem of interest" and agreeing that a "mistake" was found. It is important to emphasise that the issue goes beyond a single coding error. The failure to identify/report other TGP regions was the result of two distinct coding issues:

1. The analysis code was set to only highlight the single largest region as passing the TGP, meaning that other (smaller) regions that passed were simply not highlighted.
2. The underlying data was processed with an incorrect antisymmetrisation routine, which used the array index rather than the bias voltage, leading to incorrect outcomes of the authors' chosen tune-up method.

Whilst the referee suggests this "does not undermine the study", it does undermine confidence in the reproducibility of the signal. This is underscored by the original referees' questions during the review process: Several referees specifically asked whether other regions passed the TGP and if the supposed parity readout was reproducible elsewhere in the device's phase space. Due to these coding errors, the authors incorrectly informed the referees that the investigated region "was the only region passing the TGP within the explored gate voltage and magnetic field range". These coding issues led to an incorrect answer to a central referee question about reproducibility. It also means that the region investigated in Device B is secondary compared to much larger purportedly topological regions elsewhere in phase space.

Modifications Made: I have made it clearer in the text that two coding errors resulted in incorrect TGP outcomes being reported.

Referee 1: Comment #3 again deals with identifying candidate regions using the TGP. The only part that I see as definitely valid is that Microsoft claimed in their reply that there is no evidence of particle-hole symmetry breaking, and it is completely unclear what this statement could have meant. They are correct that antisymmetric conductance is allowed in a three-terminal device without breaking particle-hole symmetry, as has been extensively studied. They also correctly point out that Andreev reflection can lead to sub-gap local conductance, and voltage-divider effects can do the same. Given the measured conductances, which are in-between deep tunneling and the open regime, these effects are likely present. The data for Device A1 looks promising, A2 is less convincing, and B1 is in between. It would be interesting to study and debate these points in more detail, but ultimately the whole reason to move beyond arguments over transport data is that they do not tend to be conclusive.

Response: I thank the referee for their assessment and for useful feedback regarding the analysis of the underlying transport data. These have lead to improved discussions about the possibility of Andreev enhancement and the evidence for particle-hole symmetry breaking (see above) in my Matters Arising.

On the other hand, whilst I agree that transport data can be complex, I do not understand the referee's perspective of how one can "move beyond arguments over transport data" in this case. The original authors' claim is based on an experimental tune-up using transport data. The paper uses the TGP to tune up the devices into a supposedly gapped (topological) superconducting phase. The interpretation of the main "parity readout" experiment is not independent of the transport data; it is fundamentally predicated upon the TGP's output selecting an ostensibly suitable operating region. As a result, the reliability of the transport data is not an argument to be moved "beyond", but a foundational issue and I strongly believe the community should be made aware of the issues with the underlying unreported transport data.

Furthermore, as discussed above, the authors' attempt in their reply to dismiss the inconclusive features in the transport data by invoking a "strong coupling regime" and "Andreev enhancement" directly contradicts their own previous publications on the TGP (and the SM of the original manuscript). One cannot "move beyond" transport data discussions by developing an analysis routine (the TGP) that simply avoids presenting that data. That is especially relevant here as the authors now claim to have applied the TGP far outside its original regime of applicability and with very different data parameters. Avoiding arguments over the data by not presenting it is not the same as moving beyond discussions about it.

Modifications Made: As outlined at the start, I have clarified both in the main text and the supplemental material that the data does not obey the expected symmetry relations from particle-hole symmetry and that the strong coupling regime is not compatible with the authors' previously stated regime of applicability for the TGP.

Response to Referee 2

Referee 2: This Matters Arising raises substantive concerns regarding the methodology and interpretation of results in the original article by Microsoft Quantum. Specifically, it questions the reliability of the Topological Gap Protocol (TGP) used to identify gapped regions in proximitised nanowires, and presents compelling arguments suggesting that the regions where parity readout was performed may, in fact, be gapless and highly disordered—depending on the analysis parameters. These are not minor technicalities but go to the heart of the experimental validation of the parity readout.

Response: I thank Referee 2 for their thorough evaluation and strong support. I am grateful that the referee recognised that this work raises "substantive concerns" and presents "compelling arguments" regarding the original paper's methodology and interpretation. I particularly appreciate their crucial assessment that these concerns "are not minor technicalities but go to the heart of the experimental validation of the parity readout". The referee correctly identifies that the authors' reply does not fully resolve these core issues, a point underscored by the contradictions between their rebuttal and the published work (see above). By affirming the central critique—that the regions under study may in fact be "gapless and highly disordered"—the referee reinforces why my revised Matters Arising is essential for ensuring the scientific community can properly evaluate the robustness of the original paper's claims.

Referee 2: The authors of the original article have submitted a detailed reply addressing several technical points and defending the validity of their main claims. However, in my view, the response does not fully resolve the core issues raised in the comment—particularly those concerning the transparency and reproducibility of the experimental methodology, and most importantly, the inconsistent TGP outcomes and their implications for the interpretation of the parity readout experiments.

Response: I thank the referee for this assessment. I agree with their view that fundamental questions remain unresolved by the authors' reply. This reinforces why this Matters Arising is necessary for the completeness of the scientific record.

Referee 2: The Microsoft Quantum article has generated significant attention and debate within the condensed matter and quantum computing communities. The issues raised in the comment are of broad relevance—not only for this specific experiment, but also for the interpretation of parity measurements and the future use of the TGP protocol in Microsoft's experimental program. I strongly recommend that this Matters Arising be published alongside the authors' reply. The comment raises valid and unresolved concerns that merit open discussion. Given the high profile and potential implications of the original work, it is essential that the scientific community has access to both the critique and the detailed author's response in order to evaluate the robustness of the claims and to treat the original findings with appropriate caution until further clarification is provided.

Response: I thank the referee for their strong endorsement and for highlighting the broader importance of this scientific discussion. The referee's recommendation aligns perfectly with the goal of this Matters Arising: to ensure that the scientific community has the necessary information to evaluate the robustness of the claims and treat the findings with appropriate caution until further clarification is provided. I am particularly grateful for the referee's recognition that these issues have implications not only for this specific experiment, but also for the interpretation of parity measurements and the future use of the TGP.

Response to Referee 3

Referee 3: The Matters Arising submission by Legg raises technical concerns regarding the Topological Gap Protocol (TGP) implemented in Supplementary Information (S4.3) of the original Nature paper (Microsoft Quantum, Nature 638, 651–655 (2025)). The author identifies potential inconsistencies in TGP outcomes—notably sensitivity to data parameters (e.g., magnetic field ranges, cutter pairs), coding errors affecting gap/gapless classifications, and unaddressed disorder features in conductance data.
I commend the author for his rigorous scrutiny of methodological details. His insistence on examining raw conductance data and probing TGP's parameter dependencies exemplifies the essential scientific practice of independent verification. However, in my view, Matters Arising should primarily address the central discovery/conclusion of a recently published paper, rather than focusing on points confined to Supplementary Information, Zenodo data, or review materials. The Microsoft Nature paper's core conclusion is the demonstration of single-shot interferometric fermion parity measurement, evidenced by flux-dependent bimodal random telegraph signals (RTS) in quantum capacitance. The author's critique primarily targets the TGP methodology—which supports the claim of topological superconductivity but is peripheral to the parity readout demonstration. Consequently, I do not recommend publication as a Matters Arising article with only these comments. These comments are more appropriately directed at the PRB paper (Phys. Rev. B 107, 245423 (2023)).

Response: I am deeply grateful to the referee for their statements regarding the scientific scrutiny involved in my Matters Arising. The referee correctly identifies this as an essential part of scientific verification, which is indeed the sole purpose of this Matters Arising: to make the community aware of the issues with the transport data that was not presented in Nature 638, 651–655 (2025). This transport data raises significant questions about potential alternative explanations for the core claims of the original publication.

I would first like to clarify why I strongly feel my Matters Arising is highly important to the paper's main claim, rather than peripheral issues, despite not directly discussing the RTS capacitance signal. The interpretation of the main result—the RTS—is fundamentally dependent on the transport data used in the tune-up procedure. The paper's logical structure requires first using the TGP to establish the device parameter regime that satisfies a prerequisite (a topological superconducting gap), then interpreting the RTS measurement based on that prerequisite. The vast majority of important device parameters are set by the TGP outcomes that are based on this transport data.

My Matters Arising demonstrates that the tune-up mechanism used to establish this (topological) superconducting gap is unreliable and contradicted by the complete dataset, which was not presented in the original publication. As such, this is not a peripheral concern, it directly challenges the foundation the authors used to interpret the subsequent RTS measurements. Without a reliable demonstration of the prerequisite superconducting gap, alternative explanations such as quantum dot physics become highly plausible. These alternatives only become apparent when examining the full transport data, which is why it is crucial that the community has access to this information.

Modifications Made: As discussed above, I have clarified that I focus on the transport data and not the RTS measurement that is interpreted as parity readout. I have also emphasised why establishing this prerequisite is key to the interpretation of that RTS measurement and therefore is central, rather than peripheral to the main claim.

Referee 3: Additional comments: 1) The author think the nature paper relies the TGP to prove there is a superconducting gap. But the author didn't argue how the quantum capacitance readout work without if the wire is gapless. Is the model or the measurement wrong?

Response: I thank the referee for this important query. Indeed, the question of how the RTS data arises is extremely important, but—as outlined above—addressing this is not the purpose of my Matters Arising. As now highlighted in the revised version, RTS capacitance is well known in mesoscopic system e.g. from quantum dots, a fact also mentioned by the original referees. However, the primary purpose is to bring to the attention of the community the issues with the tune-up and conductance measurements in these devices, thereby enabling other researchers to understand the full context of the claim in Nature 638, 651–655 (2025). As Referee 2 aptly summarises: "it is essential that the scientific community has access to both the critique and the detailed author's response in order to evaluate the robustness of the claims and to treat the original findings with appropriate caution until further clarification is provided."

I must emphasise a critical logical issue in Microsoft Quantum's response: The conductance data is used to establish the prerequisite for the authors' conclusion, yet in their response they now invoke the interpretation of the RTS capacitance measurement as evidence for this prerequisite itself. This essentially constitutes circular reasoning. Significantly, even the authors' chosen tune-up mechanism, the TGP, yields ambiguous results regarding whether an SC gap exists, but this ambiguity was not reported because only gapped outcomes were presented in Nature 638, 651–655 (2025). Specifically, in Device A2, the TGP reports the readout region as gapless when using `cutter_value=2`. Rather than discuss this, the authors' publicly released analysis code reveals that they presented the outcome for `cutter_value=1`, whilst for all other measurements (A1 and B1) they used `cutter_value=2`. This presentation of only gap-supporting outcomes represents a significant omission that should be highlighted to the community, since a gapless outcome would be inconsistent with the authors interpretation of their main result.

Modification made: I have clarified in the revised manuscript that, whilst I do not directly assess the RTS signal, the transport data reveals that multiple non-topological explanations remain viable explanations for this data. Only by revealing the issues with these data can other explanations be considered by the community with the full context required. I have also provided references for similar RTS that have been observed in mesoscopic systems, e.g., due to quantum dots.

Referee 3: 2) To substantiate alternative explanations (e.g., "fine-tuned mesoscopic disorder effects"), the author should provide detailed experimental or simulated validation—such as modeling a fine-tuned disordered system replicating the Nature paper's results.

Response: The referee's question is precisely the one that this underlying data highlights to the community. As above, the primary goal of this Matters Arising is not to provide a fully-formed alternative explanation, but rather to fulfil Nature's guideline that: "Matters Arising are exceptionally interesting and timely scientific comments and clarifications on original research papers". The authors' own complete transport data—none of which was shown in the original paper—appears inconsistent with a superconducting gap and exhibits a high degree of disorder. Even the authors own tune-up method sometimes reported the regions of "readout" as gapless. I believe this alone fulfils the criteria for a Matters Arising as it provides a significant caveat to the interpretation of the original results.

However, even more importantly, highlighting this previously unreported data is crucial because it provides the full context for alternative explanations to be developed. The breaking of particle-hole symmetry (discussed above) particularly supports such alternative explanations such as quantum dot physics. However, the development of detailed alternative models, whilst a clear next step, is beyond the scope of my Matters Arising and more appropriate for subsequent research papers which I believe my Matters Arising will encourage.

Modification made: I have emphasised that the findings reported in my Matters Arising encourage the development of alternative explanations for the reported effect. A short discussion of trivial sources of RTS capacitance in mesoscopic systems has also been added.

Referee 3: 3) When challenging the main discovery, the author must address the entirety of relevant evidence. The Nature paper's Supplementary Information and related publications establish a proof-chain; critiques should engage holistically. For instance, while noting "highly disordered" gapless conductance data, the author omits discussion of the symmetry around zero energy—a very common feature to see in superconducting gap in both tunneling regime or AR enhanced regime.

Response: As was discussed at the start of this document, I fully agree with the referee that the discussion of symmetries could have been improved and the revised version discusses this aspect much more fully. As discussed above, I have now included line-cuts that show in many cases there is no clear symmetry of local conductance around zero-bias, contrary to what would be expected in a gapped system with particle-hole symmetry. There is also now a much more complete discussion of symmetries throughout the Matters Arising.

However, I must respectfully strongly disagree the referee's other assertion that the authors' previous papers establish a "proof-chain". In particular, I have recently highlighted significant issues with the TGP as applied in PRB 107, 245423 (2023). The underlying conductance data in the present paper are, if anything, of lower quality than those in the previous publication. Moreover, the TGP parameters used in Nature 638, 651–655 (2025) do not correspond to any simulation or device in PRB 107, 245423 (2023). As one concrete example: all simulations in PRB 107, 245423 (2023) used 5 cutter pairs, whereas the devices in Nature 638, 651–655 (2025) use only 3 cutter pairs. As also noted above, the authors claim (now) of being in the strong coupling regime contradicts their previous publications on the TGP. The fundamental inconsistencies throughout the literature in the use of the TGP undermines any claim of methodological continuity.

Modifications made: I have added line-cuts of local conductance (Figs. R1 and R2) to the main figures, extended data figures, and SM. These clearly showing the absence of symmetry around zero-bias in many cases. I have also expanded the discussion of particle-hole symmetry breaking in the main text, including the addition of Figs. R3–R5 in Supplemental Note 2, which demonstrate that the expected three-terminal symmetry relations do not hold for the authors' data.

Referee 3: 4)Greater attention to experimental nuances is needed: distinguishing tunneling conductance from enhanced Andreev reflection conductance, and addressing transport asymmetry of particle-hole partners (rather than particle-hole asymmetry itself).

Response: I very much appreciate the referee's comment regarding these experimental nuances. As discussed at the start of this document and in the above responses, I believe I have carefully addressed these distinctions and added a more complete analysis in the revised Matters Arising.

Regarding the tunnelling versus strong coupling regime: the authors' claim of operating in the strong coupling regime directly contradicts both the supplemental material of Nature 638, 651–655 (2025) and their own stated methodology in previous publications, which explicitly require the tunnelling regime for the TGP to be applicable (see discussion at the start). This fundamental inconsistency undermines the validity of the defence in the authors' reply.

Furthermore, the observation of negative local conductance provides clear evidence of broken particle-hole symmetry, which has been associated with quantum dot formation. This phenomenon cannot be explained by simple transport asymmetry between particle-hole partners and points to more fundamental issues with the authors' devices and/or experimental setup.

Modifications made: I have expanded the discussion to clearly distinguish between tunnelling and Andreev-enhanced conductance regimes, emphasising that the strong coupling regime is incompatible with the stated TGP methodology. I have also added explicit discussion of transport asymmetry versus particle-hole symmetry breaking, including new figures showing negative local conductance (see discussion around Fig. R2) and violations of the expected three-terminal symmetry relations (see Appendix or Supplemental note 2).

Referee 3: I reiterate that the author's meticulous analysis provides valuable feedback for the Microsoft Nature authors to consider—potentially improving methodology, corrections, or evaluation (e.g., the pixel-level data cropping issue).

Response: I thank the referee for their concluding remarks and for recognising that this analysis provides valuable and constructive feedback for the original authors. Most importantly, I believe this Matters Arising provides invaluable information to the broader scientific community about the authors' original experiment that would not otherwise be apparent from Nature 638, 651–655 (2025). By revealing the issues with the transport data and highlighting the methodological TGP problems identified by the referee (such as the pixel-level data cropping issue), this work enables the community to make a much more fully informed assessments of the central claims in Nature 638, 651–655 (2025). I am grateful for the referee's thorough review and constructive engagement with these important scientific issues that has considerably improved my Matters Arising.

Appendix: Absence of particle-hole symmetry relations

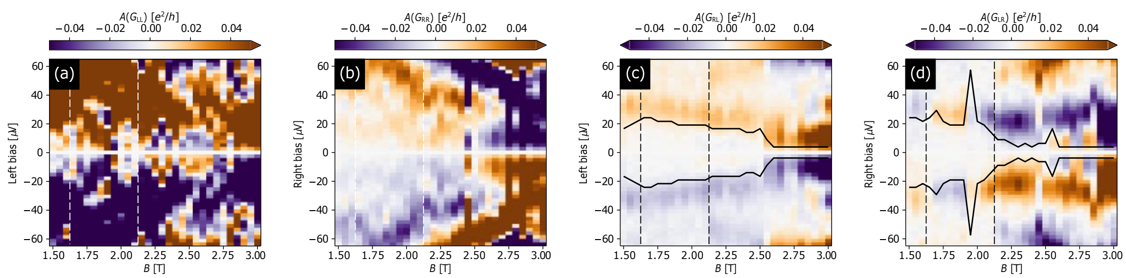

Figure R3: **Antisymmetrised local and nonlocal conductance for Device A1, cutter 2:** The authors' data does not exhibit the expected particle-hole symmetry relation (same V_p cut as shown in Matters Arising).

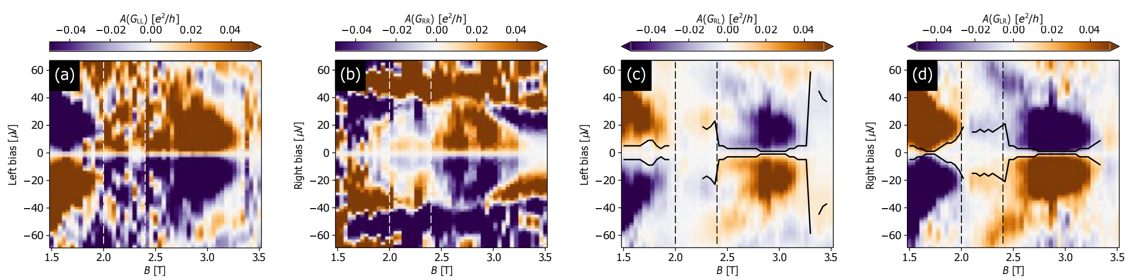

Figure R4: **Antisymmetrised local and nonlocal conductance for Device A2, cutter 1:** The authors' data does not exhibit the expected particle-hole symmetry relation (same V_p cut as shown in Matters Arising).

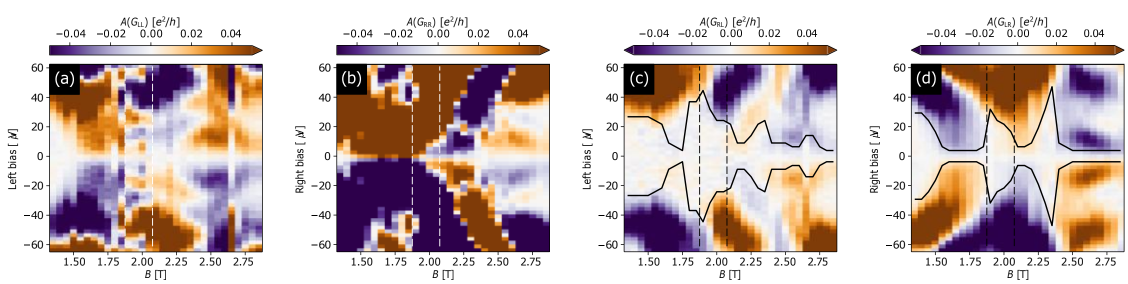

Figure R5: **Antisymmetrised local and nonlocal conductance for Device B1, cutter 2:** The authors' data does not exhibit the expected particle-hole symmetry relation (same V_p cut as shown in Matters Arising).

Reply to Matters Arising: "Interferometric single-shot parity measurement in InAs–Al hybrid devices"

Corresponding Author: Dr Chetan Nayak

Version 0:

Reviewer comments:

Referee #1

(Remarks to the Author)
I have reviewed the studied the Matters Arising from H. Legg and the Reply from Microsoft.

Is the criticism valid?
The criticisms do not deliver on their promise of calling into questions the core findings of Nature 638, 651–655 (2025). I will summarize my views briefly below.

(2) Is the criticism likely to be of interest to non-specialist readers?
The scientific content is not of interest. The criticism will be highly publicized and discussed on X, YouTube, and the like, and in this sense would be of high interest.

(3) Could it be stated more concisely?
Yes. A paragraph explaining the issues raised in Fig. 3 would be sufficient.

A short summary on my views of the criticisms is below. The criticisms focus on the transport data and TGP. Some of the technical issues raised by Legg are right, and some are wrong. Overall they do not undermine the main novelty of the work, which moves beyond transport signatures by performing parity readout. The work generates strong empirical evidence that there is a coherent, low-energy state with a parity degree of freedom. The field knows from experience that the transport data is difficult to interpret and will not be by itself conclusive.

Comment #1 essentially reiterates the prior observation that TGP outcomes depend on windowing. This is an unsurprising result that follows form how the protocol normalizes its data, and in general we should not be surprised that changing meta-parameters can change the results. For the current work, the TGP is used to identify candidate regions for parity readout. It does not cause a problem that altering meta-parameters removes candidate regions. Nothing of interest is raised here.

Comment #2, in contrast, does raise a problem of interest. It seems that a coding error made the authors miss several regions that would have passed TGP. This caused them to miss an opportunity to more broadly correlate parity readout with the TGP. Legg has found a mistake here and I am glad he points it out. This point is of interest, but does not undermine the study.

Comment #3 again deals with identifying candidate regions using the TGP. The only part that I see as definitely valid is that Microsoft claimed in their reply that there is no evidence of particle-hole symmetry breaking, and it is completely unclear what this statement could have meant. They are correct that antisymmetric conductance is allowed in a three-terminal device without breaking particle-hole symmetry, as has been extensively studied. They also correctly point out that Andreev reflection can lead to sub-gap local conductance, and voltage-divider effects can do the same. Given the measured conductances, which are in-between deep tunneling and the open regime, these effects are likely present. The data for Device A1 looks promising, A2 is less convincing, and B1 is in between. It would be interesting to study and debate these points in more detail, but ultimately the whole reason to move beyond arguments over transport data is that they do not tend to be conclusive.

Referee #2

(Remarks to the Author)
Remarks to the Authors:
This Matters Arising raises substantive concerns regarding the methodology and interpretation of results in the original article by Microsoft Quantum. Specifically, it questions the reliability of the Topological Gap Protocol (TGP) used to identify gapped regions in proximitised nanowires, and presents compelling arguments suggesting that the regions where parity readout was performed may, in fact, be gapless and highly disordered—depending on the analysis parameters. These are not minor technicalities but go to the heart of the experimental validation of the parity readout.

The authors of the original article have submitted a detailed reply addressing several technical points and defending the validity of their main claims. In particular, the authors argue that:

1) The comment does not challenge the main claims of the original article, which are based on the observation of flux-dependent bimodal random telegraph signals in quantum capacitance, interpreted as single-shot parity measurements.
2) The criticisms are confined to a supplementary section (S4.3) and do not undermine the core experimental results.
3) Some analytical and numerical arguments are presented to support the presence of a superconducting gap, contradicting the claim of a gapless system.

However, in my view, the response does not fully resolve the core issues raised in the comment—particularly those concerning the transparency and reproducibility of the experimental methodology, and most importantly, the inconsistent TGP outcomes and their implications for the interpretation of the parity readout experiments. Importantly, the comment identifies a bug in the TGP data processing that affects the gate voltage range and, consequently, the extracted gap values. Similar concerns could be raised about the magnetic field range. Although the authors acknowledge this issue and argue that its impact is minor, the potential for misidentification of gapped regions remains a legitimate concern. The disagreement over the presence or absence of a superconducting gap based on conductance measurements is substantial. While the response offers plausible alternative interpretations it does not conclusively rule out the possibility of a gapless regime.

Another relevant issue concerns reproducibility and parameter space coverage since other regions identified by the TGP were not explored for parity readout. Conversely, other regions which did not pass the TGP were not investigated for parity readout as a sanity check. This raises valid questions about the reproducibility and generality of the observed phenomena.

The Microsoft Quantum article has generated significant attention and debate within the condensed matter and quantum computing communities. The issues raised in the comment are of broad relevance—not only for this specific experiment, but also for the interpretation of parity measurements and the future use of the TGP protocol in Microsoft's experimental program. I strongly recommend that this Matters Arising be published alongside the authors' reply. The comment raises valid and unresolved concerns that merit open discussion. Given the high profile and potential implications of the original work, it is essential that the scientific community has access to both the critique and the detailed author's response in order to evaluate the robustness of the claims and to treat the original findings with appropriate caution until further clarification is provided.

Referee #3

(Remarks to the Author)
The response by Nayak et al. addresses Legg's Matters Arising (MA) submission. While I concur that the MA does not invalidate the paper's core claims, the authors should rigorously engage with the identified methodological limitations and errors.
If the journal proceeds with publication of the MA and the reply, I recommend addressing these points:

(1) TGP Sensitivity Justification
Current text: "It is not surprising that TGP has sensitivity to data ranges, as pointed out in Ref. 4."
Required action: Specify the exact sections of Ref. 4 discussing data-range sensitivity (e.g., Appendix E.3/F.2) and explicitly connect these findings to Legg's Fig.1/Fig.2 cases. Discuss how this sensitivity impacts the Nature paper's interpretations.

(2) Bug Correction Transparency
Current text: States pixel-level gap value changes (≤5 μeV in 96% of pixels).
Required action: Commit to publishing corrected data (e.g., via Zenodo repository) or provide a supplementary figure showing pre-/post-correction comparisons.

(3) Device-Specific Disorder Characterization
Current text: Cites disorder analysis from Ref. 4.
Required action: Directly address disorder in this study's device—Ref. 4's analysis of different devices is not directly related. Provide device-specific evidence (e.g., conductance fluctuation metrics) to counter Legg's "highly disordered" claim.

(4) Andreev Enhancement Implications
Current text: Attributes finite conductance to Andreev enhancement (citing Fig. 7 in [8]).

Required action: Analyze how tunneling vs. Andreev-dominated coupling regimes impact parity readout fidelity—a critical concern unaddressed in the original paper or reply.

(5) Particle-Hole Asymmetry Mechanisms
Current text: Defends asymmetry via literature citations [9,12,13].
Required action: Explain physical origins of transport asymmetry in this experiment (e.g., electrostatic inhomogeneity, non-ideal lead couplings) rather than deferring to generic theory.

Version 1:

Reviewer comments:

Referee #1

(Remarks to the Author)
The authors correctly the criticisms of Legg.

I appreciate the authors correcting me about missed regions. They are right that the ROI's were already present in the original manuscript. The change in regions identified in S7f still matters somewhat.

Overall I find these replies convincing.

Referee #2

(Remarks to the Author)
I have reviewed the revised Matters Arising by Legg, the authors' Reply, and the associated correspondence. After this additional round of refereeing, it is clear that the central focus of the Matters Arising is on the transport data and the Topological Gap Protocol (TGP) used to tune up the devices into a purported topological superconducting phase. The critique does not directly address the random telegraph signal (RTS) capacitance measurement itself, but rather challenges the foundational assumptions and methodology used to select the operating regime in which the RTS was interpreted as a parity readout of a topological superconductor. According to the Matters Arising, if the TGP is unreliable or applied inconsistently, the interpretation of the RTS as a topological parity signal is substantially weakened, though the critique stops short of providing a definitive alternative explanation.

The authors are careful in their rebuttal, stating: "We show that the issues raised do not undermine our conclusions. The observed parity-switching signals are robust and incompatible with the 'gapless' scenario suggested by the author." They argue that the parity readout data itself is confirmation of a gapped system, as a gapless wire would wash out the clear interferometric signal, a point supported by theoretical models in their supplementary information. However, this defense relies on interpreting the RTS signal as a readout of a gapped system—an interpretation that the Matters Arising argues is predicated on the very TGP data now in question. This creates a circularity: the RTS is used to validate the TGP's conclusion, while the TGP was used to justify the RTS's interpretation.

The ball is now in the editors' court to decide the best way forward—whether that's publishing this Matters Arising and the Reply, or finding another suitable way to alert the community. From my perspective, I feel that simply posting a corrected TGP map on Zenodo doesn't fully resolve the situation. The core of the critique, in my view, is that these issues are quite fundamental; they call the original interpretation into question, and I believe the community needs to be aware of that context.

I would strongly recommend that, at a minimum, the authors be required to publish additional data—such as specific examples of parity readout performed in regions that do not pass the TGP. This would serve as the crucial experimental evidence to substantiate their claim that "a gapless wire would wash out the clear interferometric signal." Importantly, this would also help addressing concerns about reproducibility and the selectivity of the reported results, and would provide a more transparent basis for evaluating the robustness of the parity readout claim.

In summary, while the Matters Arising does not dispute the existence of the RTS signal, it compellingly argues that the interpretation of that signal rests on an unvalidated and potentially flawed transport-based TGP. The community deserves to see this critique and the authors' response, in the form the editors deem most appropriate, but the scientific record would be best served by additional experimental transparency—especially regarding non-TGP regions.

Referee #3

(Remarks to the Author)
See my comments to the MA:
I have reviewed the revised Matters Arising and the authors' response. While the revision provides additional detail, my position remains unchanged: the critique does not meet the bar for publication in Nature. The core issue is its failure to provide a compelling, self-consistent alternative explanation for the central finding—the flux-periodic random telegraph signal. Merely suggesting possibilities like disorder or quantum dots (which would be surprising on a micron-scale loop) is insufficient without experimental or simulated validation. Speculative questioning, which neither confirms nor definitively refutes the original claim, does not constitute a substantive challenge.

Dear Editor,

Our point-by-point response to the referees' comments may be found below.

Sincerely,
C. Nayak (for the authors)

Referee 1:

We agree with the main points made by Referee 1. We will quote from some of them as part of our response to the other referees.

There is one technical point that we wish to clarify. Referee 1 stated "It seems that a coding error made the authors miss several regions that would have passed TGP." This isn't quite right. The coding error changed the size of the ROI2s in device B. There were three ROI2s both before and after the bug fix but one of the ROI2s grew as a result of the bug fix because an SOI2 for cutter 2 grew and merged with one of the ROI2s that was previously only present for the other two cutters. This ROI2 was not shown in our Fig. S7f because we chose to only highlight the ROI2 where we performed parity readout and to only display data for a single cutter. Nevertheless, we emphasize that we will fix this bug in the Zenodo repo.

Referee 2:

Specifically, it questions the reliability of the Topological Gap Protocol (TGP) used to identify gapped regions in proximitised nanowires, and presents compelling arguments suggesting that the regions where parity readout was performed may, in fact, be gapless and highly disordered—depending on the analysis parameters.

- The TGP has a false positive rate which is low enough that it is a very useful tune-up procedure for our devices, enabling us to cut down a large parameter space to a smaller one where we perform parity readout. Pointing out ways in which the TGP could potentially have false positives only contributes to our understanding if their statistical likelihood is computed. Nothing in the MA suggests that the false positive rate is higher than our estimates. The same holds for questions about analysis parameters. This point has been discussed extensively in Legg's other comments (e.g. arXiv:2502.19560) and our response (arXiv:2504.13240).

These are not minor technicalities but go to the heart of the experimental validation of the parity readout.

- Questions about the TGP do not, in any way, affect experimental validation of the parity readout. In fact, it is the opposite. As the first referee notes: "the main novelty of the work, which moves beyond transport signatures by performing parity readout. The field knows from experience that the transport data is difficult to interpret and will not be by itself conclusive."

Importantly, the comment identifies a bug in the TGP data processing that affects the gate voltage range and, consequently, the extracted gap values. Similar concerns could be raised about the magnetic field range.

- The statement about the magnetic field is incorrect. The bug is strictly a single-pixel shift of the voltage range. There isn't any analogous shift of the magnetic field range (and the MA doesn't claim that there is one).

Although the authors acknowledge this issue and argue that its impact is minor, the potential for misidentification of gapped regions remains a legitimate concern. The disagreement over the presence or absence of a superconducting gap based on conductance measurements is substantial. While the response offers plausible alternative interpretations it does not conclusively rule out the possibility of a gapless regime.

- Our parity measurements rule out a gapless regime, and the MA does not attempt to dispute this point. It isn't possible to conclusively rule out a gapless regime through transport data alone and we never claim to completely rule it out (the TGP has a non-zero false positive rate, but the limits to how much can be deduced purely through transport are more generally true). However, the probability that it is gapless is small, and the parity measurement is not consistent with a gapless system. Again, we note that the first referee reiterated this point, as we quoted above.

Another relevant issue concerns reproducibility and parameter space coverage since other regions identified by the TGP were not explored for parity readout. Conversely, other regions which did not pass the TGP were not investigated for parity readout as a sanity check. This raises valid questions about the reproducibility and generality of the observed phenomena.

- We did not explore every single ROI2 identified by the TGP since an exhaustive search was not the goal of this paper. We did perform parity readout in regions that did not pass the TGP but, again, we did not do this exhaustively. An exhaustive search of phase space was neither necessary nor even useful for demonstrating that parity readout can be performed rapidly with high SNR.

The issues raised in the comment are of broad relevance—not only for this specific experiment, but also for the interpretation of parity measurements and the future use of the TGP protocol in Microsoft's experimental program.

- The MA correctly identified a bug, and we will correct this. Otherwise, the MA does not raise any other topics that aren't covered in other manuscripts on transport, and it does not even attempt to formulate an alternate model for our parity measurements.

Referee 3:

(1) TGP Sensitivity Justification Current text: "It is not surprising that TGP has sensitivity to data ranges, as pointed out in Ref. 4." Required action: Specify the exact sections of Ref. 4 discussing data-range sensitivity (e.g., Appendix E.3/F.2) and explicitly connect these findings to Legg's Fig.1/Fig.2 cases. Discuss how this sensitivity impacts the Nature paper's interpretations.

- The MA no longer focuses on sensitivity to parameter ranges, and Figs. 1 and 2 from the first version of the MA are not in the second version. We agree with Legg's decision to accept the first referee's comments: "This is an unsurprising result that follows from how the protocol normalizes its data, and in general we should not be surprised that changing meta-parameters can change the results. For the current work, the TGP is used to identify candidate regions for parity readout. It does not cause a problem that altering meta-parameters removes candidate regions. Nothing of interest is raised here." As noted by the third referee, sensitivity to data ranges is addressed in Appendix E.2 and E.3 of Ref. 4. For instance, one can compare Tables II and V.

(2) Bug Correction Transparency Current text: States pixel-level gap value changes (5 μeV in 96% of pixels). Required action: Commit to publishing corrected data (e.g., via Zenodo repository) or provide a supplementary figure showing pre-/post-correction comparisons.

- We are updating this in the Zenodo.

(3) Device-Specific Disorder Characterization Current text: Cites disorder analysis from Ref. 4. Required action: Directly address disorder in this study's device—Ref. 4's analysis of different devices is not directly related. Provide device-specific evidence (e.g., conductance fluctuation metrics) to counter Legg's "highly disordered" claim.

- The MA no longer claims that our devices are highly-disordered, so we do not think that this needs to be discussed in our response. We agree with Legg's decision to drop this claim since disorder characterization is not one of the topics of our Nature paper, although the measurements presented in our paper are consistent with a low-disorder device.

(4) Andreev Enhancement Implications Current text: Attributes finite conductance to Andreev enhancement (citing Fig. 7 in [8]). Required action: Analyze how tunneling vs. Andreev-dominated coupling regimes impact parity readout fidelity—a critical concern unaddressed in the original paper or reply.

- Parity readout is conducted in a different configuration from transport. As explained in Supplemental Information S4.4, the leads are cut off, and the wires are coupled to quantum dots. The quantum dots are tuned to the vicinity of a single non-degenerate charge transition, where analogous effects are suppressed. Tunneling of Cooper pairs around the interference loop is suppressed by the dots' charging energy and the square of the tunneling amplitude.

(5) Particle-Hole Asymmetry Mechanisms Current text: Defends asymmetry via literature citations [9,12,13]. Required action: Explain physical origins of transport asymmetry in this experiment (e.g., electrostatic inhomogeneity, non-ideal lead couplings) rather than deferring to generic theory.

- A discussion of the physical origins of transport asymmetry would be appropriate in a paper about transport (of which there have been many), but our paper is about rf parity measurements.Or, in the words of the first referee: "antisymmetric conductance is allowed in a three-terminal device without breaking particle-hole symmetry, as has been extensively studied. It would be interesting to study and debate these points in more detail, but ultimately the whole reason to move beyond arguments over transport data is that they do not tend to be conclusive."