nature portfolio

Peer Review File

Interferometric Single-Shot Parity Measurement in InAs-Al Hybrid Devices

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The editorial team sought additional input from Reviewers #2 and #3 after the second round of review to establish this manuscript's technical correctness. Their responses proved satisfactory enough to proceed to publication. The editorial team wishes to point out that the results in this manuscript do not represent evidence for the presence of Majorana zero modes in the reported devices. The work is published for introducing a device architecture that might enable fusion experiments using future Majorana zero modes.

Version 0:

Reviewer comments:

Referee #1

(Remarks to the Author)
This paper reports on interferometric measurements of fermion parity in a hybrid semiconductor/superconductor heterostructure (indium arsenide-aluminum) where a nanowire is defined by means of electrostatic gating. The main idea is to create an interferometer between said nanowire and a long quantum dot which is is turn coupled to a resonator to perform quantum capacitance measurements in a reflectometry setup.The concept that the authors want to test is whether the quantum capacitance of the central quantum dot depends on the nanowire (through a state-dependent shift) and how the flux periodicity of this quantum capacitance can be used to perform fermion parity readout of the nanowire.

The experiments are very interesting, no doubt, but it is not completely novel:

as acknowledged by the authors themselves, a similar concept was already tested in Ref. 45 (coherent transport through a gate-defined nanowire island in an Aharonov-Bohm interferometer)
Another important part in the device design is to include two smaller quantum dots (1 and 3) that effectively allow fine tuning of the couplings (they can be used, for instance, to effectively interrupt the interference loop). Again, this idea is not completely new: a similar concept was proposed in Refs. 74 and 75 and used in Ref. 76 on dc transport measurements of the coupling between a quantum dot and a zero-energy state in a nanowire. This is also acknowledged by the authors (here, I also want to point out that subsequent experiments from the authors of Ref. 76, actually the ones where precise fittings of couplings where performed, should be cited, Deng et al, Phys. Rev. B 98, 085125, 2018).

That said, the novelties in the device design and concepts are sufficiently novel to warrant consideration for publication of the manuscript. In the case of (1) the geometry presented here allows to perform single shot parity readout. In the case of (2) the rf-based readout allow to extract the couplings with a much better resolution.
Concerning data, the authors seem to make an appropriate use of statistics: they present long sets of consecutive measurements of the resonator response, with state-of-the art integration times, etc. The distribution of the extracted quantum dot capacitance is clearly a bimodal distribution of two well-separated Gaussians. For a range of of gate voltage that controls the central QD detuning (red box in Fig 3a), the flux dependence is found to have h/2e periodicity. The experiment also allows to extract dwell times (longer than 1 ms) between the two associated states in the quantum capacitance.

After a thorough analysis of the data and comparison with simulations, the main conclusion is that the results are consistent with a measurement of the fermion parity encoded in a pair of Majorana zero modes in a long topological nanowire.

In my opinion, these experiments are very interesting and certainly relevant for the condensed matter community working on topological superconductors and Majorana states. What I do NOT like is the way the article is written which, sometimes subtly and sometimes more crudely, uses a language and wording that at all times leads the reader to think that we are dealing with a measurement that demonstrates parity in a topological qubit based on Majorana states. The examples are many and here I highlight only a few:

Page 1: lines 52 and 53 "albeit in a regime that does not allow qubit readout". Here the authors seem to imply that in this manuscript a qubit (and its parity readout) is demonstrated which is not true.
Page 1: “In this paper we demonstrate an interferometric measurement of the parity of a near-zero-energy state in a 1D Nanowire, thereby validating a necessary ingredient of topological quantum computation” How is this demonstrated? How do the authors know that this is a 1D system? Etc.
Page 1: lines 62-68 "By itself, this measurement does not unequivocally distinguish between MZMs in the topological phase and fine-tuned low-energy Andreev bound states in the trivial phase, but it does require the low-energy state to be supported at both ends of the wire and very weakly coupled to other low- energy states". This is an interesting sentence, of course I fully agree with it given its importance, but then the authors cite a series of papers related to Andreev qubit physics [49–53] but not THE relevant references concerning the trivial Andreev versus Majorana controversy. These papers , which are cited later in the paper, Refs [98-103] should be cited and highlighted in this important introductory paragraph. Moreover, the Nature Review Physics discussing in great detail such controversy, Nature Reviews Physics 2, 575 (2020), should be cited.
-Page 1 section2:
Already the title of the section is misleading “Topological qubit…” There is no definitive proof of topology or qubit…
-Line 76: “in this work we introduce a topological qubit design…” the same as above.
-Line 80-82: “The first component is a nanowire, sections of which can be tuned into a 1DTS state, leading to topological degeneracy of the many-body ground state…”
Page 2:
-Line 104, line 111: “To form a qubit…” “ a full qubit device therefore consists of…” again the same wording which implies that the authors demonstrate a qubit.
-Line 118: “ we focus on the left topological section…”

I could continue since the text is plagued with such misleading and ambiguous wording where theoretical prediction, device design and actual proof in experiment/data is mixed in a rather careless manner….

Things improve where the actual experiment starts to be explained (Line 121 “Our readout circuit is based on…” and I urge the authors to use this tone along the whole paper and to stay as agnostic as possible when referring to devices and measurements (specially in the initial introductory parts) and discuss the possible implications and/or their interpretation in terms of modeling only after presenting the data but not the other way around. Of course, possible alternatives and interpretations should be clearly discussed on the same footing (not hidden in some paragraph or buried in some appendix of the supplemental….).

Also in page 2, Fig 1 is extremely confusing since parts of the gates (purple and yellow) of panel c, extend all the way up to panes a and b (they shouldn’t be there). Moreover, panel b doesn’t fulfill its purpose since we just learn that the QD avoided crossings depend on the wire parity but not why.

Page 3, Parity measurement and interpretation section.
Given its importance, it would be very useful for readers to have a brief explanation of how the quantum dot C_Q quantum capacitance is extracted from the resonator response. How does this differ from standard reflectometry of, for example, a standard quantum dot? Already at this level of the article it would be very important to discuss if the design allows to rule out the possibility of an accidental quantum dot level (leading to low energy subgap physics) in the nanowire. There are many examples, at least in dc transport, where such quantum dot levels have been mistaken as Majoranas.

Page 4, line 277 “We interpret these h/2e-periodic…as originating from switches in fermion parity. Such switches have been observed in mesoscopic superconducting devices, where they were triggered by nonequilibrium QPs…” Indeed such parity switches are ubiquitous in superconducting devices. The obvious question is then: why the modeling that follows (and its rather restrictive assumptions like e.g. “the wire is in the topological phase and there are no subgap states other than the MZMs” is a valid explanation and not a more mundane one? A counterargument to my question above would be the various control experiments presented in page 6 but in my view any low-energy excitation in the wire, not necessarily of topological origin, could lead to the same result. (The model in Eq. B1 is extremely simplified!)

Page 5, related to my previous comment about the simplified modelling. Other low-lying excitations/subgap levels in the wire could lead to a rather involved interference pattern affecting the quantum capacitance in Eq. 1. Can the authors comment on this?

Below, in line 304, the authors write “For detailed comparison with experiments, we simulate a more complete model of our interferometer, expanded to include the full triple-dot system, incoherent coupling to the environment, and backaction from the measurement. As before, we neglect all states in the wire except the MZMs.” I don’t understand the meaning of “more complete model” here: although they are including more (external) effects, the main assumption of a single fermion mode made of two Majoranas is still included by hand.

Page 8: line 516: experiments form the Katsaros group should be cited together with Ref [106]: Valentini et al, Science 373 (6550), 82-88, 2021; Valentini et al,Nature 612 (7940), 442-447 (2022)

Page 8: Discussion and outlook
Again the same kind of sentences, line 545-547”We have presented dispersive gate sensing measurements of the quantum capacitance in topological qubit devices designed for the readout of fermion parity shared between MZMs at the opposite ends of a nanowire.” Please be precise in what is proven and what is not.
I agree that the experiments shows “flux-dependent bimodal random telegraph signal (RTS) in the quantum capacitance. We

interpret this RTS as switches of the parity of a fermionic state in the wire. The long switching time τRTS > 1 ms suggests a low quasiparticle poisoning rate, which we find to be within an order of magnitude of the quasiparticle density measured in a Cooper pair box device." (Lines 553-559).
The next paragraph is interesting since the authors acknowledge that their experiments are not conclusive "These measurements do not, by themselves, determine whether the low-energy states detected by interferometry are topological. However, by fitting to a model of trivial Andreev states, we have tightly constrained the properties that such states would have to have in order to be consistent with our data. To fully resolve this issue, we will discuss the device's phase diagram and the stability of the observed flux-dependent RTS in a separate publication [54]." (Lines 568-574). How can we then know that the interpretation is correct, since this much-needed check is left for another publication with additional data to which we do not have access?

In summary, I have no great criticisms of the experiment and the data (which by themselves constitute a good piece of the state of the art of single-shot parity measurements in a hybrid device and their dependence on flux), but rather in the rather misleading way in which these data are presented and the extreme simplifications of the modeling which, essentially, asumes a topological state and includes Majoranas by hand, yet again forcing an a priori interpretation of the data.

(Remarks on code availability)

Referee #2

(Remarks to the Author)
In "Interferometric Single-Shot Parity Measurement in InAs-Al Hybrid Devices" the Microsoft Azure team presents interferometric measurements on a triple quantum-dot device shunted by a hybrid superconductor/semiconductor nanowire. The quantum capacitance of the large central dot is found to exhibit h/(2e) flux periodicity. The statistics of the quantum capacitance is strongly bimodal, suggestive of two distinct states with different quantum capacitance. Dynamics is measured of these states, with a typical timescales of milliseconds. The authors offer an interpretation that the two detected states are nearly degenerate fermionic parity states in the shunting wire, each individually with h/e flux periodicity. Detuning the dot energies changes both the strength of the interferometric signal and the energy of the fermionic state in the wire. The authors suggest that their observations are consistent with single-shot parity readout of well-separated Majorana zero modes.

This manuscript has the "wow" factor that one would expect for Nature. The data support the authors' claim of single-shot parity measurements of near zero-energy states. The parity-poisoning time is quite low, which is a rare bit of good news for the field. The exact relationship to Majorana is still not completely certain. However, this is still the highest bar reached so far in terms of tests for Majorana physics and, as the authors state, places strong constraints on alternative hypotheses. Most important for me is that the authors are working with a device geometry which, although extremely challenging, will plausibly allow tests of fusion rules in the near future.

As stated above, the relationship to Majorana physics is not completely certain and needs some serious scrutiny. My two major points along these lines are:
(1) More discussion is needed on the how often this procedure works, and under what conditions. I appreciate that the authors intend to present thorough comparison between Cq and TGP measurements elsewhere. On the other hand, the authors themselves present an additional dataset "A2", tuned with TGP, as a support of the reproducibility of their result. This isn't meaningful without at least giving some context. 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? Similar questions for Device B. The authors need to give us some context to make the control experiments meaningful.
(2) Based on the studies in Fig. 6/7, the zero energy state appears to be quite fragile. Is it plausible that Em should change by 6 μeV when dot plungers are changed? Does it indicate improvements are needed for fusion rules to succeed? Measured by the usual standards of ZBP-ology these changes are small. However, one could have hoped that a candidate Majorana identified in transport would be found to be much more stable now that a more sensitive detector is available. Without arguments to the contrary, I would take this instability as evidence against Majorana.

I have a few minor points:

-The phrasing in the introduction "By itself, this measurement does not unequivocally…" is quite clear and fair. I find the following paragraph "In this work we introduce a topological qubit design that allows one to perform projective measurements of fermion parity encoded in MZMs" to be inaccurate and completely confusing. Stated at the level of "In this work" this could easily be read as a claim that a topological qubit has been demonstrated. The discussion also mixes realization-specific details (ie 9.1 nm thick InAs quantum well) with very general, aspirational statements of what this device could possibly do under unexplored circumstances. The proceeding discussion of what needs to be done "to form a qubit" is even more confusing.

It's understandable to point out that this device geometry could eventually make a qubit. However, the current level of detailed discussion is completely distracting. Given the complexity of the geometry, it's also incredibly difficult to follow. I request that the authors almost completely eliminate the discussion of how to tune up a qubit in this geometry, or justify why this I needed. I would also insist that the authors use care in language in whatever brief discussion they have on this point. For instance, saying "Here, we focus on the left topological section…" makes a stronger (and too strong) claim than the introduction and abstract.

-The authors state “The material combination and dimensions have been optimized for values…”. I request that they supply some more details on this statement. What are the techniques used for these optimizations? It should at least be clear if this is based on experiment or theory.

-The discussion of the tuneup procedure in the main text is difficult to follow. The statement “We tune dot 2 to charge degeneracy and use the TGP to select a magnetic field and a VWP1 for our measurements” makes it unclear what configuration the device is in during the TGP, and even if transport is being measured for it. It also needs to be stated if the TGP succeeds in multiple regions and how this one is chosen.
-The statement “The observed behavior is consistent with the random matrix theory prediction for a disordered quantum dot” seems overly strong, unless some statistical analysis is presented. I believe the authors simply mean this at a qualitative level, but the statement doesn’t reflect that.

-The phrasing of the sentence “We interpret these h/2e-periodic bimodal oscillations and RTS in Cq as originating from switches in fermion parity.” is prone to misunderstanding. It naively implies that the entire phenomenon is due to parity switches, rather than the h/2e as opposed to h/e periodicity (which is what I think the authors mean, although I’m still not totally sure).

-In Appendix E I found the location of Ohmics S1, S2 unclear. Can the labels be improved, or arrows drawn?

-The legend in Fig. 16i appears to have a mistake. The +/- reads 0.0 ms.

(Remarks on code availability)

Referee #3

(Remarks to the Author)
The manuscript "Interferometric Single-Shot Parity Measurement in InAs-Al Hybrid Devices" by Nayak and coworkers reports the interferometric measurement on the parity of an InAs-Al island, a step toward realizing measurement-based topological quantum computation (TQC). The authors couple the island to three quantum dots in series as a reference arm to form a loop geometry. By tuning the relevant parameters (magnetic field and gate voltages) into the right regime, they create two zero-energy states at the island edges. The coupling between these states and the reference arm is modulated by the detuning of dot-1 and dot-3, while dot-2 is tuned to the degeneracy point, whose quantum capacitance depends on the parity of the island and the magnetic flux threading the loop. This quantum capacitance is measured using an rf-reflectometry technique by coupling dot-2 with an LC oscillator. The authors then observe Aharonov-Bohm-type oscillations of the capacitance, which is associated with coherent electron interference through the reference arm and the two end states. The oscillation period is h/e for each parity branch, and the lifetime of each parity state is extracted to be the order of milliseconds. The authors claim that their observation is consistent with the hypothesis that the two end states in the island are Majorana zero modes (MZM). They further support this claim by theory simulations and some experimental control tests, such as those conducted at lower magnetic fields or with the reference arm decoupled from the island.

The field of MZM and TQC has been in intense debates since its birth. The definitive proof of MZM would be a braiding experiment, which depends critically on two factors: material advancements and the design of the outer braiding circuit (the technique). The manuscript did not report any material advancements that would lead to new or stronger MZM signatures. Similar physics has already been demonstrated in transport experiments, e.g. Albrecht et al, Nature 531, 206 (2016), Whiticar et al, Nature Comm. 11, 3212 (2020), and PRB 107, 245423 (2023) from the same group. Moreover, the parity lifetime of a subgap state in hybrid islands has also been indirectly extracted in transport experiments, see Higginbotham et al, Nature Physics 11, 1017 (2015). Therefore, the novelty of this manuscript does not lie in providing stronger evidence for MZMs, but in its methodological approach: it demonstrates that rf-parity readout “can be done” within this complicated loop geometry, which requires a lot of tune-up and parameter control. In other words, if the other three superconducting segments in Fig. 2(b) (on the right part) are operational, then a braiding experiment involving four zero-energy states could be straightforwardly executed, allowing at least a Z-operation to confirm non-Abelian statistics (and of course also their MZM nature). Given the significance of TQC and the technical challenges encountered, I consider this manuscript worthy of publication in Nature. However, before recommending acceptance, the authors must address the following concerns:

1) Are the other three superconducting segments operational or not? If not, why?

2) Given that this experiment cannot firmly exclude a fine-tuned zero-energy state, the abstract’s statement that “these results are consistent with … Majorana…” without mentioning alternatives could be misleading. Especially considering that this may be the take-home message in press release and media coverage. The authors should revise their abstract to prevent potential misinterpretation. For example, I can imagine that the results may also be consistent with the scenario proposed by Hess et al, PRL 130, 207001 (2023).

3) I am confused by the geometry of the Al strip in the device. Is the Al strip continuous (without breaks) across all the five superconducting segments (spanning over 10 micron)? Or is it etched into disconnected islands (such as the 2.5-micron island)? From the device schematics and SEM, it seems to be the former. If so, how is there still a charging energy, given that this long strip is connected to D1 and D2?

4) During the device tune-up, between step-3 (topological gap protocol) and step-4 (tuning the TQDI loop), how can the

authors ensure the device remained within the regime that passes TGP when they performed the dispersive sensing measurement? Given the variations in gate voltages between rf sensing and DC transport, the cross-talk is not negligible especially considering the tiny parameter space of TGP in their previous PRB work.

5) The Al strip is only 60-nm wide; lithographic inhomogeneity should be a serious disorder source. It is hard for me to believe that the 2.5-micron long island could maintain a small topological gap across the whole island considering this lithographic disorder. Could the authors elaborate on this?

6) In Fig. 3(a), why is there a shift in B for the kurtosis at V_QD2 = 0.5 V and 0.7 V? If E_M is not zero, then why is shift absent at other V_QD2 values in the same figure?

7) Have the authors observed the “bowtie” or “diamond” pattern (see Prada et al, PRB 96, 085418 (2017)), typical for dot-coupled Majoranas? If not, why?

Corrections of typos are also necessary:
Line 171: Fig. 2b should be Fig. 1c.
Fig. 22: panel b mislabeled as g

(Remarks on code availability)

Referee #4

(Remarks to the Author)
This work addresses the problem of measuring the fermion parity of Majorana zero modes (MZMs) in a 3-micrometer-long Al/InAs nanowire. This measurement capability is important since it can eventually enable measurement-based quantum gates as well as the readout of Majorana qubits. The reported parity-measurement technique consists in detecting variations in the quantum capacitance C_Q of a long InAs quantum dot connected to the edges of the Al/InAs nanowire via two smaller quantum dots providing gate-tunable links. The quantum capacitance is probed by rf gate reflectometry, which enables time-domain detection with microsecond time resolution.
In a narrow range of experimental parameters (in-plane B-field, V_PW1), the Microsoft team finds that C_Q vs out-of-plane B-field is switching on millisecond time scale between two oscillating branches. The two branches exhibit counter-phase oscillations with a periodicity corresponding to h/e magnetic flux across the loop formed by the Al/InAs nanowire and the series of three InAs quantum dots. These Aharonov-Bohm oscillations are interpreted as a signature of single-electron interference via MZMs, with quasi-particle poisoning in the Al superconductor inducing stochastic switching in the occupation (i.e. the parity state) of the MZMs.

On a mere scientific level, this is the main result of the paper. It confirms previously reported observations from a dc transport experiment in a similar type of system (Ref. 45). Here the novelty lies in the detection of stochastic parity switching, while in Ref. 45, pi phase shifts in Aharonov-Bohm oscillations were observed as a result of gate-induced changes in the occupation parity of the Al/InAs nanowire island.
On a technical level, the present work introduces microsecond-scale, time-domain parity detection. This is achieved by means of dispersive rf gate reflectometry, a technique routinely used for the readout of semiconductor qubits. The reported SNR of 1 on a 3.7 microseconds integration time is in line with typical values for gate-based sensing in semiconductor quantum dots (see e.g. Schaal et al, Phys. Rev. Lett. 124, 067701 (2020)).

The authors claim their work is a significant step toward the realization of topological qubits. To my view, however, the reported achievements do not meet Nature’s novelty and relevance standards. Moreover, the conclusions drawn in this work rest on questionable hypothesis and methodologies. As a result, I cannot recommend publication in Nature.
In the following I will motivate this judgement.

The observation of parity switching is indeed new for the specific case of Al/InAs nanowire systems. It is ascribed to quasiparticle poisoning with a characteristic time scale of ~1ms. If topological qubits were ever to be realized, this time scale would set un upper limit on their lifetime. This ms time scale is not that long if compared to the coherence times achieved in superconducting or semiconducting qubits, which are way simpler to operate and do not suffer from certain known limitations of Majorana qubits (e.g. the lack of protection against errors from non-Clifford gates).
I would generally agree with the view that performing single-shot parity measurements on a microsecond time scale is an important technical achievement along the way to realizing quantum computing architectures based on topological qubits. Yet, one should note that this achievement is specific to the chosen physical platform, i.e. Al superconductor on an InAs/InGaAs semiconductor heterostructure, and that, contrary to the view of the Microsoft team, the realization of topological superconductivity in this platform is still a subject of debate in the community. Similar to many earlier works, here it is shown that the experimental results are consistent with a model involving MZMs localized at the edges of the InAs nanowire. This consistency, however, does not rule out other possible scenarios with no topological superconductivity.

The interferometry experiment is carried out in narrow range of parameters controlling the electronic properties of the Al/InAs nanowire, i.e. the gate voltage V_WP1 and the in-plane magnetic field. Apart from some punctual checks outsides this parameter range, no clear view is offered of what is happening elsewhere. The storyline of the paper is centered on the physical picture of a topological state of the nanowire island, which, according to the authors, is establish following the topological gap protocol (TGP). The last sentence at the end of section 2 reads "Complete details of tuning the nanowire into the topological phase will be discussed in [54].". I find this simply unacceptable. Moreover, the TGP has been developed by the Microsoft team and it is not generally accepted by the scientific community.

Finally, I have some further more specific remarks and questions:

1) The interferometry data in Fig. 3b and Figs. 7a-c are shown on a relatively narrow B_perp range. It would instructive to see a broader field scan, including negative-field data.

2) In Fig. 7, a tiny variation (< 1mV) in the gate voltage V_WP1 has a very large impact on the measured interferometric signal. I suppose this corresponds to tuning around a given charge degeneracy point of the Al/InAs nanowire island. Is this behavior confirmed at other charge degeneracy points. On what V_WP1 range can this be reproduced?

3) In Section 5, the authors state: "It is worth noting that this method enables us to probe the Majorana splitting energy E_M with single-μeV resolution." Once again, this conclusion rests on the validity of the assumptions underlying the fitting model, which remains to be established.

4) The Cooper-pair-box control experiment confirming the quasiparticle poisoning rate was performed a zero magnetic field, as opposed large in-plane magnetic field (~2T) used in the measurements of parity switching in Al/InAs nanowire island. This difference can significantly impact the comparison.

5) The authors use first and third quantum dots as tunable barriers. I would expect their role to be more complex than that and their behavior to depend not only on their effective charge occupancy, N_g1 and N_g3, but also on their multi-particle electronic states, which can drastically change from one Coulomb valley to another. This point is not discussed.

(Remarks on code availability)

Version 1:

Reviewer comments:

Referee #1

(Remarks to the Author)
I stand by my previous report and, in fact, I must now be harsher since after one round of evaluation the authors have had the opportunity to rewrite the article carefully and avoiding the continuous mixing of objective facts with interpretation (sometimes bordering on a strong bias towards a priori conclusions on the part of the authors) but have largely ignored my suggestion (and also that of other referees).

Let's get to the facts. As I wrote in my previous report, this article certainly presents interesting experiments that deserve to be published and should motivate the community. Whether the data have the necessary novelty and relevance (beyond doubt) to be published in Nature is another matter, as I argue in what follows:

1) After quite a bit of narration and two completely schematic figures in which we learn little (except that the wire supposedly contains two Majoranas), the authors finally show us data in Figure 3. Unfortunately, these plots are the only core experiments on which the authors base their conclusions (since Figure 4 corresponds to a simulation).
2) Let's go to this data. Figure 3 shows the flux-dependent quantum capacitance measurements of the TQDI system comprised by the nanowire, the central quantum dot 2 and the two smaller dots 1 and 3 used for tuning the couplings. These data correspond to using "the TPG to select a magnetic field and a gate range for our measurements". First, we never learn what TPG stands for in the main manuscript (Topological gap protocol) and second, more serious, is that this data corresponds to a single point in parameter space of such TPG (specifically, a parallel magnetic field of 1.8T and a nanowire gate voltage of -1.832 V). After this data is presented (in particular the kurtosis of time-dependent traces with parity switches which allows to extract a bimodal distribution and typical switching times of around 2 milliseconds) the rest of the paper focuses on interpretation using Eq. (1) which is obtained after several approximations and strong assumptions, notably the existence of Majorana zero modes with a small splitting EM. Notably, the tunneling couplings t_L and t_R entering the flux-dependent tunneling in Eq (2) depend themselves on a strong assumption (a minimal model of QDs 1 and 3 coupled to Majoranas…).
3) The authors claim "demonstration of reproducibility" but, to my surprise, their analysis is never performed systematically for other points of the so-called TGP phase diagram (which is by itself very narrow, see Fig S7, for regions with ZBP and gap). Only another measurement A2 is shown in another single point in the phase diagram (Fig S9) and in another single point for another device (Fig S10). In my view, this does not rule out near zero modes of non-topological origin nor demonstrates reproducibility. If we are talking about topological zero modes, data like the one presented in Fig 3 should occur in large regions of nanowire parameter space (magnetic field/gate voltage).
4) Perhaps rephrasing my previous question a bit: how often do the authors find phase coherent parity switches in their setup? to what extent are they robust against parameter variations? to what extent is the phenomenon reproducible? etc....
4) Some other comments:
-Important concepts like the quantum capacitance (page 1) and the basic ideas behind the measurements (dispersive gate sensing of the resonator, etc) are never discussed. In this context, I also think other papers discussing this kind of rf-based

parity measurements (RF charge sensing from 2e to 1e, etc) should be cited (e.g. Phys. Rev. Applied 11, 064011, 2019) or papers with quantum dot-based Andreev subgap states where similar parity dynamics and bimodal distributions due to quasiparticle switches have been demonstrated (e.g. PRX Quantum 3, 030311, 2022)

Given my overall impression and comments above, I cannot recommend the paper for publication in Nature.

(Remarks on code availability)

Referee #2

(Remarks to the Author)
I thank the authors for their efforts. All of my comments were addressed. The new manuscript is much more readable than the previous one, and most importantly I don't see any places in the discussion where aspirational statements of future experiments get mixed with the current observations.

There is one additional concern that I hope the authors can address. In S4.4, there is a description of the tuneup that I believe was added in this round. The text indicates that QC1/QC2 have a large cross capacitance to the wire, whereas TG1/TG2 do not. This is so strange that it makes me wonder if something is wrong. Is it really the case that these gates couple so strongly to wire states? As I read the design, they should be strongly screened compared to TG1/TG2. Could the authors add some clarity in the supplement? If this cross-coupling is unexpected, I ask that they say so.

-- Comments on reports of referees #1 and #4 ---

These reviews certainly reflect the divided state of the field. The short summary is that I find Referee #1's points reasonable but don't think they fundamentally undermine the study's claims. I disagree with most of Referee #4's points.

All referees are in agreement that the central result here is not a definitive yes/no on the Majorana question. Some of the harsher criticisms approach a Majorana straw man argument, implying that the manuscript makes a definitive claim, and then faulting its strength.

I think that Referee #1’s point that the authors could have taken data for more nearby parameter ranges is reasonable. I do not agree that it significantly undermines the study or its claims to reproducibility. The authors ran an automated tune-up procedure on two devices and saw similar results. This is a level of reproducibility that could even be compared with a semiconductor spin qubit. Although I do agree it would be nice to see some more parameter dependence, I can understand the authors’ choice to fix parameters based on a pre-defined criteria and tune up from there.

I do not agree that if the modes are topological “data like the one presented in Fig 3 should occur in large regions of nanowire parameter space”. It is subjective what counts as large, but the regions identified by the TGP would be considered “small” by most people. That doesn’t mean they aren’t topological and worth of study, just that there is strong competition with disorder.

The referee is of course correct that TGP and several other concepts were not defined. This really should be improved.

Referee #4 emphasizes that the current manuscript does not give a definitive answer to the Majorana question. All referees and the submitted manuscript seem to be in agreement on this point.

I do not agree with the referee that this is not solid progress. The “wow” factor here is precisely that interferometric, single-shot parity readout actually works, and even more so in a challenging architecture that is compatible with future tests of fusion rules. I still find it amazing that this was actually possible. Looking ahead, the key point is that this result positions the field to moves beyond endless debates of “robustness” of transport features.

I do not agree with Referee #4 quoting Kouwenhoven’s perspective on arXiv, and meanwhile holding Kouwenhoven’s retractions against the present Microsoft team. These researchers were not involved. They have introduced automated tuning methods to identify promising regions precisely to avoid the pitfalls of Kouwenhoven’s earlier work.

(Remarks on code availability)

Referee #3

(Remarks to the Author)
The authors have addressed all points of the previous referee reports. In my opinion, the paper is ready for publication.

Comments on critical reports of referees #1 and #4

I have read carefully the reports of referees #1 and #4, below are my comments:
I disagree with referee #1's opinion on "continuous mixing of objective facts with interpretation, sometimes bias towards a priori conclusion". In fact, the authors have discussed trivial explanations in the abstract, introduction, main text, and conclusion parts (pretty much everywhere in the manuscript). Thus, the manuscript is quite balanced. The rest concerns of referee 1 have actually been addressed in the manuscript supplement. E.g. 1) the referee criticizes that only Fig. 3 is experimental data. In fact, Figs. S6, S7, S9, S10, S11, S12, S13, S14, S16, S17 are all experimental data with very rich physics. 2) The referee criticizes the meaning of TGP, which has been elaborated in detail in session S4 in supplement. 3-4) The referee criticizes the lacking of control tests and reproducibility, which have been systematically shown in sessions S5-S7 in supplement. Referee #1 is correct that the concept of rf-based parity measurement is not new. But compared to previous works, the architecture in current manuscript is much more complicated/advanced and goes way beyond: This architecture can enable a braiding experiment, i.e. a final proof of Majoranas.

Referee #4 first quotes Leo Kouwenhoven's recent perspective article and Daniel Loss' PRL (Hess et al) to criticize the TGP. First of all, TGP is not the main focus of this manuscript, thus the criticism is not much relevant. I know the two papers above very well. The referee quotes many points from Kouwenhoven's perspective article, in fact, Microsoft's approach was listed as one of the four promising directions (the brighter future) in that perspective article. The referee describes the retracted papers on this topic as "proven scientific misconduct". This is a false statement and unfounded. On the contrary, in Kouwenhoven's perspective article, he has explicitly discussed this and provided the official conclusion from Delft University of Technology and Dutch national committee of scientific integrity, i.e. no violation of integrity, no misconduct, and no intentional mistakes. Referee #4 provides no comments on the scientific content of the manuscript, except for the novelty of the rf-based technique. I have discussed on this in the previous paragraph.

To summarize, the authors have addressed all scientific questions raised by the referees. The interferometric single-shot measurement presented in the manuscript is novel and impressive. More importantly, it can enable braiding of Majoranas, serving as an important step towards settling the debates and controversies on this topic. Thus, I fully support its publication in Nature.

(Remarks on code availability)

Referee #4

(Remarks to the Author)
Following my comments and those from the other referees, the authors have improved their manuscript correcting for some the weaknesses identified in the originally submitted version. Despite that, my main criticism on the relevance of this work remains unaffected. This work rests on an extremely fragile ground. As I already pointed out, the topological-gap protocol (TGP) is not accepted by the community. This I could conclude from private discussions with many experts in the field. The widely spread skepticism, to my view fully justified, is also explicitly emerging in some recent publications. For instance, in a recent preprint (arXiv:2406.17568), Leo Kouwenhoven (former Microsoft team member) writes "The protocol, however, contains hidden assumptions that may not correspond to the real device situation. Disorder-induced ABSs and smooth barrier effects together can give false-positives on the topological gap protocol [73,74,75]". Reference 73 (Hess et al., Phys. Rev. Lett. 130, 207001) reports numerical simulations showing that nanowires hosting trivial Andreev states can still pass the Microsoft TGP. By admitting that other more trivial mechanisms cannot be ruled out, even Microsoft researchers do not seem to be fully confident in the reliability of their protocol. As a matter of fact, the supposedly topological regimes identified in the new Figs. S7 appear as ephemeral, not quite exhibiting the robustness typically expected of topological features.

I would like to stress the fact that the present manuscript comes after a decade of focused and well-funded research. The Microsoft team has been producing a fairly large amount of work often published in high-visibility journals, including many from the npj family. Most of the claims made are still controversial, a number of papers have raised significant criticism, and some were eventually retracted due to proven scientific misconduct. As a result, a clear, unambiguous observation of topological superconductivity and Majorana edge modes is still missing. In his perspective paper, Kouwenhoven writes "The signals of these states in local and non-local transport can mimic almost perfectly the predicted signatures for MBS in the nanowire-Majorana model, making it very hard to distinguish topological from trivial states. I cannot exclude that InAs/Sb-based hybrids will ever fulfill the requirements for the nanowire-Majorana approach, but I doubt it." Nowadays, the scientific community, or at least a good part of it, is no expecting "wow" factor papers but some true progress: if not a demonstration of a Majorana qubit, some solid evidence not relying on the agreement or compatibility with models based on debatable assumptions. This has been the case for pretty much all of the previously published works, and the present paper makes no exception to this trend, as it is also unanimously noticed by the other referees.

Referee 1 says: "In summary, I have no great criticisms of the experiment and the data (which by themselves constitute a good piece of the state of the art of single-shot parity measurements in a hybrid device and their dependence on flux), but rather in the rather misleading way in which these data are presented and the extreme simplifications of the modeling which, essentially, assumes a topological state and includes Majoranas by hand, yet again forcing an a priori interpretation of the data.", and also "There is no definitive proof of topology or qubit. . .".

Referee 2 says: “As stated above, the relationship to Majorana physics is not completely certain and needs some serious scrutiny.”

Referee 3 says: “the novelty of this manuscript does not lie in providing stronger evidence for MZMs”.

On a technical level, I already acknowledged a certain level of advance, but I estimated it well below the bar for publication in a high-impact journal like Nature. I pointed out that reflectometry technics are a standard practice in solid-state qubits. Let me add that the present work is not even the first one where dispersive reflectometry readout is applied to hybrid superconductor-semiconductor devices expected to provide a path to topologically protected qubits. See e.g.: Ramadze et al., Phys. Rev Appl. 11, 064011 (2019); Nguyen et al., Phys Rev B, 108, L041302 (2023) ; Hinderling et al., arXiv:2307.06718. Surprisingly, none of these papers is cited.

Due to the above reasons, I am firmly convinced this work should not be published in Nature or any other high visibility journal.

(Remarks on code availability)

Dear Editor,

We are hereby resubmitting our manuscript titled "Interferometric Single-Shot Parity Measurement in InAs-Al Hybrid Devices" for your consideration at Nature. Per your guidance, we have condensed the text and addressed the comments/questions of the referees.

We would like to thank the referees for their thorough review of our manuscript and the constructive feedback. It is encouraging to note their recognition of the novelty and significance of our work. For instance, Referee #1 acknowledges the innovation in our device design and concepts as being sufficiently novel for publication consideration. Referee #2 expresses that our manuscript delivers the 'wow' factor expected for Nature, highlighting our work in a challenging device geometry that could enable tests of fusion rules shortly. Similarly, Referee #3 remarks on the potential for straightforward execution of a braiding experiment involving four zero-energy states if the other three superconducting segments are operational, deeming our manuscript worthy of publication in Nature due to "the significance of TQC and the technical challenges encountered". Referee #4, while not recommending publication in Nature, did recognize the "important technical achievement along the way to realizing quantum computing architectures based on topological qubits."

We have taken the feedback seriously and have made substantial revisions to address the concerns raised. Specifically:

- We have added a section in the supplementary material detailing the tune-up procedure, including Topological Gap Protocol (TGP) measurement data and tuning between TGP stage and rf readout of fermion parity stage, as outlined in Sec. S4.3.
- As requested by several referees, we have revised the manuscript to more carefully separate the underlying design concept from the actual measurements and their interpretation.
- We have expanded the discussion on alternative interpretations of our data in both the main text and the Supplementary Material.
- In response to several specific comments of the referees, we have clarified the distinctions between this work and prior studies, especially those in Ref. 45. Our comprehensive response below further clarifies these points, and we believe it effectively addresses the criticisms of Referee #4.
- We have significantly shortened the paper by transferring several topics to the Supplementary Material, particularly the discussion of additional measurements and consistency checks that bolster our interpretation.

We believe that the extensive revisions, informed by the referees' insightful comments, have significantly improved our manuscript. We hope that you will find our revised manuscript meets Nature's publication standards. Enclosed, you will find a summary of the major changes and a point-by-point response to the referees' comments.

Sincerely,
C. Nayak (for the authors)

1. OVERVIEW OF MAJOR CHANGES

- We have significantly shortened the main manuscript and expanded the Supplementary Material. We moved all of the old Section 4 to the Supplementary Material, where it is now Sec. S6. We moved the discussion of our consistency checks, such as quasiparticle injection, into the Supplementary Material.
- Based on the feedback of the referees, we have expanded our description of the device tuning procedure, and include measurement results from the Topological Gap Protocol (Sec. S4). We removed a reference to a future paper since the relevant Topological Gap Protocol data is now in this paper.
- Throughout the manuscript, we have softened the language around our claims and have been more careful to separate the design concept from measured results.
- We have extended the discussion of the trivial scenario based on analysis of a model of quasi-MZMs.
- We have changed $B_\perp$ to B_x to clarify the field sweeps, in response to referee 4's comment.
- We corrected a typo: the Al strip is 3.5 nm thick, not 6.5 nm.
- We have corrected a factor of 2 in the charging energy of the Cooper pair box measurement (presented in Sec. S8 of the revised Supplementary Material). This leads to a change in the estimated quasiparticle density by a factor of $2\sqrt{2}$, but does not alter any of the conclusions drawn from the data.
- We have corrected a factor of $\sqrt{2}$ in the discussion of the expected signal-to-noise ratio (Sec. S2.6), which was due to an error in the earlier version.

2. REFEREE #1 (REMARKS TO THE AUTHOR)

This paper reports on interferometric measurements of fermion parity in a hybrid semiconductor/superconductor heterostructure (indium arsenide-aluminum) where a nanowire is defined by means of electrostatic gating. The main idea is to create an interferometer between said nanowire and a long quantum dot which is is turn coupled to a resonator to perform quantum capacitance measurements in a reflectometry setup.The concept that the authors want to test is whether the quantum capacitance of the central quantum dot depends on the nanowire (through a state-dependent shift) and how the flux periodicity of this quantum capacitance can be used to perform fermion parity readout of the nanowire.

The experiments are very interesting, no doubt, but it is not completely novel: as acknowledged by the authors themselves, a similar concept was already tested in Ref. 45 (coherent transport through a gate-defined nanowire island in an Aharonov-Bohm interferometer) Another important part in the device design is to include two smaller quantum dots (1 and 3) that effectively allow fine tuning of the couplings (they can be used, for instance, to effectively interrupt the interference loop). Again, this idea is not completely new: a similar concept was proposed in Refs. 74 and 75 and used in Ref. 76 on dc transport measurements of the coupling between a quantum dot and a zero-energy state in a nanowire. This is also acknowledged by the authors (here, I also want to point out that subsequent experiments from the authors of Ref. 76, actually the ones where precise fittings of couplings where performed, should be cited, Deng et al, Phys. Rev. B 98, 085125, 2018).

We agree with the referee's assessment that the Deng et al. paper is pertinent and have accordingly cited it. We would like to emphasize, however, that there are a number of important distinctions between Ref. 45 and our current work. Specifically, Ref 45 employs dc transport measurement of a nanowire island in the Coulomb blockade regime with fixed charge (and parity) due to its charging energy. This yields a time-averaged interference signal, while our manuscript reports a time-resolved measurement, enabling us to demonstrate single-shot fermion parity readout and extract parity switching rate. Such single-shot readout is one of the requirements for measurement-based quantum computation. Given these significant differences, we posit that our work introduces novel contributions and merits consideration for publication in Nature.

That said, the novelties in the device design and concepts are sufficiently novel to warrant consideration for publication of the manuscript. In the case of (1) the geometry presented here allows to perform single shot parity readout. In the case of (2) the rf-based readout allow to extract the couplings with a much better resolution. Concerning data, the authors seem to make an appropriate use of statistics: they present long sets of consecutive measurements of the resonator response, with state-of-the art integration times, etc. The distribution of the extracted quantum dot capacitance is clearly a bimodal distribution of two well-separated Gaussians. For a range of of gate voltage that controls the central QD detuning (red box in Fig 3a), the flux dependence is found to have h/2e periodicity. The *experiment also allows to extract dwell times (longer than 1 ms) between the two associated states in the quantum capacitance.*

After a thorough analysis of the data and comparison with simulations, the main conclusion is that the results are consistent with a measurement of the fermion parity encoded in a pair of Majorana zero modes in a long topological nanowire.

In my opinion, these experiments are very interesting and certainly relevant for the condensed matter community working on topological superconductors and Majorana states. What I do NOT like is the way the article is written which, sometimes subtly and sometimes more crudely, uses a language and wording that at all times leads the reader to think that we are dealing with a measurement that demonstrates parity in a topological qubit based on Majorana states. The examples are many and here I highlight only a few:

Page 1: lines 52 and 53 "albeit in a regime that does not allow qubit readout". Here the authors seem to imply that in this manuscript a qubit (and its parity readout) is demonstrated which is not true.

We changed this sentence to: "Such dc transport experiments allow time-averaged measurements of the fermion parity in the interferometer loop. However, to realize the proposals of Refs. 2, 40–44, time-resolved measurements of the fermion parity in the interference loop are required."

Page 1: "In this paper we demonstrate an interferometric measurement of the parity of a near-zero-energy state in a 1D Nanowire, thereby validating a necessary ingredient of topological quantum computation" How is this demonstrated? How do the authors know that this is a 1D system? Etc.

The referee raises important questions that a reader may ask. To answer them directly, we have added additional discussions of tuning to the single sub-band regime and the topological gap protocol in Sec. S4.1, Sec. S4.3 and Sec. S4.4. In transport measurements, we see the signatures of mesa depletion (formation of the quasi-1D nanowire) and wire depletion enabling tuning to the lowest sub-band. In the lowest sub-band, we run the TGP to identify the plunger gate voltage and B-field regime where we perform rf measurements.

Page 1: lines 62-68 "By itself, this measurement does not unequivocally distinguish between MZMs in the topological phase and fine-tuned low-energy Andreev bound states in the trivial phase, but it does require the low-energy state to be supported at both ends of the wire and very weakly coupled to other low- energy states". This is an interesting sentence, of course I fully agree with it given its importance, but then the authors cite a series of papers related to Andreev qubit physics [49–53] but not THE relevant references concerning the trivial Andreev versus Majorana controversy. These papers , which are cited later in the paper, Refs [98-103] should be cited and highlighted in this important introductory paragraph. Moreover, the Nature Review Physics discussing in great detail such controversy, Nature Reviews Physics 2, 575 (2020), should be cited.

We added these citations to this sentence, including the one suggested by the referee.

-Page 1 section2: Already the title of the section is misleading "Topological qubit…" There is no definitive proof of topology or qubit…

We changed the section title to "Device Design and Setup".

-Line 76: "in this work we introduce a topological qubit design…" the same as above.

We changed the wording in this sentence to "In this work, we introduce a device design that allows one to perform projective measurements of fermion parity, as required in Refs. 39-44."

-Line 80-82: "The first component is a nanowire, sections of which can be tuned into a 1DTS state, leading to topological degeneracy of the many-body ground state…"

We changed this to "The first component is a nanowire. If it is in a 1DTS state, there will MZMs at its ends. "

Page 2: -Line 104, line 111: "To form a qubit…" " a full qubit device therefore consists of…" again the same wording which implies that the authors demonstrate a qubit.

We have changed this to (emphasis added): "To realize a topological qubit, as outlined in Ref. 2, *it would be necessary* to tune the second and fourth segments, each of length $L \approx 3\,\mu\mathrm{m}$ long, into the 1DTS state while the other three would be fully depleted underneath the Al nanowire (see Fig. S1 in Sec. S1 for details). "

-Line 118: " we focus on the left topological section…"

We have changed this to "we focus on the second section..."

I could continue since the text is plagued with such misleading and ambiguous wording where theoretical prediction, device design and actual proof in experiment/data is mixed in a rather careless manner. . . .

We acknowledge these issues and have tried to address them throughout the manuscript.

Things improve where the actual experiment starts to be explained (Line 121 "Our readout circuit is based on. . . " and I urge the authors to use this tone along the whole paper and to stay as agnostic as possible when referring to devices and measurements (specially in the initial introductory parts) and discuss the possible implications and/or their interpretation in terms of modeling only after presenting the data but not the other way around. Of course, possible alternatives and interpretations should be clearly discussed on the same footing (not hidden in some paragraph or buried in some appendix of the supplemental. . . .).

Also in page 2, Fig 1 is extremely confusing since parts of the gates (purple and yellow) of panel c, extend all the way up to panes a and b (they shouldn't be there).

This is an issue of rendering in some PDF readers that we have tried to resolve in the updated manuscript.

Moreover, panel b doesn't fulfill its purpose since we just learn that the QD avoided crossings depend on the wire parity but not why.

We added a reference to eq. 2 in the figure caption to explain this.

Page 3, Parity measurement and interpretation section. Given its importance, it would be very useful for readers to have a brief explanation of how the quantum dot C_Q quantum capacitance is extracted from the resonator response. How does this differ from standard reflectometry of, for example, a standard quantum dot?

This is discussed in detail in Sec. S3.1.Our approach follows standard techniques that analyze the frequency shift due to the state of the quantum dot by comparing to a previously acquired reference measurement of the resonator response as frequency is varied. The extracted frequency shift can be converted to a change in capacitance relative to its classical value.

Already at this level of the article it would be very important to discuss if the design allows to rule out the possibility of an accidental quantum dot level (leading to low energy subgap physics) in the nanowire. There are many examples, at least in dc transport, where such quantum dot levels have been mistaken as Majoranas.

In our setup, the superconducting wire extends continuously over a distance greater than 10 microns and is eventually grounded. Also, the superconducting wire is strongly coupled to the underlying 2DEG to ensure an induced gap $\Delta_{\text{ind}} \sim 110\,\mu\text{eV}$, as elaborated in Sec. S1.3. The tunneling rate from the 2DEG to the normal metal is comparable to the parent gap, see Stanescu et al., PRB 84, 144522 (2011). Given the micron-long localization length measured in similar devices (refer to Ref. [4]), the formation of a quantum dot (QD) directly beneath the superconductor can be ruled out. Furthermore, our device tuning protocol includes the TGP protocol, with supplementary data provided in Sec. S4.3. We have not detected the formation of accidental quantum dots in the junction region adjacent to the wire, as such dots would typically induce particle-hole symmetry breaking in the dc transport data, which is absent in our observations.

Lastly, we discuss the compatibility of the observed results with local states at both ends of the wire both in the main text and in Sec. S2.7 of the manuscript, which we have extended in the revised version.

Page 4, line 277 "We interpret these h/2e-periodic. . . as originating from switches in fermion parity. Such switches have been observed in mesoscopic superconducting devices, where they were triggered by nonequilibrium QPs. . . " Indeed such parity switches are ubiquitous in superconducting devices. The obvious question is then: why the modeling that follows (and its rather restrictive assumptions like e.g. "the wire is in the topological phase and there are no subgap states other than the MZMs" is a valid explanation and not a more mundane one? A counterargument to my question above would be the various control experiments presented in page 6 but in my view any low-energy excitation in the wire, not necessarily of topological origin, could lead to the same result. (The model in Eq. B1 is extremely simplified!)

While parity switches due to localized states are common in mesoscopic devices, the defining feature of our study is the phase-coherent parity switches across several microns of the interferometer's circumference. For instance, the hypothesis involving a trivial local state at one end of the nanowire fails to explain our experimental data. Indeed, our low-field measurements, detailed in Fig. S14, invalidate standard interpretations of the observed phenomena. We have also evaluated and constrained the likelihood of local states at both ends of the wire as a plausible explanation, see discussion in Sec. S2.7. Thus, we turned to the Majorana zero mode (MZM) model as the most likely explanation. However, we have moderated our claims in response to the referee's suggestions.

Page 5, related to my previous comment about the simplified modelling. Other low-lying excitations/subgap levels in the wire could lead to a rather involved interference pattern affecting the quantum capacitance in Eq. 1. Can the authors comment on this?

As noted above, this is considered in Sec. S2.7.

Below, in line 304, the authors write "For detailed comparison with experiments, we simulate a more complete model of our interferometer, expanded to include the full triple-dot system, incoherent coupling to the environment, and backaction from the measurement. As before, we neglect all states in the wire except the MZMs." I don't understand the meaning of "more complete model" here: although they are including more (external) effects, the main assumption of a single fermion mode made of two Majoranas is still included by hand.

More complete means including "the full triple-dot system, incoherent coupling to the environment, and backaction from the measurement." Note that we went beyond the MZM model in Sec. S2.7 and summarized those results at the end of Sec. 3. Given the referee's comments, we have pulled more of this discussion from Sec. S2.7 to the end of Sec. 3 in the main text.

Page 8: line 516: experiments form the Katsaros group should be cited together with Ref [106]: Valentini et al, Science 373 (6550), 82-88, 2021; Valentini et al,Nature 612 (7940), 442-447 (2022)

In line 516, we cite Ref. 106 in connection with estimating E_M from Coulomb blockade peak spacing. The second of the Valentini et al papers (which deals with Coulomb Blockade spectroscopy) is now cited in this sentence while the first Valentini paper is now cited in the introduction where we mention trivial Andreev bound states.

Page 8: Discussion and outlook Again the same kind of sentences, line 545-547"We have presented dispersive gate sensing measurements of the quantum capacitance in topological qubit devices designed for the readout of fermion parity shared between MZMs at the opposite ends of a nanowire." Please be precise in what is proven and what is not.

We have shortened this sentence to "We have presented dispersive gate sensing measurements of the quantum capacitance in InAs-Al hybrid devices."

I agree that the experiments shows "flux-dependent bimodal random telegraph signal (RTS) in the quantum capacitance. We interpret this RTS as switches of the parity of a fermionic state in the wire. The long switching time τRTS ¿ 1 ms suggests a low quasiparticle poisoning rate, which we find to be within an order of magnitude of the quasiparticle density measured in a Cooper pair box device." (Lines 553-559). The next paragraph is interesting since the authors acknowledge that their experiments are not conclusive "These measurements do not, by themselves, determine whether the low-energy states detected by interferometry are topological. However, by fitting to a model of trivial Andreev states, we have tightly constrained the properties that such states would have to have in order to be consistent with our data. To fully resolve this issue, we will discuss the device's phase diagram and the stability of the observed flux-dependent RTS in a separate publication [54]." (Lines 568-574). How can we then know that the interpretation is correct, since this much-needed check is left for another publication with additional data to which we do not have access?

After reading the feedback of the referees, we appreciate how important the transport data used for tuneup is. Consequently, we have added TGP data to the section on device tuning and have changed this sentence to "Moreover, these devices have passed the TGP, as discussed in Sec. S4.3."

In summary, I have no great criticisms of the experiment and the data (which by themselves constitute a good piece of the state of the art of single-shot parity measurements in a hybrid device and their dependence on flux), but rather in the rather misleading way in which these data are presented and the extreme simplifications of the modeling which, essentially, asumes a topological state and includes Majoranas by hand, yet again forcing an a priori interpretation of the data.

3. REFEREE #2 (REMARKS TO THE AUTHOR)

In "Interferometric Single-Shot Parity Measurement in InAs-Al Hybrid Devices" the Microsoft Azure team presents interferometric measurements on a triple quantum-dot device shunted by a hybrid superconductor/semiconductor nanowire. The quantum capacitance of the large central dot is found to exhibit h/(2e) flux periodicity. The statistics of the quantum capacitance is strongly bimodal, suggestive of two distinct states with different quantum capacitance. Dynamics is measured of these states, with a typical timescales of milliseconds. The authors offer an interpretation that the two detected states are nearly degenerate fermionic parity states in the shunting wire, each individually

> *with h/e flux periodicity. Detuning the dot energies changes both the strength of the interferometric signal and the energy of the fermionic state in the wire. The authors suggest that their observations are consistent with single-shot parity readout of well-separated Majorana zero modes.*
>
> *This manuscript has the "wow" factor that one would expect for Nature. The data support the authors' claim of single-shot parity measurements of near zero-energy states. The parity-poisoning time is quite low, which is a rare bit of good news for the field. The exact relationship to Majorana is still not completely certain. However, this is still the highest bar reached so far in terms of tests for Majorana physics and, as the authors state, places strong constraints on alternative hypotheses. Most important for me is that the authors are working with a device geometry which, although extremely challenging, will plausibly allow tests of fusion rules in the near future.*
>
> *As stated above, the relationship to Majorana physics is not completely certain and needs some serious scrutiny. My two major points along these lines are: (1) More discussion is needed on the how often this procedure works, and under what conditions. I appreciate that the authors intend to present thorough comparison between Cq and TGP measurements elsewhere. On the other hand, the authors themselves present an additional dataset "A2", tuned with TGP, as a support of the reproducibility of their result. This isn't meaningful without at least giving some context. 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? Similar questions for Device B. The authors need to give us some context to make the control experiments meaningful.*

The referees' feedback makes it clear that a discussion of the TGP data is necessary to address the types of questions asked above. We have expanded Sec. S4.3 (on the TGP step of device tune-up) to address these questions. Measurements A1 and A2 (and the measurement of device B) were performed after using the TGP to tune up the device – specifically, to determine the parallel magnetic field and the voltage range in which the rf measurement is performed. Measurements A1 and A2 were performed more than a month apart, during which time there was some drift in the electrostatic potential in the device (presumably due to the slow movement of charges in the dielectric), as we also saw in Ref. 4 We only talk about hysteresis in the paper - should we stick with that here as well? Something like "...were performed more than a month apart, during which time there was some change in the electrostatic potential in the device (presumably due to hysteresis caused by numerous gate excursions).". In each measurement, this was the only region passing the TGP within the explored gate voltage and magnetic field range. (It may be the same region which simply drifted over the course of a month.) In all cases, there is at least some shift between the TGP regions and the bimodal C_Q regions due to cross-capacitance between the gates. We did not measure any voltage and field windows in this device or device B that passed the TGP but did not have bimodal C_Q and RTS.

> *(2) Based on the studies in Fig. 6/7, the zero energy state appears to be quite fragile. Is it plausible that Em should change by 6 μeV when dot plungers are changed? Does it indicate improvements are needed for fusion rules to succeed? Measured by the usual standards of ZBP-ology these changes are small. However, one could have hoped that a candidate Majorana identified in transport would be found to be much more stable now that a more sensitive detector is available. Without arguments to the contrary, I would take this instability as evidence against Majorana.*

Note that the evolution seen in Fig. 6 and 7 (which are now Fig. S11 and Fig. S12 in the revised version) is due to variation of the wire plunger, not the dot plunger. The wire plunger is changed by nearly 1 mV, which is roughly the size of the topological phase. This is not a small change since the lever arm of the wire plunger gate WP1 and the energy scale associated with the topological phase are, respectively, 40–60 meV/V and 20 µeV. Hence, a 1 mV change of V_{WP1} corresponds to a chemical potential change in the wire of 40–60 µeV, which is larger than the topological gap. Therefore, it is not surprising that E_M changed by 6 µeV. We have added a statement explaining this at the end of the first paragraph in Sec. S6: "Since the lever arm for the wireplunger gate is 40–60 meV/V (see Sec. S4.1), a change of 1 mV corresponds to a chemical potential change in the wire of 40–60 µeV, which is large on the scale of the physics considered here."

> *I have a few minor points:*
>
> *-The phrasing in the introduction "By itself, this measurement does not unequivocally..." is quite clear and fair. I find the following paragraph "In this work we introduce a topological qubit design that allows one to perform projective measurements of fermion parity encoded in MZMs" to be inaccurate and completely confusing. Stated at the level of "In this work" this could easily be read as a claim that a topological qubit has been demonstrated. The discussion also mixes realization-specific details (ie 9.1 nm thick InAs quantum well) with very general, aspirational statements of what this device could possibly do under unexplored circumstances. The proceeding discussion of what needs to be done "to form a qubit" is even more confusing.*

We have simplified and condensed this section, removing these statements. This paragraph now reads: "In this work, we introduce a device design that allows one to perform projective measurements of fermion parity, as required in Refs. 39-44."

It's understandable to point out that this device geometry could eventually make a qubit. However, the current level of detailed discussion is completely distracting. Given the complexity of the geometry, it's also incredibly difficult to follow. I request that the authors almost completely eliminate the discussion of how to tune up a qubit in this geometry, or justify why this I needed. I would also insist that the authors use care in language in whatever brief discussion they have on this point. For instance, saying "Here, we focus on the left topological section..." makes a stronger (and too strong) claim than the introduction and abstract.

As suggested by the referee, this discussion has been almost completely eliminated. It now reads: "To realize the ideas of Ref. 2, the second and fourth segments, each of length $L \approx 3\,\mu\text{m}$ long, would be tuned with the TGP while the other three would be fully depleted underneath the Al nanowire (see Fig. S1 in Sec. S1 for details). The complete gate layout, abbreviations for the different gates, and the voltage configurations for different operation modes are described in Sec. S1. Here, we focus on the second section, of length $L \approx 3\,\mu\text{m}$, shown in Fig. 1c, and implement a parity measurement using its associated interferometer. "

-The authors state "The material combination and dimensions have been optimized for values...". I request that they supply some more details on this statement. What are the techniques used for these optimizations? It should at least be clear if this is based on experiment or theory.

We added an extra statement explaining that this is "through simulations of the material stack in combination with Hall mobility, weak anti-localization, localization length measurements, and induced gap measurements, as explained in Ref. 4."

-The discussion of the tuneup procedure in the main text is difficult to follow. The statement "We tune dot 2 to charge degeneracy and use the TGP to select a magnetic field and a VWP1 for our measurements" makes it unclear what configuration the device is in during the TGP, and even if transport is being measured for it. It also needs to be stated if the TGP succeeds in multiple regions and how this one is chosen.

From the referee's questions, it is clear that we need to elaborate on these points. We have expanded the discussion in Sec. S4, which includes further explanation of these points.

-The statement "The observed behavior is consistent with the random matrix theory prediction for a disordered quantum dot" seems overly strong, unless some statistical analysis is presented. I believe the authors simply mean this at a qualitative level, but the statement doesn't reflect that.

We have removed this sentence.

-The phrasing of the sentence "We interpret these h/2e-periodic bimodal oscillations and RTS in Cq as originating from switches in fermion parity." is prone to misunderstanding. It naively implies that the entire phenomenon is due to parity switches, rather than the h/2e as opposed to h/e periodicity (which is what I think the authors mean, although I'm still not totally sure).

We agree that our original phrasing was confusing. We have fixed this sentence to avoid misunderstanding: "We interpret the RTS in Cq as originating from switches in fermion parity."

-In Appendix E I found the location of Ohmics S1, S2 unclear. Can the labels be improved, or arrows drawn?

We have clarified this in the main text of Sec. S1.1: "The 2DEG density in these reservoirs is set by the voltage on the helper gates "HGi" which run from the dot region all the way to metallic Ohmic source contacts (denoted by purple boxes labeled "Si" in Fig. S1a) outside the field of view of Fig. S1a but near the edge of the 2DEG mesa."

-The legend in Fig. 16i appears to have a mistake. The +/- reads 0.0 ms.

We have fixed the legend. It is +/- 0.1 ms.

4. REFEREE #3 (REMARKS TO THE AUTHOR)

The manuscript "Interferometric Single-Shot Parity Measurement in InAs-Al Hybrid Devices" by Nayak and coworkers reports the interferometric measurement on the parity of an InAs-Al island, a step toward realizing measurement-based topological quantum computation (TQC). The authors couple the island to three quantum dots in series as a reference arm to form a loop geometry. By tuning the relevant parameters (magnetic field and gate voltages) into the right regime, they create two zero-energy states at the island edges. The coupling between these states and the reference arm is modulated by the detuning of dot-1 and dot-3, while dot-2 is tuned to the degeneracy *point, whose quantum capacitance depends on the parity of the island and the magnetic flux threading the loop. This quantum capacitance is measured using an rf-reflectometry technique by coupling dot-2 with an LC oscillator. The authors then observe Aharonov-Bohm-type oscillations of the capacitance, which is associated with coherent electron interference through the reference arm and the two end states. The oscillation period is h/e for each parity branch, and the lifetime of each parity state is extracted to be the order of milliseconds. The authors claim that their observation is consistent with the hypothesis that the two end states in the island are Majorana zero modes (MZM). They further support this claim by theory simulations and some experimental control tests, such as those conducted at lower magnetic fields or with the reference arm decoupled from the island.*

This is a good summary of the results in our paper. The only key point that wasn't mentioned here is that the capacitance difference between the two parities is large ($\sim 1\,\mathrm{fF}$), enabling high SNR or, equivalently, a low probability of assignment error.

The field of MZM and TQC has been in intense debates since its birth. The definitive proof of MZM would be a braiding experiment, which depends critically on two factors: material advancements and the design of the outer braiding circuit (the technique). The manuscript did not report any material advancements that would lead to new or stronger MZM signatures. Similar physics has already been demonstrated in transport experiments, e.g. Albrecht et al, Nature 531, 206 (2016), Whiticar et al, Nature Comm. 11, 3212 (2020), and PRB 107, 245423 (2023) from the same group.

The Albrecht et al and Whiticar et al. papers are not from the same group as the present paper. The paper published in PRB 107, 245423 (2023) and the present paper are from a team consisting only of Microsoft employees and contractors, with no involvement by the Copenhagen University faculty/post-docs/students who wrote Nature 531, 206 (2016) and Nature Comm. 11, 3212 (2020). The Microsoft team has made material advancements since 2020. For instance, the quantum well thickness, top barrier thickness/composition, dielectric, and width of the Al strip are all different in our work from their values in the Whiticar et al. paper. As a result, the 2DEG mobility is significantly higher, the induced gap is in the optimal range, and the device can be tuned to the lowest sub-band. These advances were reported in Ref. 4 and they helped enable the work reported in the present paper. Meanwhile,the Albrecht et al. paper is on VLS wires, not 2DEGs. It is in the Coulomb blockade regime for the wire and doesn't have an interference loop.

Moreover, the parity lifetime of a subgap state in hybrid islands has also been indirectly extracted in transport experiments, see Higginbotham et al, Nature Physics 11, 1017 (2015).

The measurements in Nature Physics 11, 1017 (2015) were performed in the strongly-blockaded regime in which, according to the authors, thermally-excited quasiparticles were the dominant source of poisoning and the leads were a negligible source. In our case, the device is grounded (and the leads could be a quasiparticle sink) and poisoning is dominated by non-equilibrium quasiparticles. Thus the physics underlying the parity lifetime is different in the two cases: both the quasiparticle densities and the relaxation processes are different.

Therefore, the novelty of this manuscript does not lie in providing stronger evidence for MZMs, ...

As we discuss briefly in the main text and in more detail in Supplementary Material Sec. S2.7, our measurement puts tight constraints on trivial scenarios, such as the quasi-MZM model. In other words, we provide *different* evidence than in transport measurements.

... but in its methodological approach: it demonstrates that rf-parity readout "can be done" within this complicated loop geometry, which requires a lot of tune-up and parameter control. In other words, if the other three superconducting segments in Fig. 2(b) (on the right part) are operational, then a braiding experiment involving four zero-energy states could be straightforwardly executed, allowing at least a Z-operation to confirm non-Abelian statistics (and of course also their MZM nature). Given the significance of TQC and the technical challenges encountered, I consider this manuscript worthy of publication in Nature. However, before recommending acceptance, the authors must address the following concerns:

1) Are the other three superconducting segments operational or not? If not, why?

The superconductor is continuous, so there aren't really distinct superconducting segments. However, there are separate gates, and they are all operational. In this paper, we have grounded some of the gates, as discussed in Fig. S1. We clarified this with the following change to the sentence that introduces the segments: "The Al strip is continuous and there are no breaks between the sections, but each has a different "plunger" gate in the first gate layer"

2) Given that this experiment cannot firmly exclude a fine-tuned zero-energy state, the abstract's statement that "these results are consistent with ... Majorana..." without mentioning alternatives could be misleading. Especially considering that this may be the take-home message in press release and media coverage. The authors should revise their abstract to prevent potential misinterpretation. For example, I can imagine that the results may also be consistent with the scenario proposed by Hess et al, PRL 130, 207001 (2023).

In response to the referee's request, we have updated our abstract to explicitly acknowledge alternative interpretations. This addition underscores our serious consideration and analysis of other explanations, and how our measurements limit their plausibility. We wish to highlight that, while parity switches due to localized states are frequently observed in mesoscopic devices, the distinctive aspect of our study is the phase-coherent parity switches extending over several microns of the interferometer's circumference. Notably, the hypothesis of a trivial local state at one end of the nanowire is inconsistent with our experimental findings. Our low-field measurements, detailed in Fig. S14, invalidate standard interpretations of the observed phenomena. We have also scrutinized and restricted the probability of local states at both wire ends, which we believe the referee may associate with the Hess et al. scenario. Our discussion in Sec. S2.7 demonstrates that such a scenario would necessitate an improbable degree of fine-tuning at the neV level. While we cannot discount other scenarios, we emphasize that our data is consistent with the Majorana scenario. Additionally, our devices have successfully passed the TGP and were tuned as outlined in Sec. S4.3. We trust this addresses the referee's concerns.

3) I am confused by the geometry of the Al strip in the device. Is the Al strip continuous (without breaks) across all the five superconducting segments (spanning over 10 micron)? Or is it etched into disconnected islands (such as the 2.5-micron island)? From the device schematics and SEM, it seems to be the former. If so, how is there still a charging energy, given that this long strip is connected to D1 and D2?

The superconducting strip is continuous and grounded. The quantum dots have charging energy, but the superconductor does not. The dots' charging energy is sufficient to enable the measurement. As mentioned above, this has been emphasized by the following sentence: "The Al strip is continuous and there are no breaks between the sections, but each has a different "plunger" gate in the first gate layer"

4) During the device tune-up, between step-3 (topological gap protocol) and step-4 (tuning the TQDI loop), how can the authors ensure the device remained within the regime that passes TGP when they performed the dispersive sensing measurement? Given the variations in gate voltages between rf sensing and DC transport, the cross-talk is not negligible especially considering the tiny parameter space of TGP in their previous PRB work.

We have added an additional discussion of this point in the Supplementary Materials Sec. S4. In short, we have measured the cross-capacitance between the wire plunger and the gates that are varied between the transport and rf measurement configurations by tracking the motion of zero-bias peaks as a function of these gates. Only the gates QC1 and QC2 (the cutters between the long dot and the small dots) have a non-negligible effect. We minimized this effect by limiting the voltage excursions on these two gates.

5) The Al strip is only 60-nm wide; lithographic inhomogeneity should be a serious disorder source. It is hard for me to believe that the 2.5-micron long island could maintain a small topological gap across the whole island considering this lithographic disorder. Could the authors elaborate on this?

Similar devices have been measured to give $> 1\,\mu\text{m}$ localization length and pass the TGP. This means that there are significant correlations between the ends of the device, in both local and non-local transport [4]. The devices reported in this paper have all passed the TGP, as described in Sec. S4.3. Taken together, these measurements indicate that lithographic disorder does not limit device performance. (Note that the nanowire is not an island. It is grounded.)

6) In Fig. 3(a), why is there a shift in B for the kurtosis at V_{QD2} = 0.5 V and 0.7 V? If E_M is not zero, then why is shift absent at other V_{QD2} values in the same figure?

The tunneling matrix elements have signs that vary from one charge state of the dot to another. We have removed the sentence suggesting that they are governed by random matrix theory, but the statement that they vary from charge state to the next is still correct.

7) Have the authors observed the "bowtie" or "diamond" pattern (see Prada et al, PRB 96, 085418 (2017)), typical for dot-coupled Majoranas? If not, why?

We have not done dc transport measurements of the coupled dot-wire system in the regime in which the dot is Coulomb blockaded because our focus is on using rf measurements for the dot-wire coupling in these devices.

Corrections of typos are also necessary: Line 171: Fig. 2b should be Fig. 1c.

This typo has been fixed.

Fig. 22: panel b mislabeled as g

This typo has been fixed.

5. REFEREE #4 (REMARKS TO THE AUTHOR)

This work addresses the problem of measuring the fermion parity of Majorana zero modes (MZMs) in a 3-micrometer-long Al/InAs nanowire. This measurement capability is important since it can eventually enable measurement-based quantum gates as well as the readout of Majorana qubits. The reported parity-measurement technique consists in detecting variations in the quantum capacitance C_Q of a long InAs quantum dot connected to the edges of the Al/InAs nanowire via two smaller quantum dots providing gate-tunable links. The quantum capacitance is probed by rf gate reflectometry, which enables time-domain detection with microsecond time resolution. In a narrow range of experimental parameters (in-plane B-field, V_{PW1}), the Microsoft team finds that C_Q vs out-of-plane B-field is switching on millisecond time scale between two oscillating branches. The two branches exhibit counter-phase oscillations with a periodicity corresponding to h/e magnetic flux across the loop formed by the Al/InAs nanowire and the series of three InAs quantum dots. These Aharonov-Bohm oscillations are interpreted as a signature of single-electron interference via MZMs, with quasi-particle poisoning in the Al superconductor inducing stochastic switching in the occupation (i.e. the parity state) of the MZMs.

This is a good summary of the results in our paper. The only key point that wasn't mentioned here is that the capacitance difference between the two parities is large ($\sim 1\,\mathrm{fF}$), enabling high SNR or, equivalently, a low probability of assignment error.

On a mere scientific level, this is the main result of the paper. It confirms previously reported observations from a dc transport experiment in a similar type of system (Ref. 45). Here the novelty lies in the detection of stochastic parity switching, while in Ref. 45, pi phase shifts in Aharonov-Bohm oscillations were observed as a result of gate-induced changes in the occupation parity of the Al/InAs nanowire island.

Ref. 45 reports a dc transport measurement of the time-averaged parity, which oscillates rapidly at the charge degeneracy point due to the coupling to the leads. Away from charge degeneracy, the fermion parity is fixed and there is no ground state degeneracy. Thus, it is in a completely different regime, which is incompatible with the proposals of Refs. 2, 40–44. Thus our paper establishes a novel result that referee 4 quite fairly states "can eventually enable measurement-based quantum gates as well as the readout of Majorana qubits."

On a technical level, the present work introduces microsecond-scale, time-domain parity detection. This is achieved by means of dispersive rf gate reflectometry, a technique routinely used for the readout of semiconductor qubits. The reported SNR of 1 on a 3.7 microseconds integration time is in line with typical values for gate-based sensing in semiconductor quantum dots (see e.g. Schaal et al, Phys. Rev. Lett. 124, 067701 (2020)).

The authors claim their work is a significant step toward the realization of topological qubits. To my view, however, the reported achievements do not meet Nature's novelty and relevance standards. Moreover, the conclusions drawn in this work rest on questionable hypothesis and methodologies. As a result, I cannot recommend publication in Nature. In the following I will motivate this judgement.

The observation of parity switching is indeed new for the specific case of Al/InAs nanowire systems. It is ascribed to quasiparticle poisoning with a characteristic time scale of 1ms. If topological qubits were ever to be realized, this time scale would set un upper limit on their lifetime. This ms time scale is not that long if compared to the coherence times achieved in superconducting or semiconducting qubits, which are way simpler to operate and do not suffer from certain known limitations of Majorana qubits (e.g. the lack of protection against errors from non-Clifford gates). I would generally agree with the view that performing single-shot parity measurements on a microsecond time scale is an important technical achievement along the way to realizing quantum computing architectures based on topological qubits. Yet, one should note that this achievement is specific to the chosen physical platform, i.e. Al superconductor on an InAs/InGaAs semiconductor heterostructure, and that, contrary to the view of the Microsoft team, the realization of topological superconductivity in this platform is still a subject of debate in the community. Similar to many earlier works, here it is shown that the experimental results are consistent with a model involving MZMs localized at the edges of the InAs nanowire. This consistency, however, does not rule out other possible scenarios with no topological superconductivity.

We respectfully acknowledge the difference between our perspective and the referee's on several points. Firstly, a poisoning time of 10 ms and a measurement time of 1 µs would give an error rate due to poisoning of 10^{-4}. The poisoning time that we have demonstrated is within a factor of 5 of this target, and there is room for optimizing our devices with respect to the poisoning time. For example, a more scalable topological qubit layour, referred to as two-sided tetrons, are designed with a smaller cross-sectional area[2], which makes them more robust to IR radiation— one of the primary sources of non-equilibrium quasiparticle generation. Secondly, comparing our devices to superconducting qubits, which have undergone two decades of optimization, may not provide a fair assessment at this stage.

With regards to the progress demonstrated here, the design and readout protocols reported in this manuscript are quite versatile, suitable for a variety of material combinations beyond the specific Al–InAs platform used in this demonstration. For example, these techniques could easily be applied to superconductor-semiconductor platforms using superconductors that would give a larger topological gap.

We maintain that the introduction of a topological qubit would constitutes a significant breakthrough in the field of quantum computation. The referee's mentions 'the lack of protection against errors from non-Clifford gates.' However, there has been progress in distillation protocols for magic states that rely on high-fidelity Cliffor states. And, it is worth remembering that, in both semiconducting and superconducting qubits, no quantum gates are topologically protected against errors. Indeed, there are many other important differences between topological qubits and other solid-state qubits. For a more detailed comparison of qubits, we refer the referee to the preprint 'Assessing requirements to scale to practical quantum advantage' [arXiv:2211.07629]. We contend that quantum computation requires innovative ideas that pave the way to utility-scale applications.

The interferometry experiment is carried out in narrow range of parameters controlling the electronic properties of the Al/InAs nanowire, i.e. the gate voltage V_{WP1} and the in-plane magnetic field. Apart from some punctual checks outsides this parameter range, no clear view is offered of what is happening elsewhere. The storyline of the paper is centered on the physical picture of a topological state of the nanowire island, which, according to the authors, is establish following the topological gap protocol (TGP). The last sentence at the end of section 2 reads "Complete details of tuning the nanowire into the topological phase will be discussed in [54].". I find this simply unacceptable. Moreover, the TGP has been developed by the Microsoft team and it is not generally accepted by the scientific community.

We have removed this sentence and provided those details in Supplementary Materials Sec. S4.

Finally, I have some further more specific remarks and questions:

1) The interferometry data in Fig. 3b and Figs. 7a-c are shown on a relatively narrow $B_\perp$ range. It would instructive to see a broader field scan, including negative-field data.

The resonator response is $B_\perp$ dependent, so we restrict our $B_\perp$ sweep to a narrow range. Additionally, in the experiment the x-axis of the magnetic field which is swept to tune the flux through the loop is close, but not perfectly, aligned with the out-of-plane direction (misalignment < 1 degree). The range of this B_x sweep is chosen such that the perpendicular component $B_\perp$ is swept symmetrically around zero. We have replaced $B_\perp$ with B_x throughout the text and added a note in Sec. 3: "We sweep the x component of magnetic field B_x in steps of 0.14 mT to study the dependence on the external flux Φ through the interferometer loop and sweep V_{QD2} to find charge transitions in dot 2. B_x is offset from the true out-of-plane axis by < 1 degree, and the B_x sweep range is offset from 0 such that the perpendicular field component $B_\perp$ is swept symmetrically around 0.

2) In Fig. 7, a tiny variation (< 1 mV) in the gate voltage V_{WP1} has a very large impact on the measured interferometric signal. I suppose this corresponds to tuning around a given charge degeneracy point of the Al/InAs nanowire island. Is this behavior confirmed at other charge degeneracy points. On what V_{WP1} range can this be reproduced?

The InAs-Al nanowire doesn't have a charging energy. By varying V_{WP1}, we change the chemical potential in the wire by an amount proportional to the lever arm of WP1, which is 40–60 meV/V. A variation of 1 mV is not a tiny variation; it corresponds to a chemical potential change in the wire of 40–60 µeV, which is larger than the topological gap value returned by the TGP (or any estimate of the likely topological gap in this material system). Therefore, it is not surprising that it has a large impact on the signal. We have added statement explaining this at the end of the first paragraph in Sec. S6: "Since the lever arm for the wireplunger gate is 40–60 meV/V (see Sec. S4.1), a change of 1 mV corresponds to a chemical potential change in the wire of 40–60 µeV, which is large on the scale of the physics considered here."

3) In Section 5, the authors state: "It is worth noting that this method enables us to probe the Majorana splitting energy E_M with single-μeV resolution." Once again, this conclusion rests on the validity of the assumptions underlying the fitting model, which remains to be established.

We changed this sentence to “By fitting our data to the model in Sec. S2, we are able to determine the parameter E_{M} with single-µeV resolution. ”

> *4) The Cooper-pair-box control experiment confirming the quasiparticle poisoning rate was performed a zero magnetic field, as opposed large in-plane magnetic field ($\sim 2\,\mathrm{T}$) used in the measurements of parity switching in Al/InAs nanowire island. This difference can significantly impact the comparison.*

We thank the referee for their comment. Indeed, the magnetic field’s influence on the poisoning rate and non-equilibrium quasiparticle density warrants further study. While an increased magnetic field may increase the rate of quasiparticle generation, it will also suppress superconductivity at the device’s ground pads, potentially enhancing quasiparticle trapping efficiency. To avoid confusion, we have explicitly re-emphasized in the main text that the values from the RTS measurement are for a $\sim 2\,\mathrm{T}$ magnetic field.

In this context, We also note that we have updated our charging energy estimates, resulting in a revised quasiparticle density of approximately $2\,\mathrm{\mu m}^{-3}$

> *5) The authors use first and third quantum dots as tunable barriers. I would expect their role to be more complex than that and their behavior to depend not only on their effective charge occupancy, N_{g1} and N_{g3}, but also on their multi-particle electronic states, which can drastically change from one Coulomb valley to another. This point is not discussed.*

This is correct. We have added a sentence emphasizing this: “Note that the parameters $t_{ij}, t_{\mathrm{m}i}$ depend on the charge states of both the small and long dots which are selected to yield favorable values.”

Response to Second Round of Referee Reports

Response to Referee 1:

We appreciate Referee 1's detailed feedback and have carefully considered the points raised. We respectfully disagree with the assertion that we have mixed objective facts with interpretation. As noted by Referee 3, we have discussed trivial explanations extensively throughout the manuscript, including the abstract, introduction, main text, and conclusion. This ensures a balanced presentation of our findings. Moreover, where we have relied on a topological model to understand our device, we explained our assumptions. For instance, at the beginning of Section 2, we wrote "The first component is a nanowire which will have MZMs at its ends *if it is in a 1DTS state*" (emphasis added), which is an objectively true statement, not an interpretation.

Regarding the experimental data, we agree with Referee 1 that the data in Figure 3 represents a core experiment. However, we would like to highlight that additional experimental data is presented in Figures S6, S7, S9, S10, S11, S12, S13, S14, S16, S17, S18, and S19 in the supplement, which provide a rich context for our findings. The meaning of the TGP has been elaborated in section S4 of the supplement and has been extensively discussed in our earlier PRB paper. Referee 1 correctly points out that the data in Figure 3 is at one point in the phase diagram of the wire. However, interferometry data at other points within the expected topological regime is shown in Figure S12.

The referee asserts that "After this data is presented ... the rest of the paper focuses on interpretation using Eq. (1)...." In fact, we use a more complete model, as stated in Section 3: " For detailed comparison with experiments, we ... simulate a more complete model [than Eq. (1)]... that includes the full triple-dot system, incoherent coupling to the environment (using parameters inferred from separate measurements, cf. Secs. S9 and S10), and measurement backaction." We also interpret the data in terms of a non-topological model. Near the end of Section 3, we wrote "By extending the model introduced above, we have analyzed the quasi-MZM scenario ...," which we also discuss in detail in S2.7.

We understand the concern about the reproducibility of our results. We had the same concern ourselves, which we addressed by repeating this measurement twice on the same device and then on a second device. As noted by Referee 2, we ran an automated tune-up procedure on two devices and observed similar results, which demonstrates a level of reproducibility comparable to semiconductor spin qubits. While it could be instructive to explore greater parameter dependence, we chose to fix parameters based on predefined criteria and tune up from there. Referee 1 objects that "this does not rule out near zero modes of non-topological origin." This should not be a surprise since, in Section 4, we stated "These measurements do not, by themselves, determine whether the low-energy states detected by interferometry are topological. However, our data tightly constrains the allowable energy splittings in models of trivial Andreev states."

Response to Referee 4:

We appreciate Referee 4's comments and have made improvements to the manuscript based on the feedback, which the referee acknowledges "the authors have improved their manuscript correcting for some the weaknesses identified in the originally submitted version."

However, the Referee's assertion that our work rests on extremely fragile ground is belied by the absence of criticism of the validity of our main result, which is the successful implementation of interferometric, single-shot parity readout in a forward-compatible architecture. The referee's only criticism is that "reflectometry technics are a standard practice in solid-state qubits," which is true but does not contradict the novelty of successfully using these techniques for interferometric, single-shot parity readout.

The criticism of the TGP overlooks the fact that our previous results using the TGP were peer-reviewed and published in Phys. Rev. B 107, 245423 (2023), where extensive numerical and experimental evidence was presented. Referee 4 implies that the model considered in Reference 73 (Hess et al.) applies to the realistic nanowire devices reported here and claims that the trivial Andreev states scenario considered in Reference 73 can still pass the Microsoft TGP.

We disagree with both statements:

Firstly, the model by Hess et al. does not consider the realistic disorder level in our wires. As reported in Phys. Rev. B 107, 245423 (2023), the localization length in the relevant density regime is below the wire length, whereas the model of Hess et al. considers a scenario where the localization length is much longer than the wire length. Therefore, it is not obvious that this model is relevant for understanding the devices measured in this manuscript. Moreover, it is unlikely that a trivial gap closing and reopening, induced by a periodic array of Andreev states, is generically present in real, physical wires with disorder, as pointed out in Phys. Rev. Lett. 132, 099601 (2024).

Secondly, their model fails the TGP because it has trivial gap closings but not the concurrent presence of stable zero-bias peaks.

Finally, as noted by Referee 3, the TGP is not the main focus of the present manuscript. The TGP is used as a tuning procedure, so this criticism isn't particularly relevant to our current work.

As noted by Referee 3, the criticism based on Kouwenhoven's perspective article neglected to mention that our approach was listed as one of the four promising directions in that article. The retracted papers cited by Referee 4 were not authored by our team. We would also like to re-emphasize that we have introduced automated tuning methods and standardized data analysis protocols to avoid the pitfalls of earlier work.

Response to Referee 2:

Referee 2 asked the following question about Section S4.4: "The text indicates that QC1/QC2 have a large cross capacitance to the wire, whereas TG1/TG2 do not. ... If this cross-coupling is unexpected, I ask that they say so." The measured cross-coupling is consistent with our electrostatic simulations and is not unexpected since the fine leads to these gates are relatively close to the bulk of the wire. We added the following sentence to S4.4.: "However, we find a significant cross capacitance for the gates QC1 and QC2, consistent with electrostatic simulations (where it can be understood as resulting from the routing of these gate leads close to the wire)."

In summary, we believe that our manuscript presents novel findings that contribute significantly to the field. We have addressed all scientific questions raised by the referees and believe that our work is ready for publication in Nature.