In the manuscript, the authors present a proposal for a resonator-type Josephson parametric amplifier based on a hybrid superconductor–semiconductor nanowire. This proposal builds upon their previous work published in *SciPost Phys. 18, 013 (2025)*, where the current-phase relation (CPR) of a hybrid Josephson junction lacking inversion symmetry was demonstrated. The design relies on the nonsinusoidal CPR of hybrid nanowire Josephson junctions (JJs). Although Sn/InSb or Sn/InAs junctions are likely not in the short-junction limit, their CPRs are nevertheless expected to contain higher-order harmonics. This is a reasonable assumption, and the ratio between the amplitudes of the first and second harmonics, $I_1/I_2 \approx 4$, is plausible. However, the most limiting factor remains the barrier between the superconductor and semiconductor, as discussed in *Phys. Rev. B 71, 052506 (2005)*.
As correctly stated in the manuscript, the mere presence of higher-order CPR terms is not sufficient to realize a single junction that dominates three-wave mixing. However, some statements such as “fourth-order nonlinear term of the inductive potential” and “A common device with third-order nonlinearity is the Superconducting Nonlinear Asymmetric Inductive eLement (SNAIL)” may be confusing to readers.
I suggest defining the nonlinear Josephson inductance L_J early in the manuscript and expressing $L_J(\phi)$ for the CPR given in Eq. (1). It may even be possible to derive a condition under which the even nonlinearity of L_J vanishes—a “sweet spot.” Additionally, the Josephson potential and Hamiltonian could be introduced, but in that context, I recommend using the terminology “higher-order anharmonic terms of the potential.”
The weakest point of the proposal is the rather complex magnetic field tuning required to bring the JJ to the sweet spot. It is unclear why electric field tuning has not been explored. Since the sweet spot should also depend on the ratio I_2/I_1, which could be gate-voltage dependent, one could potentially use micromagnets and tune the JJ via gate voltage instead.
While the idea is promising, several aspects of the manuscript could benefit from clarification and refinement.

In the introduction section, the formulation "By way of assessment of device characteristics from key metrics such as power gain, 1 dB compression power and operable frequency bandwidth, Josephson junctions based on Sn as superconductor offer the critical current levels (100s of nA) that are compatible with SNAIL operation." is misleading, as it suggests that Sn-based Josephson junctions have already been benchmarked in terms of amplifier metrics (gain, compression power, bandwidth), comparable to SNAIL performance. In reality, the only parameter assessed here is the critical current, which indeed falls in the suitable range for SNAIL operation, but no amplifier-level comparison was carried out.

In the caption of Fig. 1, fitting parameters η_{n1} and η_{n2} are introduced for harmonic amplitudes I_n, extracted from numerical simulations. These parameters are not consistently used in the main text, and their values are not listed. Moreover, in Eq. (2), η_{11} and η_{21} are replaced by α and β, while η_{12} and η_{22} appear redundant. Since the magnetic field dependence of the critical current amplitudes is already normalized to the critical field B_c, the critical current should be treated as a fitting parameter (possibly with distinct values B_{nc}), and η_{n2} should be omitted.

The manuscript states that, to estimate parameters leading to optimal nonlinearity (Kerr-free operation with strong third-order nonlinearity), an optimization was performed over two variables, *a* and *c*. However, as correctly noted in the text, parameter *a* does not affect the shape of the potential well around the minimum. This is also clearly illustrated in Figs. 6(a) and 6(b). From Eq. (3), it follows directly that *a* corresponds only to a global phase shift and therefore has no effect on the local nonlinearity. Given this, it is puzzling that the optimization yields a precise value for *a*, as reported in the caption of Fig. 2.

On the other hand, parameter *c* (proportionality between mutual harmonic phase shift δ_{12} and the magentic field) plays a crucial role in determining the nonlinearity. In the manuscript, however, its origin and behavior are only briefly mentioned. In Ref. 46, it is stated, that δ_{12} arises as a consequence of spin–orbit interaction in the numerical simulations. The manuscript would benefit from an expanded discussion of the role and origin of δ_{12}, including how its presence can be ensured and how much experimental control is available. Since the optimization of the nonlinearity relies critically on this parameter, clarifying its microscopic origin and practical tunability would be useful.

Figure 12 in Appendix D is mistakenly described: "In Fig. 12(b), we show gain vs. signal strength $|\alpha_s|$ with maximum gain at $\omega_s=\omega_p/2$, and a gain of 20 dB achieved over a bandwidth of 40 MHz." In fact, the figure shows gain versus signal frequency. Nevertheless, a figure displaying gain versus signal strength would be of greater interest, as the use of the sweet spot is motivated by improvements in dynamic range. I would expect the 1 dB compression point to be estimated in order to demonstrate the viability of the optimization.

The manuscript presents an interesting proposal, but it requires **revisions** before it can be considered for publication. The authors should address the points above to improve clarity, consistency, and scientific rigor.