

Fully Tunable Strong Spin-Orbit Interactions in Light Hole Germanium Quantum Channels

Corresponding Author: Dr Patrick Del Vecchio

This manuscript has been transferred from another nature journal. This document only contains information relating to versions considered at Communications Physics.

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

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

The paper "Fully Tunable Strong Spin-Orbit Interactions in Light Hole Germanium Quantum Channels" by Del Vecchio et al. proposes and theoretically analyzes a novel Ge heterostructure that allows for switchable spin-orbit interactions via gate tuning. If successful, this would represent a significant advance in the field of semiconductor spin qubits, addressing a key challenge in achieving both fast qubit manipulation and long coherence times. The authors present a solid physical model and detailed calculations supporting their claims, which form the strongest part of the manuscript. However, the dense technical content, compromising space for in-depth discussion of experimental feasibility, limits the possible impact of the work. To support my recommendation for major revisions, I highlight the following key concerns:

* The paper is difficult to follow for potential readers; for instance, the origin of Hamiltonian (1) is not clearly given. The authors should provide the necessary references or derive it in the Supplementary Information (SI) to ensure reproducibility.

* The main outcomes of the paper are the parameters leading to the g - and β -lines presented in Figs. 2 and 3. While the motivation for on/off switching is given in the introduction, the implication of the g -line for leakage prevention is mentioned only briefly in the conclusion. The authors should expand on the practical implications of these findings (leakage mechanisms in particular) for qubit operation and qubit coherence.

* In particular, while the robustness of the β -line is tested against θ , I_x , and I_y , sensitivity to electric field fluctuations is not discussed. In fact, the results presented in Fig. 3 suggest that the β -line is very steep in the F_z direction, which would make it highly sensitive to charge noise. The authors should comment on this point. Note that this implies an "off-state" with zero spin-orbit interaction is not necessarily a low-decoherence state if the noise sensitivity is high.

* There appears to be an inconsistency between Eq. (3) and the SI regarding the sign of $\gamma_{\parallel}$ (The authors should check Eq. 3 vs Eq. D11, for instance).

* The abstract emphasizes: "The large in-plane g -factor also facilitates the integration of superconductors." Yet, after reading the manuscript, it is still not clear to me how this integration would be facilitated. In fact, a similar statement is repeated without reference or explanation at the end of the results section. The authors should clarify the physical mechanism behind this statement.

* Finally, the authors assume a particular content of Sn in the GeSn buffer layer (16%) to achieve the light-hole ground state. The authors should put this number in the context of current experimental capabilities and comment on the feasibility of growing such structures with low defect densities.

* Suggestion: To improve the readability and impact of the manuscript, the authors could consider moving some of the technical details to the SI and expand on the physical implications of their findings in the main text.

Overall, the manuscript presents a novel pathway to achieve tunable spin-orbit interactions in Ge qubits. However, due to

the concerns outlined above, I am unable to recommend publication in Communications Physics in the current form. I look forward to reviewing a revised manuscript that addresses these critical issues and incorporates the suggestions to improve clarity and impact.

Reviewer #2

(Remarks to the Author)

Review Report

Patrick et al. present a theoretical study of tunable strong spin–orbit interaction (SOI) in light-hole (LH) Ge qubits using model Hamiltonian analysis. They propose a Ge heterostructure consisting of a GeSn buffer layer and a top barrier layer to introduce tensile biaxial strains and the resulting LH as the ground state, which exhibits k-linear SOI tunability that can be suppressed to zero in 2D planar, 1D channel, and 0D quantum dot geometries. They further report the tunable out-of-plane g-factor for 1D and 0D devices. Additionally, the Rabi frequency for the LH Ge qubit is estimated. The study is systematic and is related to a hot topic. However, partially claimed novel results of this manuscript have already been discussed in previous works by the same authors, such as strong SOI in LHs and the use of LH to carry a qubit (Refs. 56 and 57). On the other hand, the introduction of Sn element into Si-based spin qubit is incompatible with current CMOS technology, sacrificing the most important advantage over other qubit platforms. In addition, the validity of the assumption in the model needs to be further verified. Specially,

1. The claimed novelty of this work relies on the tunability of the SOI, which in turn depends critically on assuming an asymmetric confinement profile. This assumption is weakly justified. The proposed device is highly likely to introduce non-uniform strain instead of a uniform one. On the other hand, existing experimental and theoretical studies on Ge quantum wells are, to my knowledge, based on symmetric confinement profiles, and no clear experimental evidence for the proposed asymmetric confinement is provided. More importantly, the specific proposals suggested to achieve asymmetric confinement are not experimentally realistic and lack sufficient elaboration. A very thin Si capping layer would lead to significant gate leakage due to tunneling. Direct contact between an oxide layer and Ge is generally unfavorable in Ge-based heterostructures because the oxide of Ge is unstable and the Ge/oxide interface is known to host orders-of-magnitude higher defects than Si/SiO₂. In addition, placing the qubit closer to the gate would substantially enhance charge noise arising from gate–oxide interface defects, which is widely regarded as the dominant noise source in Ge qubits.

2. The authors use a Sn content of 10% in plotting the energy confinement profile in Fig.1(b), but substitute it with 16% in the following content. Why not use a consistent value and write it clearly throughout the manuscript rather than using x in Ge_{1-x}Sn_x?

3. In Fig. 3(b, c), the SOI-off points show large $\partial\Omega/\partial F_z$, indicating strong sensitivity to charge noise. Given the magnified charge noise in the proposed device, idling the qubit at $\Omega = 0$ under these conditions may negate the intended benefit of turning off the SOI. A quantitative or qualitative discussion of charge noise and its impact on qubit performance at the SOI-off point is required.

4. The magnetic field is specified as $B_0 = 0.05$ T. In Fig. 3, the field appears normalized, indicating that the results are independent of magnetic field magnitude within a reasonable range. The reason for choosing this specific value of magnetic field should be clarified.

5. How is the specific value of Rabi frequency for a realistic qubit device? How does it compare with the ultrafast manipulation speed of 1.2 GHz observed in Ge NW?

6. The used sizes of quantum dots, such as l_x, l_y , are smaller than a planar Ge hole qubit (~100 nm), which is unfavorable for device fabrication. The authors are suggested to choose sizes within experimental availability.

7. On page 4, “QC effective parameters $g_c \perp, \parallel$ ” should be “ $g_c \perp$ ”.

The authors are suggested to thoroughly address the above points before the paper can be recommended for publication.

Reviewer #3

(Remarks to the Author)

In “Fully Tunable Strong Spin-Orbit Interactions in Light-Hole Germanium Quantum Channels”, Dr. Del Vecchio and coauthors present an asymmetric Ge/GeSn quantum well in which light-hole spin qubits can be defined. The vertical asymmetry of the well can be partially compensated by an applied vertical electric field, leading to operating (“idle”) points where the spin–orbit coupling is effectively turned off. This could be advantageous as an idling regime with reduced sensitivity to noise. Away from this point, the system exhibits strong tunability and large manipulability enabled by the strong SOI of light-hole states. The work builds on previous contributions by some of the authors on Ge/GeSn heterostructures and takes a further step by considering elongated quantum dots.

Overall, I find the manuscript interesting and potentially suitable for publication. At the same time, several elements appear closely related to the authors’ prior work on Ge/GeSn heterostructures, and the degree of conceptual advance relative to those papers is not fully clear from the current presentation. Below I provide comments that, if addressed, could strengthen the manuscript:

- 1) If I understand correctly, the main mechanism behind the idling point is the restoration of inversion symmetry via a vertical electric field. This seems rather generic, in the sense that any sufficiently asymmetric device could exhibit similar behavior. I therefore wonder about the robustness of this effect. In practice, additional mechanisms can asymmetrically affect linear and cubic spin-orbit terms (alloy disorder, strain, reduced symmetry at interfaces, etc.). Is the idling point robust against such mechanisms? I think this should be discussed.
- 2) The light-hole nature of the ground state appears to depend strongly on the Sn content. The manuscript seems to treat the GeSn alloy within a virtual-crystal approximation, whereas alloy disorder is unavoidable. Beyond corrections to effective masses, linear/cubic spin-orbit couplings, and g-factors, could alloy disorder also affect (or even prevent) the light-hole character of the ground state? If so, this should be mentioned.
- 3) The results are obtained using 6-band kp theory. However, if one includes the split-off band in Ge, should the conduction band also be included? The conduction band is farther away in energy than the split-off band, but they are of the same order of magnitude.
- 4) More generally, I find tunability through a vertical electric field experimentally delicate. Wouldn't one risk switching to a different occupation state while tuning the field?
- 5) I think the novelties of the paper, such as the evaluation of elongated dots in Ge/GeSn heterostructures should be emphasized more clearly throughout the manuscript to better distinguish it from prior works.
- 6) When $\gamma_{\parallel}$ is introduced after Eq. (1), it is described as the effective mass. I believe it is instead the inverse of the (dimensionless) in-plane effective mass, consistent with what is written for the vertical one near the end of page 2.
- 7) On page 2, the manuscript states: "The Schrieffer-Wolff transformation employed to derive Eq. (1) provides exact formulas...". What is meant by "exact" here? Don't these parameters receive higher-order corrections?

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

I thank the authors for their detailed responses and revision of the manuscript. I am satisfied with the explanations provided, and I think the revised manuscript has improved, in particular with the clarification of the role of longitudinal noise sensitivity at the zero-SOI operation point and the discussion of the feasibility of growing GeSn heterostructures. By reading the revised manuscript and the discussion with all the referees I am also more convinced of the novelty and potential impact of the work, which is now better distinguished from the authors' previous contributions. I therefore recommend publication in Communications Physics.

Reviewer #2

(Remarks to the Author)

The authors have answered my questions properly. I agree its publication in its current form.

Reviewer #3

(Remarks to the Author)

I thank the authors for their responses to the previous comments and for the revisions made to the manuscript. The authors have addressed several of the points raised in the first round. However, two relevant points regarding the robustness of the light-hole (LH) ground state and the influence of inhomogeneous strain remain insufficiently addressed.

1) In the first round, I raised the issue of whether alloy disorder could affect the light-hole character of the ground state. The authors respond by stating that they do not expect a qualitative change in the energy spectrum since most of the wavefunction resides in pure Ge. However, my concern was more specific: can alloy disorder in the $\text{Ge}_{1-x}\text{Sn}_x$ buffer layer introduce significant local fluctuations in the biaxial strain? Such fluctuations could, in turn, lead to substantial changes in the HH-LH splitting. Given that the LH ground state relies on a delicate balance between tensile strain and confinement, I believe the authors should clarify whether these strain fluctuations are expected to be small enough or if they could potentially alter the LH character of the state in certain regions of the device. A more quantitative response would be appreciated as the current response is purely qualitative.

2) Regarding the effects of non-uniform strain raised by Referee 2, the authors argue that corrections to the Rashba interaction are small, citing Phys. Rev. Lett. 131, 097002 (2023). However, the system analyzed in that PRL paper assumes a Ge quantum well where the HH-LH energy separation is relatively large. In the device proposed in this manuscript, the LH and HH states can be much closer in energy. Since the spin-orbit length (and thus the Rashba correction) is inversely proportional to the HH-LH energy difference, it is likely that the corrections from inhomogeneous strain would be significantly enhanced in the authors' proposed structure compared to the one in the cited PRL. Can we really discard these corrections in a regime where the HH-LH gap is small? I am not asking for a detailed simulation including such effects but discussing the impact of this type of correction is relevant.

In summary, while the manuscript has improved, these two points are central to the physical validity of the robustness of the LH qubit.

Version 2:

Reviewer comments:

Reviewer #3

(Remarks to the Author)

I thank the authors for their detailed replies to my questions. I recommend the manuscript for publication.

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Responses to Reviewers' Comments

We thank the reviewers for their valuable and encouraging reports. In the following, we address their comments and provide additional insights into the key contributions reported in the manuscript.

I. REPORT OF THE 1ST REFEREE

The paper "Fully Tunable Strong Spin-Orbit Interactions in Light Hole Germanium Quantum Channels" by Del Vecchio et al. proposes and theoretically analyzes a novel Ge heterostructure that allows for switchable spin-orbit interactions via gate tuning. If successful, this would represent a significant advance in the field of semiconductor spin qubits, addressing a key challenge in achieving both fast qubit manipulation and long coherence times. The authors present a solid physical model and detailed calculations supporting their claims, which form the strongest part of the manuscript. However, the dense technical content, compromising space for in-depth discussion of experimental feasibility, limits the possible impact of the work. To support my recommendation for major revisions, I highlight the following key concerns:

1. *The paper is difficult to follow for potential readers; for instance, the origin of Hamiltonian (1) is not clearly given. The authors should provide the necessary references or derive it in the Supplementary Information (SI) to ensure reproducibility.*

We would first like to thank the referee for the constructive comments. The effective Hamiltonian (1) is derived from the full 6-band $k \cdot p$ Hamiltonian presented in the supplementary information. For the sake of clarity, we reorganized a large portion of the supplemental material and added more information in the "perturbation theory" section.

2. *The main outcomes of the paper are the parameters leading to the g - and β -lines presented in Figs. 2 and 3. While the motivation for on/off switching is given in the introduction, the implication of the g -line for leakage prevention is mentioned only briefly in the conclusion. The authors should expand on the practical implications of these findings (leakage mechanisms in particular) for qubit operation and qubit coherence.*

We thank the referee for bringing up this point. The g -lines effectively correspond to a gapless qubit, similarly to the gapless spin qubit studied in the recent work Phys. Rev. Lett. 135, 197001 (2025). Such qubits present many advantages for integration notably the lack of a global clock and full electric baseband control, and how they eliminate leakage channels through the large energy gap to the next excited states. Since these are discussed at length in that reference, we believe we do not need to expand much further on this in our work. We nevertheless added a comment just before the conclusion to elaborate a bit more on this point.

3. *In particular, while the robustness of the β -line is tested against θ , l_x , and l_y , sensitivity to electric field fluctuations is not discussed. In fact, the results presented in Fig. 3 suggest that the β -line is very steep in the F_z direction, which would make it highly sensitive to charge noise. The authors should comment on this point. Note that this implies an "off-state" with zero spin-orbit interaction is not necessarily a low-decoherence state if the noise sensitivity is high.*

We agree that the Rabi frequency graphs indicate a steep derivative with respect to the gate field. However we emphasize that it is not necessarily an indication of a higher sensitivity to charge noise. The dephasing rate of the qubit is mostly sensitive to longitudinal noise in the qubit subspace, or components of the noise that change the Larmor frequency. Regarding this, Fig. 2c and 2e suggest that close to the zero-SOI operation point ($F_z \approx 4.4 \text{ V}/\mu\text{m}$), the in-plane and out-of-plane components of the g -factor have very small slopes with respect to F_z and thus we do not expect a significant increase in charge noise sensitivity. In fact, the longitudinal component of the z operator, $|\langle 1|z|1\rangle - \langle 0|z|0\rangle|/2$, shows a minimum with respect to F_z near the zero-SOI operation point (see Fig. 1 below. We added a sentence in the main text just before the conclusion to address this point.)

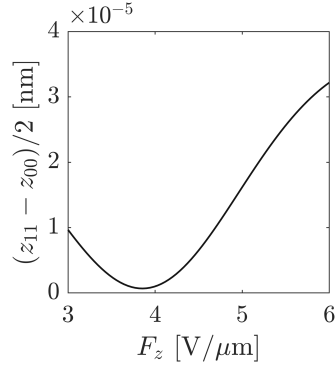


FIG. 1: Longitudinal component of the z operator. $l_x = l_y = 40$ nm, $\mathbf{B} = (0.05 \text{ T})\mathbf{e}_y$.

4. *There appears to be an inconsistency between Eq. (3) and the SI regarding the sign of $\gamma_{\parallel}^p$ (The authors should check Eq. 3 vs Eq. D11, for instance).*

We agree there is a sign inconsistency between equation D11 and equation 3. This is because we decided to use in the main text the usual convention of positive hole effective mass for convenience. On the other hand, in the supplemental material, the theory describes the hole effective mass within the electron picture and is therefore negative. $-\gamma_{\parallel} > 0$ and the square root in equation D11 is real. We added a comment at the beginning of the supplemental material to clarify this point.

5. *The abstract emphasizes: "The large in-plane g -factor also facilitates the integration of superconductors." Yet, after reading the manuscript, it is still not clear to me how this integration would be facilitated. In fact, a similar statement is repeated without reference or explanation at the end of the results section. The authors should clarify the physical mechanism behind this statement.*

We thank the referee for pointing this out. A large in-plane g -factor facilitates the integration of superconductors since they generally are substantially more resilient against in-plane magnetic fields compared to out-of-plane magnetic fields. For instance, Fig. 3 and Fig. 4a in Phys. Rev. Appl. 5, 044004 (2016) provide an indication that the quality factor of nanowire resonators in out-of-plane magnetic fields decreases much faster with B than in in-plane fields. In the updated manuscript, we included this reference to the main text.

6. *Finally, the authors assume a particular content of Sn in the GeSn buffer layer (16%) to achieve the light-hole ground state. The authors should put this number in the context of current experimental capabilities and comment on the feasibility of growing such structures with low defect densities.*

We thank the reviewer for bringing this issue, which is important from a practical standpoint. The growth of GeSn can be challenging given the metastable nature of this alloy and its large lattice parameter relative to that of silicon. However, by using nonequilibrium epitaxy growth processes it is possible to grow high-quality GeSn layers enabling their use in a range of photonic and optoelectronic applications. For an overview, see for Appl. Phys. Lett. 118, 110502 (2021). As described in that paper, device-quality GeSn heterostructures can be grown on silicon wafers by chemical vapor deposition using GeH_4 and SnCl_4 as precursors. Germanium interlayer is typically grown first on silicon to manage the large lattice mismatch. Following this process, our group grows routinely GeSn materials. We have demonstrated in the past the growth of GeSn at a concentration of 18% (see Appl. Phys. Lett. 112, 251903 (2018)). Thus, the proposed heterostructure is achievable using current epitaxy processes. We added a comment regarding this in the main text at the beginning of the Section "Light hole ground state requirements".

7. *Suggestion: To improve the readability and impact of the manuscript, the authors could consider moving some of the technical details to the SI and expand on the physical implications of their findings in the main text.*

We thank the referee for the suggestion. We added a few lines after equation 1 in the main text on the physical origins of the Rashba parameters appearing in the LH effective Hamiltonian, which may help differentiate LH ground states from conventional HH ground states.

Overall, the manuscript presents a novel pathway to achieve tunable spin-orbit interactions in Ge qubits. However, due to the concerns outlined above, I am unable to recommend publication in Communications Physics in the current form. I look forward to reviewing a revised manuscript that addresses these critical issues and incorporates the suggestions to improve clarity and impact.

We thank the Referee for their constructive feedback. We believe we have addressed all their comments and the updated manuscript is now improved and ready for publication.

II. REPORT OF THE 2ND REFEREE

Patrick et al. present a theoretical study of tunable strong spin-orbit interaction (SOI) in light-hole (LH) Ge qubits using model Hamiltonian analysis. They propose a Ge heterostructure consisting of a GeSn buffer layer and a top barrier layer to introduce tensile biaxial strains and the resulting LH as the ground state, which exhibits k-linear SOI tenability that can be suppressed to zero in 2D planar, 1D channel, and 0D quantum dot geometries. They further report the tunable out-of-plane g-factor for 1D and 0D devices. Additionally, the Rabi frequency for the LH Ge qubit is estimated. The study is systematic and is related to a hot topic. However, partially claimed novel results of this manuscript have already been discussed in previous works by the same authors, such as strong SOI in LHs and the use of LH to carry a qubit (Refs. 56 and 57).

We thank the Referee for their assessment and for finding our work systematic and potentially impactful as related to an hot topic. In the updated version of the manuscript near the end of the Introduction, we better emphasize the novelty of this work compared to previous literature by noting the focus on squeezed dots within heterostructures with built-in vertical asymmetry.

1. *On the other hand, the introduction of Sn element into Si-based spin qubit is incompatible with current CMOS technology, sacrificing the most important advantage over other qubit platforms. In addition, the validity of the assumption in the model needs to be further verified.*

We respectfully but strongly disagree with this statement. The proposed GeSn-based heterostructure is compatible with silicon and silicon processing. Indeed, GeSn is a group IV isovalent alloy regarded as CMOS-compatible because it can be grown on silicon substrates using processes (e.g., CVD or MBE) that are compatible with standard silicon fabrication workflows and thermal budgets. Its integration leverages the same material platform as silicon and germanium. Importantly, qubits based on GeSn can benefit from nuclear spin-free isotopes and rely on the same underlying material system and fabrication approaches as other silicon-based qubits. This ensures that advanced quantum functionalities can be developed without breaking compatibility with scalable, industry-standard semiconductor manufacturing. For these reasons, the current manuscript focuses on this system.

2. *The claimed novelty of this work relies on the tunability of the SOI, which in turn depends critically on assuming an asymmetric confinement profile. This assumption is weakly justified. The proposed device is highly likely to introduce non-uniform strain instead of a uniform one. On the other hand, existing experimental and theoretical studies on Ge quantum wells are, to my knowledge, based on symmetric confinement profiles, and no clear experimental evidence for the proposed asymmetric confinement is provided. More importantly, the specific proposals suggested to achieve asymmetric confinement are not experimentally realistic and lack sufficient elaboration. A very thin Si capping layer would lead to significant gate leakage due to tunneling. Direct contact between an oxide layer and Ge is generally unfavorable in Ge-based heterostructures because the oxide of Ge is unstable and the Ge/oxide interface is known to host orders-of-magnitude higher defects than Si/SiO₂. In addition, placing the qubit closer to the gate would substantially enhance charge noise arising from gate-oxide interface defects, which is widely regarded as the dominant noise source in Ge qubits.*

We thank the referee for bringing up this detail and agree that Ge oxide is poor. However, high-quality oxides can be deposited by ALD, as shown in recent work: T. Aso et al. Materials Science in Semiconductor Processing 190, 109372 (2025). We stress that the basic requirement is a top barrier such that its band offset ΔE is large enough to prevent the light hole from leaking to the surface at strong vertical gate field, i.e. $\Delta E \gtrsim eF_z d$, where d is the thickness of the barrier. We put an emphasis on *thin* upper barriers to alleviate the requirement on ΔE . On hindsight, we agree that "a few ångströms" thick is far too restrictive, and could increase charge noise. Assuming the device operates at gate fields lower than $\sim 10 \text{ V}/\mu\text{m}$, a $d \lesssim 10 \text{ nm}$ upper barrier that prevents leakage of the hole to the surface would require a band offset $\gtrsim 100 \text{ meV}$ only, which is still satisfied using Si, or even a Si-rich SiGe alloy. We modified the main text to put more emphasis on the *no leaking* requirement, rather than on *very thin* barriers. We also added the above article in the list of references. Effects of non-uniform strain may also be present at the quantum well layer, but will fall-off with distance for thicker barriers. Nevertheless, its effects on the LH will likely be similar to HHs by rescaling the strength of the linear Rashba term. According to Phys. Rev. Lett. 131, 097002 (2023) (see Fig. S7 and Eq. S17 therein), shear strains introduce spin-orbit lengths $\ell_{\text{so}}^{-1} \sim 0.32 \mu\text{m}^{-1}$ in the Ge QW underneath a 50 nm top barrier. This translates to $\beta_1 \sim \hbar^2/(m^* \ell_{\text{so}}) \sim 0.01 \text{ meV nm}$, which is much smaller than the $\beta_1 \sim 1 \text{ meV nm}$ we report for LHs. Therefore, we believe the presence of shear strains would not have qualitative effects on our results, even considering thinner top barriers.

3. *The authors use a Sn content of 10% in plotting the energy confinement profile in Fig.1(b), but substitute it with 16% in the following content. Why not use a consistent value and write it clearly throughout the manuscript*

rather than using x in $\text{Ge}_{1-x}\text{Sn}_x$?

We decided to plot the envelope functions at 10% Sn content in Fig. 1 only for better visuals, i.e. one could easily see the three types of confined levels (LHs, HHs at the Ge/GeSn interface and those at the surface barrier) with the best proportions. We added a sentence in the caption of Fig. 1 in the main text to clarify this.

4. In Fig. 3(b, c), the SOI-off points show large $\partial\Omega/\partial F_z$, indicating strong sensitivity to charge noise. Given the magnified charge noise in the proposed device, idling the qubit at $\Omega = 0$ under these conditions may negate the intended benefit of turning off the SOI. A quantitative or qualitative discussion of charge noise and its impact on qubit performance at the SOI-off point is required.

We agree that the Rabi frequency plots indicate a steep incline with respect to the gate field. However it is not necessarily an indication of a higher sensitivity to charge noise. The dephasing rate of the qubit is mostly sensitive to longitudinal noise in the qubit subspace, or components of the noise that change the Larmor frequency. Regarding this, Fig. 2c and 2e suggest that close to the zero-SOI operation point ($F_z \approx 4.4 \text{ V}/\mu\text{m}$), the in-plane and out-of-plane components of the g -factor have very small inclines with respect to F_z and thus we do not expect a significant increase in charge noise sensitivity. In fact, the longitudinal component of the z operator, $|\langle 1|z|1\rangle - \langle 0|z|0\rangle|/2$, shows a minimum with respect to F_z near the zero-SOI operation point (see the figure just below). We added a sentence in the main text in the paragraph just before the conclusion to clarify this point.

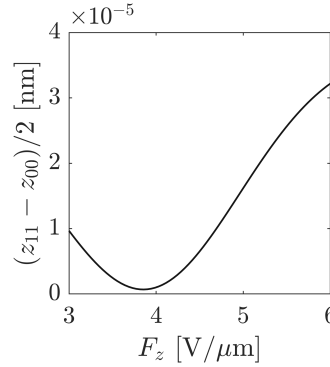


FIG. 2: Longitudinal component of the z operator. $l_x = l_y = 40 \text{ nm}$, $\mathbf{B} = (0.05 \text{ T})\mathbf{e}_y$.

5. The magnetic field is specified as $B_0 = 0.05 \text{ T}$. In Fig. 3, the field appears normalized, indicating that the results are independent of magnetic field magnitude within a reasonable range. The reason for choosing this specific value of magnetic field should be clarified.

The value of 0.05 T was chosen within a range of small magnetic fields such that the Rabi frequency vs B remains in a linear regime. Then dividing the Rabi frequency by $\mu_B B/\hbar$ allowed us to report values that are not specific to a particular magnetic field strength. It also ensures the lowest quantum dot doublet corresponds to the lowest spin-split orbital. For instance, we choose the characteristic lengths $l_{x,y}$ such that the orbital spacing $\Delta E \sim 0.1 \text{ meV}$. Then assuming an average g -factor $g \sim 5$, we find $B \ll \Delta E/(g\mu_B) \sim 0.34 \text{ T}$.

6. How is the specific value of Rabi frequency for a realistic qubit device? How does it compare with the ultrafast manipulation speed of 1.2 GHz observed in Ge NW?

An estimation of specific Rabi frequencies is generally tricky to make, since it is proportional to the electrical driving strength F_{ac} at the qubit location, which is hard to estimate theoretically. However around $F_z = 4 \text{ V}/\mu\text{m}$, the spin-dipole length $|\langle 0|y|1\rangle| \sim 1 \text{ nm}$ and the Larmor frequency is 5.5 GHz for $\mathbf{B} = (0.05 \text{ T})\mathbf{e}_y$. Then multiplying $|\langle 0|y|1\rangle|$ by $eF_{ac}/\hbar$ with $F_{ac} = 0.001 \text{ V}/\mu\text{m}$ gives $\Omega \approx 1.5 \text{ GHz}$. Similar estimates can be made from Fig. 3 in the main text, with $\omega_B \approx 4.40 \text{ GHz}$ at $B = 0.05 \text{ T}$. These numbers are not too far from the ultra-fast Rabi driving reported in Ge/Si core/shell nanowires (Nat. Nanotechnol. 16, 308-312 (2021)), notably with driving frequencies approaching the same order of magnitude as the Larmor frequency. This gives us confidence that our estimations are realistic.

7. The used sizes of quantum dots, such as l_x, l_y , are smaller than a planar Ge hole qubit (100 nm), which is unfavorable for device fabrication. The authors are suggested to choose sizes within experimental availability.

We acknowledge the confusion the sizes can bring. The lengths used throughout the calculations are not representative of the physical gate sizes, as they depend on voltage applied and the fictitious (bare electron) effective mass used, and not only on the gate size. They represent an effective confinement length to estimate the

effects of the electric field from top gates. We did not refer to them as 'radius' or 'diameter' for the same reason. This approach makes abstraction of gate patterns and voltages, while giving a qualitatively good description of the potential landscape near the center of the dot. A possibly more meaningful measure of confinement would be the orbital subband spacing between the QD ground state doublet and the next excited orbital. For instance, a length ~ 40 nm gives a subband spacing of roughly 0.1 meV, which is comparable to what is expected in HH systems, or slightly smaller. Given the larger in-plane effective masses of LHs compared to HHs, and consequently smaller orbital spacings, we believe that our dot sizes are appropriate in this context.

8. On page 4, "QC effective parameters $g_{\perp,\parallel}^c$ " should be " $g_{\perp}^c$ ".

We thank the referee for pointing this out. It is now corrected.

The authors are suggested to thoroughly address the above points before the paper can be recommended for publication.

We thank the Referee for the constructive comments. We believe we have addressed all their questions and the updated manuscript is now improved and ready for publication.

III. REPORT OF THE 3RD REFEREE

In "Fully Tunable Strong Spin-Orbit Interactions in Light-Hole Germanium Quantum Channels", Dr. Del Vecchio and coauthors present an asymmetric Ge/GeSn quantum well in which light-hole spin qubits can be defined. The vertical asymmetry of the well can be partially compensated by an applied vertical electric field, leading to operating ("idle") points where the spin-orbit coupling is effectively turned off. This could be advantageous as an idling regime with reduced sensitivity to noise. Away from this point, the system exhibits strong tunability and large manipulability enabled by the strong SOI of light-hole states. The work builds on previous contributions by some of the authors on Ge/GeSn heterostructures and takes a further step by considering elongated quantum dots.

Overall, I find the manuscript interesting and potentially suitable for publication. At the same time, several elements appear closely related to the authors' prior work on Ge/GeSn heterostructures, and the degree of conceptual advance relative to those papers is not fully clear from the current presentation. Below I provide comments that, if addressed, could strengthen the manuscript:

We would first like to thank the referee for the valuable comments on our work. Regarding the advance of this work relative to our previous works, which was also pointed out by the 2nd referee, we modified a sentence near the end of the introduction to better highlight the new aspects addressed in the present paper.

1. *If I understand correctly, the main mechanism behind the idling point is the restoration of inversion symmetry via a vertical electric field. This seems rather generic, in the sense that any sufficiently asymmetric device could exhibit similar behavior. I therefore wonder about the robustness of this effect. In practice, additional mechanisms can asymmetrically affect linear and cubic spin-orbit terms (alloy disorder, strain, reduced symmetry at interfaces, etc.). Is the idling point robust against such mechanisms? I think this should be discussed.*

We first wish to thank the reviewer for the constructive comments. We agree that additional mechanisms at the interfaces may affect the Rashba SOI in the heterostructure. We are aware of works such as Phys. Rev. B 69, 155321 (2004) that discuss such effects in III-V materials. However, incorporating this into $k \cdot p$ theory requires additional parameters that are not yet known for GeSn alloys. We did not want to introduce additional parameters into our analysis, and focused instead on the breaking of spatial symmetry since this effect is known to occur in group IV alloys and is well documented. We believe that a discussion on additional mechanisms affecting SOI would be more appropriate in a separate, dedicated publication. We added a sentence at the end of the Section "The proposed heterostructure" to justify this point. Regarding the corrections from shear strains, which was also pointed out by the 2nd referee, we estimate that the main effect on the LH will be similar to HHs by renormalizing the magnitude of the linear Rashba coefficient, see for instance Phys. Rev. Lett. 131, 097002 (2023) (Fig. S7 and Eq. S17). However this correction is about $\beta_1 \sim 0.01$ meV nm, which is much smaller than the values we report for LHs ($\beta_1 \sim 1$ meV nm).

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Alloy disorder may change the character of the light hole ground state to some extent, but we do not think it would qualitatively change the energy spectrum, notably since most of the wavefunction is located in pure Ge.

Disorder at the Ge/GeSn interface or at the oxide may as well affect spin orbit properties, but these would be hard to accurately predict. For instance the quality of the oxide interface generally depends on the nature of the oxide, on the amount of impurities at the GeSn surface and on the oxide growth technique. We believe that expanding these details would hinder on the generality of the work. A comment on this has been added at the end of the Section "The proposed heterostructure".

3. *The results are obtained using 6-band kp theory. However, if one includes the split-off band in Ge, should the conduction band also be included? The conduction band is farther away in energy than the split-off band, but they are of the same order of magnitude.*

We agree that the conduction band has a contribution that is about the same order of magnitude as the split-off band. Some of our previous calculations (e.g. Phys. Rev. B 110, 045409 (2024)) indicate that its total contribution is typically around 5%, in comparison to around 10% for the split-off band. However, most of the contributions for Rashba SOI comes from the SO band – for instance we calculate $\beta_1 \approx -1.26$ meV nm using the 6-band model, while with 8-band we get $\beta_1 \approx -1.56$ meV nm. We decided to not include it in our calculations for the sake of analytical simplicity.

4. *More generally, I find tunability through a vertical electric field experimentally delicate. Wouldn't one risk switching to a different occupation state while tuning the field?*

We thank the Referee for raising this point. We believe that tunability through the vertical field is not too problematic once the device is calibrated into a specific charge configuration. This is because, once a specific charge sector is reached, the dot can be decoupled from nearby reservoirs by increasing the barrier gate voltages. In this situation, the charges remain well isolated within the dot and the vertical gate field can be adjusted towards or away from the zero-SOI lines without changing the charge occupancy. Moreover, the large $\partial\Omega/\partial F_z$ near the β -line also ensures that a change in operation mode does not requires a large variation of F_z , thus relaxing the constraints on the applied gate voltage.

5. *I think the novelties of the paper, such as the evaluation of elongated dots in Ge/GeSn heterostructures should be emphasized more clearly throughout the manuscript to better distinguish it from prior works.*

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6. *When $\gamma_{\parallel}$ is introduced after Eq. (1), it is described as the effective mass. I believe it is instead the inverse of the (dimensionless) in-plane effective mass, consistent with what is written for the vertical one near the end of page 2.*

We thank the reviewer for pointing this out. It is now corrected

7. *On page 2, the manuscript states: "The Schrieffer-Wolff transformation employed to derive Eq. (1) provides exact formulas...". What is meant by "exact" here? Don't these parameters receive higher-order corrections?*

We acknowledge the confusion with perturbation theory providing exact results. The SWT procedure gives exact formulas for the effective parameters as long as the order of the transformation matches that of the k -power associated with that parameter. For instance, the effective mass is quadratic in k , and thus requires only a 2nd order SWT to be described exactly. This is because the full $k \cdot p$ Hamiltonian is built in a way such that it is diagonal at $\mathbf{k}_{\parallel} = 0$, while the off-diagonal part (the perturbation) is *at least* linear in k (see equations F1 – we also recall that $\alpha_0/l_B^2 = \mu_B B$ and B is quadratic in k). A 3rd order SWT or higher would not give any contributions to the effective mass since those would provide corrections that are at least cubic in k . Since the effective Hamiltonian presented in the main text is cubic in k , all the effective parameters therein are described exactly by a 3rd order SWT.

Responses to Reviewers' Comments

We thank the reviewers for their valuable and encouraging reports. In the following, we address their comments and provide additional insights into the key contributions reported in the manuscript.

I. REPORT OF THE 1ST REFEREE

1st round

The paper "Fully Tunable Strong Spin-Orbit Interactions in Light Hole Germanium Quantum Channels" by Del Vecchio et al. proposes and theoretically analyzes a novel Ge heterostructure that allows for switchable spin-orbit interactions via gate tuning. If successful, this would represent a significant advance in the field of semiconductor spin qubits, addressing a key challenge in achieving both fast qubit manipulation and long coherence times. The authors present a solid physical model and detailed calculations supporting their claims, which form the strongest part of the manuscript. However, the dense technical content, compromising space for in-depth discussion of experimental feasibility, limits the possible impact of the work. To support my recommendation for major revisions, I highlight the following key concerns:

1. *The paper is difficult to follow for potential readers; for instance, the origin of Hamiltonian (1) is not clearly given. The authors should provide the necessary references or derive it in the Supplementary Information (SI) to ensure reproducibility.*

We would first like to thank the referee for the constructive comments. The effective Hamiltonian (1) is derived from the full 6-band $k \cdot p$ Hamiltonian presented in the supplementary information. For the sake of clarity, we reorganized a large portion of the supplemental material and added more information in the "perturbation theory" section.

2. *The main outcomes of the paper are the parameters leading to the g - and β -lines presented in Figs. 2 and 3. While the motivation for on/off switching is given in the introduction, the implication of the g -line for leakage prevention is mentioned only briefly in the conclusion. The authors should expand on the practical implications of these findings (leakage mechanisms in particular) for qubit operation and qubit coherence.*

We thank the referee for bringing up this point. The g -lines effectively correspond to a gapless qubit, similarly to the gapless spin qubit studied in the recent work Phys. Rev. Lett. 135, 197001 (2025). Such qubits present many advantages for integration notably the lack of a global clock and full electric baseband control, and how they eliminate leakage channels through the large energy gap to the next excited states. Since these are discussed at length in that reference, we believe we do not need to expand much further on this in our work. We nevertheless added a comment just before the conclusion to elaborate a bit more on this point.

3. *In particular, while the robustness of the β -line is tested against θ , l_x , and l_y , sensitivity to electric field fluctuations is not discussed. In fact, the results presented in Fig. 3 suggest that the β -line is very steep in the F_z direction, which would make it highly sensitive to charge noise. The authors should comment on this point. Note that this implies an "off-state" with zero spin-orbit interaction is not necessarily a low-decoherence state if the noise sensitivity is high.*

We agree that the Rabi frequency graphs indicate a steep derivative with respect to the gate field. However we emphasize that it is not necessarily an indication of a higher sensitivity to charge noise. The dephasing rate of the qubit is mostly sensitive to longitudinal noise in the qubit subspace, or components of the noise that change the Larmor frequency. Regarding this, Fig. 2c and 2e suggest that close to the zero-SOI operation point ($F_z \approx 4.4 \text{ V}/\mu\text{m}$), the in-plane and out-of-plane components of the g -factor have very small slopes with respect to F_z and thus we do not expect a significant increase in charge noise sensitivity. In fact, the longitudinal component of the z operator, $|\langle 1|z|1\rangle - \langle 0|z|0\rangle|/2$, shows a minimum with respect to F_z near the zero-SOI operation point (see Fig. 1 below. We added a sentence in the main text just before the conclusion to address this point.)

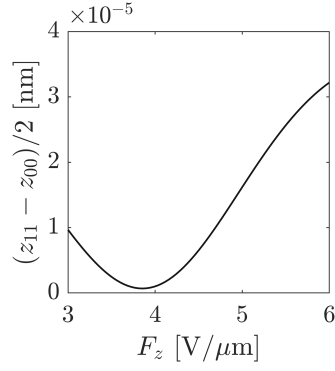


FIG. 1: Longitudinal component of the z operator. $l_x = l_y = 40$ nm, $\mathbf{B} = (0.05 \text{ T})\mathbf{e}_y$.

4. *There appears to be an inconsistency between Eq. (3) and the SI regarding the sign of $\gamma_{\parallel}^p$ (The authors should check Eq. 3 vs Eq. D11, for instance).*

We agree there is a sign inconsistency between equation D11 and equation 3. This is because we decided to use in the main text the usual convention of positive hole effective mass for convenience. On the other hand, in the supplemental material, the theory describes the hole effective mass within the electron picture and is therefore negative. $-\gamma_{\parallel} > 0$ and the square root in equation D11 is real. We added a comment at the beginning of the supplemental material to clarify this point.

5. *The abstract emphasizes: "The large in-plane g -factor also facilitates the integration of superconductors." Yet, after reading the manuscript, it is still not clear to me how this integration would be facilitated. In fact, a similar statement is repeated without reference or explanation at the end of the results section. The authors should clarify the physical mechanism behind this statement.*

We thank the referee for pointing this out. A large in-plane g -factor facilitates the integration of superconductors since they generally are substantially more resilient against in-plane magnetic fields compared to out-of-plane magnetic fields. For instance, Fig. 3 and Fig. 4a in Phys. Rev. Appl. 5, 044004 (2016) provide an indication that the quality factor of nanowire resonators in out-of-plane magnetic fields decreases much faster with B than in in-plane fields. In the updated manuscript, we included this reference to the main text.

6. *Finally, the authors assume a particular content of Sn in the GeSn buffer layer (16%) to achieve the light-hole ground state. The authors should put this number in the context of current experimental capabilities and comment on the feasibility of growing such structures with low defect densities.*

We thank the reviewer for bringing this issue, which is important from a practical standpoint. The growth of GeSn can be challenging given the metastable nature of this alloy and its large lattice parameter relative to that of silicon. However, by using nonequilibrium epitaxy growth processes it is possible to grow high-quality GeSn layers enabling their use in a range of photonic and optoelectronic applications. For an overview, see for Appl. Phys. Lett. 118, 110502 (2021). As described in that paper, device-quality GeSn heterostructures can be grown on silicon wafers by chemical vapor deposition using GeH_4 and SnCl_4 as precursors. Germanium interlayer is typically grown first on silicon to manage the large lattice mismatch. Following this process, our group grows routinely GeSn materials. We have demonstrated in the past the growth of GeSn at a concentration of 18% (see Appl. Phys. Lett. 112, 251903 (2018)). Thus, the proposed heterostructure is achievable using current epitaxy processes. We added a comment regarding this in the main text at the beginning of the Section "Light hole ground state requirements".

7. *Suggestion: To improve the readability and impact of the manuscript, the authors could consider moving some of the technical details to the SI and expand on the physical implications of their findings in the main text.*

We thank the referee for the suggestion. We added a few lines after equation 1 in the main text on the physical origins of the Rashba parameters appearing in the LH effective Hamiltonian, which may help differentiate LH ground states from conventional HH ground states.

Overall, the manuscript presents a novel pathway to achieve tunable spin-orbit interactions in Ge qubits. However, due to the concerns outlined above, I am unable to recommend publication in Communications Physics in the current form. I look forward to reviewing a revised manuscript that addresses these critical issues and incorporates the suggestions to improve clarity and impact.

We thank the Referee for their constructive feedback. We believe we have addressed all their comments and the updated manuscript is now improved and ready for publication.

2nd round

I thank the authors for their detailed responses and revision of the manuscript. I am satisfied with the explanations provided, and I think the revised manuscript has improved, in particular with the clarification of the role of longitudinal noise sensitivity at the zero-SOI operation point and the discussion of the feasibility of growing GeSn heterostructures. By reading the revised manuscript and the discussion with all the referees I am also more convinced of the novelty and potential impact of the work, which is now better distinguished from the authors' previous contributions. I therefore recommend publication in Communications Physics.

II. REPORT OF THE 2ND REFEREE

1st round

Patrick et al. present a theoretical study of tunable strong spin-orbit interaction (SOI) in light-hole (LH) Ge qubits using model Hamiltonian analysis. They propose a Ge heterostructure consisting of a GeSn buffer layer and a top barrier layer to introduce tensile biaxial strains and the resulting LH as the ground state, which exhibits k-linear SOI tenability that can be suppressed to zero in 2D planar, 1D channel, and 0D quantum dot geometries. They further report the tunable out-of-plane g-factor for 1D and 0D devices. Additionally, the Rabi frequency for the LH Ge qubit is estimated. The study is systematic and is related to a hot topic. However, partially claimed novel results of this manuscript have already been discussed in previous works by the same authors, such as strong SOI in LHs and the use of LH to carry a qubit (Refs. 56 and 57).

We thank the Referee for their assessment and for finding our work systematic and potentially impactful as related to an hot topic. In the updated version of the manuscript near the end of the Introduction, we better emphasize the novelty of this work compared to previous literature by noting the focus on squeezed dots within heterostructures with built-in vertical asymmetry.

1. *On the other hand, the introduction of Sn element into Si-based spin qubit is incompatible with current CMOS technology, sacrificing the most important advantage over other qubit platforms. In addition, the validity of the assumption in the model needs to be further verified.*

We respectfully but strongly disagree with this statement. The proposed GeSn-based heterostructure is compatible with silicon and silicon processing. Indeed, GeSn is a group IV isovalent alloy regarded as CMOS-compatible because it can be grown on silicon substrates using processes (e.g., CVD or MBE) that are compatible with standard silicon fabrication workflows and thermal budgets. Its integration leverages the same material platform as silicon and germanium. Importantly, qubits based on GeSn can benefit from nuclear spin-free isotopes and rely on the same underlying material system and fabrication approaches as other silicon-based qubits. This ensures that advanced quantum functionalities can be developed without breaking compatibility with scalable, industry-standard semiconductor manufacturing. For these reasons, the current manuscript focuses on this system.

2. *The claimed novelty of this work relies on the tunability of the SOI, which in turn depends critically on assuming an asymmetric confinement profile. This assumption is weakly justified. The proposed device is highly likely to introduce non-uniform strain instead of a uniform one. On the other hand, existing experimental and theoretical studies on Ge quantum wells are, to my knowledge, based on symmetric confinement profiles, and no clear experimental evidence for the proposed asymmetric confinement is provided. More importantly, the specific proposals suggested to achieve asymmetric confinement are not experimentally realistic and lack sufficient elaboration. A very thin Si capping layer would lead to significant gate leakage due to tunneling. Direct contact between an oxide layer and Ge is generally unfavorable in Ge-based heterostructures because the oxide of Ge is unstable and the Ge/oxide interface is known to host orders-of-magnitude higher defects than Si/SiO₂. In addition, placing the qubit closer to the gate would substantially enhance charge noise arising from gate-oxide interface defects, which is widely regarded as the dominant noise source in Ge qubits.*

We thank the referee for bringing up this detail and agree that Ge oxide is poor. However, high-quality oxides can be deposited by ALD, as shown in recent work: T. Aso et al. Materials Science in Semiconductor Processing 190, 109372 (2025). We stress that the basic requirement is a top barrier such that its band offset ΔE is large enough to prevent the light hole from leaking to the surface at strong vertical gate field, i.e. $\Delta E \gtrsim eF_z d$, where d is the thickness of the barrier. We put an emphasis on *thin* upper barriers to alleviate the requirement on ΔE . On hindsight, we agree that "a few ångströms" thick is far too restrictive, and could increase charge noise. Assuming the device operates at gate fields lower than $\sim 10 \text{ V}/\mu\text{m}$, a $d \lesssim 10 \text{ nm}$ upper barrier that prevents leakage of the hole to the surface would require a band offset $\gtrsim 100 \text{ meV}$ only, which is still satisfied using Si, or even a Si-rich SiGe alloy. We modified the main text to put more emphasis on the *no leaking* requirement, rather than on *very thin* barriers. We also added the above article in the list of references. Effects of non-uniform strain may also be present at the quantum well layer, but will fall-off with distance for thicker barriers. Nevertheless, its effects on the LH will likely be similar to HHs by rescaling the strength of the linear Rashba term. According to Phys. Rev. Lett. 131, 097002 (2023) (see Fig. S7 and Eq. S17 therein), shear strains introduce spin-orbit lengths $\ell_{\text{so}}^{-1} \sim 0.32 \mu\text{m}^{-1}$ in the Ge QW underneath a 50 nm top barrier. This translates to $\beta_1 \sim \hbar^2/(m^* \ell_{\text{so}}) \sim 0.01 \text{ meV nm}$, which is much smaller than the $\beta_1 \sim 1 \text{ meV nm}$ we report for LHs. Therefore, we believe the presence of shear strains would not have qualitative effects on our results, even considering thinner top barriers.

3. The authors use a Sn content of 10% in plotting the energy confinement profile in Fig.1(b), but substitute it with 16% in the following content. Why not use a consistent value and write it clearly throughout the manuscript rather than using x in $\text{Ge}_{1-x}\text{Sn}_x$?

We decided to plot the envelope functions at 10% Sn content in Fig. 1 only for better visuals, i.e. one could easily see the three types of confined levels (LHs, HHs at the Ge/GeSn interface and those at the surface barrier) with the best proportions. We added a sentence in the caption of Fig. 1 in the main text to clarify this.

4. In Fig. 3(b, c), the SOI-off points show large $\partial\Omega/\partial F_z$, indicating strong sensitivity to charge noise. Given the magnified charge noise in the proposed device, idling the qubit at $\Omega = 0$ under these conditions may negate the intended benefit of turning off the SOI. A quantitative or qualitative discussion of charge noise and its impact on qubit performance at the SOI-off point is required.

We agree that the Rabi frequency plots indicate a steep incline with respect to the gate field. However it is not necessarily an indication of a higher sensitivity to charge noise. The dephasing rate of the qubit is mostly sensitive to longitudinal noise in the qubit subspace, or components of the noise that change the Larmor frequency. Regarding this, Fig. 2c and 2e suggest that close to the zero-SOI operation point ($F_z \approx 4.4 \text{ V}/\mu\text{m}$), the in-plane and out-of-plane components of the g -factor have very small inclines with respect to F_z and thus we do not expect a significant increase in charge noise sensitivity. In fact, the longitudinal component of the z operator, $|\langle 1|z|1\rangle - \langle 0|z|0\rangle|/2$, shows a minimum with respect to F_z near the zero-SOI operation point (see the figure just below). We added a sentence in the main text in the paragraph just before the conclusion to clarify this point.

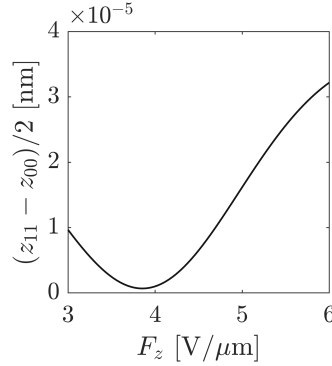


FIG. 2: Longitudinal component of the z operator. $l_x = l_y = 40 \text{ nm}$, $\mathbf{B} = (0.05 \text{ T})\mathbf{e}_y$.

5. The magnetic field is specified as $B_0 = 0.05 \text{ T}$. In Fig. 3, the field appears normalized, indicating that the results are independent of magnetic field magnitude within a reasonable range. The reason for choosing this specific value of magnetic field should be clarified.

The value of 0.05 T was chosen within a range of small magnetic fields such that the Rabi frequency vs B remains in a linear regime. Then dividing the Rabi frequency by $\mu_B B/\hbar$ allowed us to report values that are not specific to a particular magnetic field strength. It also ensures the lowest quantum dot doublet corresponds to the lowest spin-split orbital. For instance, we choose the characteristic lengths $l_{x,y}$ such that the orbital spacing $\Delta E \sim 0.1 \text{ meV}$. Then assuming an average g -factor $g \sim 5$, we find $B \ll \Delta E/(g\mu_B) \sim 0.34 \text{ T}$.

6. How is the specific value of Rabi frequency for a realistic qubit device? How does it compare with the ultrafast manipulation speed of 1.2 GHz observed in Ge NW?

An estimation of specific Rabi frequencies is generally tricky to make, since it is proportional to the electrical driving strength F_{ac} at the qubit location, which is hard to estimate theoretically. However around $F_z = 4 \text{ V}/\mu\text{m}$, the spin-dipole length $|\langle 0|y|1\rangle| \sim 1 \text{ nm}$ and the Larmor frequency is 5.5 GHz for $\mathbf{B} = (0.05 \text{ T})\mathbf{e}_y$. Then multiplying $|\langle 0|y|1\rangle|$ by $eF_{ac}/\hbar$ with $F_{ac} = 0.001 \text{ V}/\mu\text{m}$ gives $\Omega \approx 1.5 \text{ GHz}$. Similar estimates can be made from Fig. 3 in the main text, with $\omega_B \approx 4.40 \text{ GHz}$ at $B = 0.05 \text{ T}$. These numbers are not too far from the ultra-fast Rabi driving reported in Ge/Si core/shell nanowires (Nat. Nanotechnol. 16, 308-312 (2021)), notably with driving frequencies approaching the same order of magnitude as the Larmor frequency. This gives us confidence that our estimations are realistic.

7. The used sizes of quantum dots, such as l_x, l_y , are smaller than a planar Ge hole qubit (100 nm), which is unfavorable for device fabrication. The authors are suggested to choose sizes within experimental availability.

We acknowledge the confusion the sizes can bring. The lengths used throughout the calculations are not representative of the physical gate sizes, as they depend on voltage applied and the fictitious (bare electron) effective mass used, and not only on the gate size. They represent an effective confinement length to estimate the effects of the electric field from top gates. We did not refer to them as 'radius' or 'diameter' for the same reason. This approach makes abstraction of gate patterns and voltages, while giving a qualitatively good description of the potential landscape near the center of the dot. A possibly more meaningful measure of confinement would be the orbital subband spacing between the QD ground state doublet and the next excited orbital. For instance, a length ~ 40 nm gives a subband spacing of roughly 0.1 meV, which is comparable to what is expected in HH systems, or slightly smaller. Given the larger in-plane effective masses of LHs compared to HHs, and consequently smaller orbital spacings, we believe that our dot sizes are appropriate in this context.

8. On page 4, “QC effective parameters $g_{\perp,\parallel}^c$ ” should be “ $g_{\perp}^c$ ”.

We thank the referee for pointing this out. It is now corrected.

The authors are suggested to thoroughly address the above points before the paper can be recommended for publication.

We thank the Referee for the constructive comments. We believe we have addressed all their questions and the updated manuscript is now improved and ready for publication.

2nd round

The authors have answered my questions properly. I agree its publication in its current form.

III. REPORT OF THE 3RD REFEREE

1st round

In “Fully Tunable Strong Spin-Orbit Interactions in Light-Hole Germanium Quantum Channels”, Dr. Del Vecchio and coauthors present an asymmetric Ge/GeSn quantum well in which light-hole spin qubits can be defined. The vertical asymmetry of the well can be partially compensated by an applied vertical electric field, leading to operating (“idle”) points where the spin-orbit coupling is effectively turned off. This could be advantageous as an idling regime with reduced sensitivity to noise. Away from this point, the system exhibits strong tunability and large manipulability enabled by the strong SOI of light-hole states. The work builds on previous contributions by some of the authors on Ge/GeSn heterostructures and takes a further step by considering elongated quantum dots.

Overall, I find the manuscript interesting and potentially suitable for publication. At the same time, several elements appear closely related to the authors’ prior work on Ge/GeSn heterostructures, and the degree of conceptual advance relative to those papers is not fully clear from the current presentation. Below I provide comments that, if addressed, could strengthen the manuscript:

We would first like to thank the referee for the valuable comments on our work. Regarding the advance of this work relative to our previous works, which was also pointed out by the 2nd referee, we modified a sentence near the end of the introduction to better highlight the new aspects addressed in the present paper.

1. *If I understand correctly, the main mechanism behind the idling point is the restoration of inversion symmetry via a vertical electric field. This seems rather generic, in the sense that any sufficiently asymmetric device could exhibit similar behavior. I therefore wonder about the robustness of this effect. In practice, additional mechanisms can asymmetrically affect linear and cubic spin-orbit terms (alloy disorder, strain, reduced symmetry at interfaces, etc.). Is the idling point robust against such mechanisms? I think this should be discussed.*

We first wish to thank the reviewer for the constructive comments. We agree that additional mechanisms at the interfaces may affect the Rashba SOI in the heterostructure. We are aware of works such as Phys. Rev. B 69, 155321 (2004) that discuss such effects in III-V materials. However, incorporating this into $k \cdot p$ theory requires additional parameters that are not yet known for GeSn alloys. We did not want to introduce additional parameters into our analysis, and focused instead on the breaking of spatial symmetry since this effect is known to occur in group IV alloys and is well documented. We believe that a discussion on additional mechanisms affecting SOI would be more appropriate in a separate, dedicated publication. We added a sentence at the end of the Section “The proposed heterostructure” to justify this point. Regarding the corrections from shear strains, which was also pointed out by the 2nd referee, we estimate that the main effect on the LH will be similar to HHs by renormalizing the magnitude of the linear Rashba coefficient, see for instance Phys. Rev. Lett. 131, 097002 (2023) (Fig. S7 and Eq. S17). However this correction is about $\beta_1 \sim 0.01$ meV nm, which is much smaller than the values we report for LHs ($\beta_1 \sim 1$ meV nm).

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Alloy disorder may change the character of the light hole ground state to some extent, but we do not think it would qualitatively change the energy spectrum, notably since most of the wavefunction is located in pure Ge. Disorder at the Ge/GeSn interface or at the oxide may as well affect spin orbit properties, but these would be hard to accurately predict. For instance the quality of the oxide interface generally depends on the nature of the oxide, on the amount of impurities at the GeSn surface and on the oxide growth technique. We believe that expanding these details would hinder on the generality of the work. A comment on this has been added at the end of the Section “The proposed heterostructure”.

3. *The results are obtained using 6-band kp theory. However, if one includes the split-off band in Ge, should the conduction band also be included? The conduction band is farther away in energy than the split-off band, but they are of the same order of magnitude.*

We agree that the conduction band has a contribution that is about the same order of magnitude as the split-off band. Some of our previous calculations (e.g. Phys. Rev. B 110, 045409 (2024)) indicate that its total contribution is typically around 5%, in comparison to around 10% for the split-off band. However, most of the contributions for Rashba SOI comes from the SO band – for instance we calculate $\beta_1 \approx -1.26$ meV nm using the 6-band model, while with 8-band we get $\beta_1 \approx -1.56$ meV nm. We decided to not include it in our calculations for the sake of analytical simplicity.

4. *More generally, I find tunability through a vertical electric field experimentally delicate. Wouldn't one risk switching to a different occupation state while tuning the field?*

We thank the Referee for raising this point. We believe that tunability through the vertical field is not too problematic once the device is calibrated into a specific charge configuration. This is because, once a specific charge sector is reached, the dot can be decoupled from nearby reservoirs by increasing the barrier gate voltages. In this situation, the charges remain well isolated within the dot and the vertical gate field can be adjusted towards or away from the zero-SOI lines without changing the charge occupancy. Moreover, the large $\partial\Omega/\partial F_z$ near the β -line also ensures that a change in operation mode does not require a large variation of F_z , thus relaxing the constraints on the applied gate voltage.

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We acknowledge the confusion with perturbation theory providing exact results. The SWT procedure gives exact formulas for the effective parameters as long as the order of the transformation matches that of the k -power associated with that parameter. For instance, the effective mass is quadratic in k , and thus requires only a 2nd order SWT to be described exactly. This is because the full $k \cdot p$ Hamiltonian is built in a way such that it is diagonal at $\mathbf{k}_{\parallel} = 0$, while the off-diagonal part (the perturbation) is *at least* linear in k (see equations F1 – we also recall that $\alpha_0/l_B^2 = \mu_B B$ and B is quadratic in k). A 3rd order SWT or higher would not give any contributions to the effective mass since those would provide corrections that are at least cubic in k . Since the effective Hamiltonian presented in the main text is cubic in k , all the effective parameters therein are described exactly by a 3rd order SWT.

2nd round

I thank the authors for their responses to the previous comments and for the revisions made to the manuscript. The authors have addressed several of the points raised in the first round. However, two relevant points regarding the robustness of the light-hole (LH) ground state and the influence of inhomogeneous strain remain insufficiently addressed.

1. *In the first round, I raised the issue of whether alloy disorder could affect the light-hole character of the ground state. The authors respond by stating that they do not expect a qualitative change in the energy spectrum since most of the wavefunction resides in pure Ge. However, my concern was more specific: can alloy disorder in the $\text{Ge}_{1-x}\text{Sn}_x$ buffer layer introduce significant local fluctuations in the biaxial strain? Such fluctuations could, in turn, lead to substantial changes in the HH-LH splitting. Given that the LH ground state relies on a delicate balance between tensile strain and confinement, I believe the authors should clarify whether these strain fluctuations are expected to be small enough or if they could potentially alter the LH character of the state in certain regions of the device. A more quantitative response would be appreciated as the current response is purely qualitative.*

We thank the reviewer for raising this important point regarding the possible impact of alloy disorder on the LH ground state. We agree that the stability of the LH character depends critically on the strain configuration and HH-LH splitting. However, based on both our previous experimental investigations of GeSn alloys and the results presented in this work, we do not find evidence that alloy disorder introduces strain fluctuations large enough to qualitatively modify the LH nature of the ground state.

First, our experimental observations on Sn-rich GeSn alloys did not indicate any measurable strain fluctuations. In an earlier study on GeSn alloys with Sn compositions around 18% (directly relevant to the present manuscript),

we investigated the optical emission using different excitation wavelengths probing different depths of the material [Atomically uniform Sn-rich GeSn semiconductors with 3.0–3.5 μm room-temperature optical emission, Appl. Phys. Lett. 112, 251903 (2018)]. We found that the emission wavelength remained unchanged regardless of the excitation depth. Since the emission energy is highly sensitive to strain, this observation indicates the absence of measurable strain variations along the growth direction (z -direction).

In the same study, atom probe tomography measurements demonstrated that the GeSn alloy exhibits a random alloy distribution without measurable compositional clustering or local compositional fluctuations at the atomic scale. These observations strongly suggest that the alloy disorder does not generate substantial local strain inhomogeneities.

More recently, we investigated GeSn-based heavy-hole quantum wells through magnetotransport measurements [GeSn quantum wells as a platform for spin-resolved hole transport, Communications Materials 5, 71 (2024)]. In that work, the experimental observations related to the spin-orbit interaction strength and subband structure were quantitatively reproduced using $k \cdot p$ calculations assuming a uniform biaxial strain across the quantum well. The excellent agreement between experiment and theory indicates that the strain landscape can be accurately described within the framework of homogeneous biaxial strain, without invoking additional local strain fluctuations associated with alloy disorder.

Taken together, these experimental observations indicate that any local strain fluctuations induced by alloy disorder are sufficiently small compared to the overall engineered tensile strain and confinement energies governing the HH-LH splitting. Therefore, while alloy disorder may introduce minor local perturbations, we do not expect it to alter the qualitative LH character of the ground state or produce spatial regions with a different HH/LH ordering.

We also investigated in the past the epitaxial growth of tensile strain Ge quantum wells on GeSn [A Light-Hole Germanium Quantum Well on Silicon, Advanced Materials 43, 2201192 (2022)]. In that work, we performed reciprocal space mapping using high-resolution X-ray diffraction to evaluate the strain in the grown Ge/GeSn heterostructure. The effect of the coherent growth of Ge on the GeSn layers was clear in the XRD (004) symmetric scan (Fig. S2a in that work). In the absence of the Ge layer the GeSn-related peaks are observed between 64.2° and 65.5° . When the 12.5 nm-thick Ge quantum well is introduced in the stacking, these GeSn-related peaks shift to 64.0° and $\sim 64.25^\circ$. In addition, Pendellosung fringes are observed below 64.0° , which originate from the phase shift between the scattered waves induced by the variation in the out-of-plane lattice parameter across the GeSn/Ge/GeSn stacking. This is an additional indication that the growth of Ge is pseudomorphic with no detectable fluctuations.

2. *Regarding the effects of non-uniform strain raised by Referee 2, the authors argue that corrections to the Rashba interaction are small, citing Phys. Rev. Lett. 131, 097002 (2023). However, the system analyzed in that PRL paper assumes a Ge quantum well where the HH-LH energy separation is relatively large. In the device proposed in this manuscript, the LH and HH states can be much closer in energy. Since the spin-orbit length (and thus the Rashba correction) is inversely proportional to the HH-LH energy difference, it is likely that the corrections from inhomogeneous strain would be significantly enhanced in the authors' proposed structure compared to the one in the cited PRL. Can we really discard these corrections in a regime where the HH-LH gap is small? I am not asking for a detailed simulation including such effects but discussing the impact of this type of correction is relevant.*

We thank the referee for bringing this point, and agree about the relevance of such effects. The LH-HH splitting energy in Ge/SiGe heterostructures is expected to be around 50–75 meV, depending on the amount of residual epitaxial strain in the SiGe barriers and on the well thickness. This range is not much larger than the splitting for the system analysed in our manuscript; we agree that for small Sn contents and thin wells, confinement competes with strain and can make the LH-HH splitting arbitrarily small. This is confirmed by the bold black line in Fig. 1c, for instance. However, this region is avoided by ensuring a Ge layer thickness > 10 nm. In this case the LH-HH splitting is around ~ 30 meV near the operation point $F_z \approx 4.4$ V/ μm . This is smaller than Ge/SiGe systems but still in the same order of magnitude. Given the argument above and that given to referee II, we believe such effects can be neglected in our manuscript. We added a sentence at the end of the section “planar ground state properties” to better emphasize that we focus on systems with $\Delta E \gtrsim 20$ meV such that the LH ground state remains well separated from the nearest HH level.

Responses to Reviewers' Comments

We thank the reviewers for their valuable and encouraging reports. In the following, we address their comments and provide additional insights into the key contributions reported in the manuscript.

I. REPORT OF THE 1ST REFEREE

1st round

The paper "Fully Tunable Strong Spin-Orbit Interactions in Light Hole Germanium Quantum Channels" by Del Vecchio et al. proposes and theoretically analyzes a novel Ge heterostructure that allows for switchable spin-orbit interactions via gate tuning. If successful, this would represent a significant advance in the field of semiconductor spin qubits, addressing a key challenge in achieving both fast qubit manipulation and long coherence times. The authors present a solid physical model and detailed calculations supporting their claims, which form the strongest part of the manuscript. However, the dense technical content, compromising space for in-depth discussion of experimental feasibility, limits the possible impact of the work. To support my recommendation for major revisions, I highlight the following key concerns:

1. *The paper is difficult to follow for potential readers; for instance, the origin of Hamiltonian (1) is not clearly given. The authors should provide the necessary references or derive it in the Supplementary Information (SI) to ensure reproducibility.*

We would first like to thank the referee for the constructive comments. The effective Hamiltonian (1) is derived from the full 6-band $k \cdot p$ Hamiltonian presented in the supplementary information. For the sake of clarity, we reorganized a large portion of the supplemental material and added more information in the "perturbation theory" section.

2. *The main outcomes of the paper are the parameters leading to the g - and β -lines presented in Figs. 2 and 3. While the motivation for on/off switching is given in the introduction, the implication of the g -line for leakage prevention is mentioned only briefly in the conclusion. The authors should expand on the practical implications of these findings (leakage mechanisms in particular) for qubit operation and qubit coherence.*

We thank the referee for bringing up this point. The g -lines effectively correspond to a gapless qubit, similarly to the gapless spin qubit studied in the recent work Phys. Rev. Lett. 135, 197001 (2025). Such qubits present many advantages for integration notably the lack of a global clock and full electric baseband control, and how they eliminate leakage channels through the large energy gap to the next excited states. Since these are discussed at length in that reference, we believe we do not need to expand much further on this in our work. We nevertheless added a comment just before the conclusion to elaborate a bit more on this point.

3. *In particular, while the robustness of the β -line is tested against θ , l_x , and l_y , sensitivity to electric field fluctuations is not discussed. In fact, the results presented in Fig. 3 suggest that the β -line is very steep in the F_z direction, which would make it highly sensitive to charge noise. The authors should comment on this point. Note that this implies an "off-state" with zero spin-orbit interaction is not necessarily a low-decoherence state if the noise sensitivity is high.*

We agree that the Rabi frequency graphs indicate a steep derivative with respect to the gate field. However we emphasize that it is not necessarily an indication of a higher sensitivity to charge noise. The dephasing rate of the qubit is mostly sensitive to longitudinal noise in the qubit subspace, or components of the noise that change the Larmor frequency. Regarding this, Fig. 2c and 2e suggest that close to the zero-SOI operation point ($F_z \approx 4.4 \text{ V}/\mu\text{m}$), the in-plane and out-of-plane components of the g -factor have very small slopes with respect to F_z and thus we do not expect a significant increase in charge noise sensitivity. In fact, the longitudinal component of the z operator, $|\langle 1|z|1\rangle - \langle 0|z|0\rangle|/2$, shows a minimum with respect to F_z near the zero-SOI operation point (see Fig. 1 below. We added a sentence in the main text just before the conclusion to address this point.)

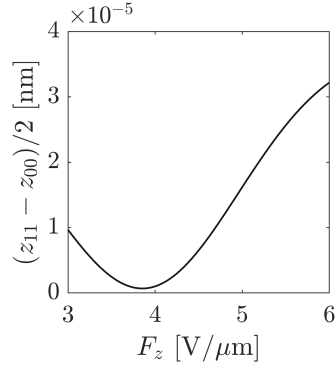


FIG. 1: Longitudinal component of the z operator. $l_x = l_y = 40$ nm, $\mathbf{B} = (0.05 \text{ T})\mathbf{e}_y$.

4. *There appears to be an inconsistency between Eq. (3) and the SI regarding the sign of $\gamma_{\parallel}^p$ (The authors should check Eq. 3 vs Eq. D11, for instance).*

We agree there is a sign inconsistency between equation D11 and equation 3. This is because we decided to use in the main text the usual convention of positive hole effective mass for convenience. On the other hand, in the supplemental material, the theory describes the hole effective mass within the electron picture and is therefore negative. $-\gamma_{\parallel} > 0$ and the square root in equation D11 is real. We added a comment at the beginning of the supplemental material to clarify this point.

5. *The abstract emphasizes: "The large in-plane g -factor also facilitates the integration of superconductors." Yet, after reading the manuscript, it is still not clear to me how this integration would be facilitated. In fact, a similar statement is repeated without reference or explanation at the end of the results section. The authors should clarify the physical mechanism behind this statement.*

We thank the referee for pointing this out. A large in-plane g -factor facilitates the integration of superconductors since they generally are substantially more resilient against in-plane magnetic fields compared to out-of-plane magnetic fields. For instance, Fig. 3 and Fig. 4a in Phys. Rev. Appl. 5, 044004 (2016) provide an indication that the quality factor of nanowire resonators in out-of-plane magnetic fields decreases much faster with B than in in-plane fields. In the updated manuscript, we included this reference to the main text.

6. *Finally, the authors assume a particular content of Sn in the GeSn buffer layer (16%) to achieve the light-hole ground state. The authors should put this number in the context of current experimental capabilities and comment on the feasibility of growing such structures with low defect densities.*

We thank the reviewer for bringing this issue, which is important from a practical standpoint. The growth of GeSn can be challenging given the metastable nature of this alloy and its large lattice parameter relative to that of silicon. However, by using nonequilibrium epitaxy growth processes it is possible to grow high-quality GeSn layers enabling their use in a range of photonic and optoelectronic applications. For an overview, see for Appl. Phys. Lett. 118, 110502 (2021). As described in that paper, device-quality GeSn heterostructures can be grown on silicon wafers by chemical vapor deposition using GeH_4 and SnCl_4 as precursors. Germanium interlayer is typically grown first on silicon to manage the large lattice mismatch. Following this process, our group grows routinely GeSn materials. We have demonstrated in the past the growth of GeSn at a concentration of 18% (see Appl. Phys. Lett. 112, 251903 (2018)). Thus, the proposed heterostructure is achievable using current epitaxy processes. We added a comment regarding this in the main text at the beginning of the Section "Light hole ground state requirements".

7. *Suggestion: To improve the readability and impact of the manuscript, the authors could consider moving some of the technical details to the SI and expand on the physical implications of their findings in the main text.*

We thank the referee for the suggestion. We added a few lines after equation 1 in the main text on the physical origins of the Rashba parameters appearing in the LH effective Hamiltonian, which may help differentiate LH ground states from conventional HH ground states.

Overall, the manuscript presents a novel pathway to achieve tunable spin-orbit interactions in Ge qubits. However, due to the concerns outlined above, I am unable to recommend publication in Communications Physics in the current form. I look forward to reviewing a revised manuscript that addresses these critical issues and incorporates the suggestions to improve clarity and impact.

We thank the Referee for their constructive feedback. We believe we have addressed all their comments and the updated manuscript is now improved and ready for publication.

2nd round

I thank the authors for their detailed responses and revision of the manuscript. I am satisfied with the explanations provided, and I think the revised manuscript has improved, in particular with the clarification of the role of longitudinal noise sensitivity at the zero-SOI operation point and the discussion of the feasibility of growing GeSn heterostructures. By reading the revised manuscript and the discussion with all the referees I am also more convinced of the novelty and potential impact of the work, which is now better distinguished from the authors' previous contributions. I therefore recommend publication in Communications Physics.

II. REPORT OF THE 2ND REFEREE

1st round

Patrick et al. present a theoretical study of tunable strong spin-orbit interaction (SOI) in light-hole (LH) Ge qubits using model Hamiltonian analysis. They propose a Ge heterostructure consisting of a GeSn buffer layer and a top barrier layer to introduce tensile biaxial strains and the resulting LH as the ground state, which exhibits k-linear SOI tenability that can be suppressed to zero in 2D planar, 1D channel, and 0D quantum dot geometries. They further report the tunable out-of-plane g-factor for 1D and 0D devices. Additionally, the Rabi frequency for the LH Ge qubit is estimated. The study is systematic and is related to a hot topic. However, partially claimed novel results of this manuscript have already been discussed in previous works by the same authors, such as strong SOI in LHs and the use of LH to carry a qubit (Refs. 56 and 57).

We thank the Referee for their assessment and for finding our work systematic and potentially impactful as related to an hot topic. In the updated version of the manuscript near the end of the Introduction, we better emphasize the novelty of this work compared to previous literature by noting the focus on squeezed dots within heterostructures with built-in vertical asymmetry.

1. *On the other hand, the introduction of Sn element into Si-based spin qubit is incompatible with current CMOS technology, sacrificing the most important advantage over other qubit platforms. In addition, the validity of the assumption in the model needs to be further verified.*

We respectfully but strongly disagree with this statement. The proposed GeSn-based heterostructure is compatible with silicon and silicon processing. Indeed, GeSn is a group IV isovalent alloy regarded as CMOS-compatible because it can be grown on silicon substrates using processes (e.g., CVD or MBE) that are compatible with standard silicon fabrication workflows and thermal budgets. Its integration leverages the same material platform as silicon and germanium. Importantly, qubits based on GeSn can benefit from nuclear spin-free isotopes and rely on the same underlying material system and fabrication approaches as other silicon-based qubits. This ensures that advanced quantum functionalities can be developed without breaking compatibility with scalable, industry-standard semiconductor manufacturing. For these reasons, the current manuscript focuses on this system.

2. *The claimed novelty of this work relies on the tunability of the SOI, which in turn depends critically on assuming an asymmetric confinement profile. This assumption is weakly justified. The proposed device is highly likely to introduce non-uniform strain instead of a uniform one. On the other hand, existing experimental and theoretical studies on Ge quantum wells are, to my knowledge, based on symmetric confinement profiles, and no clear experimental evidence for the proposed asymmetric confinement is provided. More importantly, the specific proposals suggested to achieve asymmetric confinement are not experimentally realistic and lack sufficient elaboration. A very thin Si capping layer would lead to significant gate leakage due to tunneling. Direct contact between an oxide layer and Ge is generally unfavorable in Ge-based heterostructures because the oxide of Ge is unstable and the Ge/oxide interface is known to host orders-of-magnitude higher defects than Si/SiO₂. In addition, placing the qubit closer to the gate would substantially enhance charge noise arising from gate-oxide interface defects, which is widely regarded as the dominant noise source in Ge qubits.*

We thank the referee for bringing up this detail and agree that Ge oxide is poor. However, high-quality oxides can be deposited by ALD, as shown in recent work: T. Aso et al. Materials Science in Semiconductor Processing 190, 109372 (2025). We stress that the basic requirement is a top barrier such that its band offset ΔE is large enough to prevent the light hole from leaking to the surface at strong vertical gate field, i.e. $\Delta E \gtrsim eF_z d$, where d is the thickness of the barrier. We put an emphasis on *thin* upper barriers to alleviate the requirement on ΔE . On hindsight, we agree that "a few ångströms" thick is far too restrictive, and could increase charge noise. Assuming the device operates at gate fields lower than $\sim 10 \text{ V}/\mu\text{m}$, a $d \lesssim 10 \text{ nm}$ upper barrier that prevents leakage of the hole to the surface would require a band offset $\gtrsim 100 \text{ meV}$ only, which is still satisfied using Si, or even a Si-rich SiGe alloy. We modified the main text to put more emphasis on the *no leaking* requirement, rather than on *very thin* barriers. We also added the above article in the list of references. Effects of non-uniform strain may also be present at the quantum well layer, but will fall-off with distance for thicker barriers. Nevertheless, its effects on the LH will likely be similar to HHs by rescaling the strength of the linear Rashba term. According to Phys. Rev. Lett. 131, 097002 (2023) (see Fig. S7 and Eq. S17 therein), shear strains introduce spin-orbit lengths $\ell_{\text{so}}^{-1} \sim 0.32 \mu\text{m}^{-1}$ in the Ge QW underneath a 50 nm top barrier. This translates to $\beta_1 \sim \hbar^2/(m^* \ell_{\text{so}}) \sim 0.01 \text{ meV nm}$, which is much smaller than the $\beta_1 \sim 1 \text{ meV nm}$ we report for LHs. Therefore, we believe the presence of shear strains would not have qualitative effects on our results, even considering thinner top barriers.

3. The authors use a Sn content of 10% in plotting the energy confinement profile in Fig.1(b), but substitute it with 16% in the following content. Why not use a consistent value and write it clearly throughout the manuscript rather than using x in $\text{Ge}_{1-x}\text{Sn}_x$?

We decided to plot the envelope functions at 10% Sn content in Fig. 1 only for better visuals, i.e. one could easily see the three types of confined levels (LHs, HHs at the Ge/GeSn interface and those at the surface barrier) with the best proportions. We added a sentence in the caption of Fig. 1 in the main text to clarify this.

4. In Fig. 3(b, c), the SOI-off points show large $\partial\Omega/\partial F_z$, indicating strong sensitivity to charge noise. Given the magnified charge noise in the proposed device, idling the qubit at $\Omega = 0$ under these conditions may negate the intended benefit of turning off the SOI. A quantitative or qualitative discussion of charge noise and its impact on qubit performance at the SOI-off point is required.

We agree that the Rabi frequency plots indicate a steep incline with respect to the gate field. However it is not necessarily an indication of a higher sensitivity to charge noise. The dephasing rate of the qubit is mostly sensitive to longitudinal noise in the qubit subspace, or components of the noise that change the Larmor frequency. Regarding this, Fig. 2c and 2e suggest that close to the zero-SOI operation point ($F_z \approx 4.4 \text{ V}/\mu\text{m}$), the in-plane and out-of-plane components of the g -factor have very small inclines with respect to F_z and thus we do not expect a significant increase in charge noise sensitivity. In fact, the longitudinal component of the z operator, $|\langle 1|z|1\rangle - \langle 0|z|0\rangle|/2$, shows a minimum with respect to F_z near the zero-SOI operation point (see the figure just below). We added a sentence in the main text in the paragraph just before the conclusion to clarify this point.

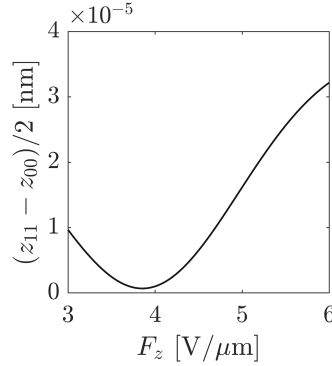


FIG. 2: Longitudinal component of the z operator. $l_x = l_y = 40 \text{ nm}$, $\mathbf{B} = (0.05 \text{ T})\mathbf{e}_y$.

5. The magnetic field is specified as $B_0 = 0.05 \text{ T}$. In Fig. 3, the field appears normalized, indicating that the results are independent of magnetic field magnitude within a reasonable range. The reason for choosing this specific value of magnetic field should be clarified.

The value of 0.05 T was chosen within a range of small magnetic fields such that the Rabi frequency vs B remains in a linear regime. Then dividing the Rabi frequency by $\mu_B B/\hbar$ allowed us to report values that are not specific to a particular magnetic field strength. It also ensures the lowest quantum dot doublet corresponds to the lowest spin-split orbital. For instance, we choose the characteristic lengths $l_{x,y}$ such that the orbital spacing $\Delta E \sim 0.1 \text{ meV}$. Then assuming an average g -factor $g \sim 5$, we find $B \ll \Delta E/(g\mu_B) \sim 0.34 \text{ T}$.

6. How is the specific value of Rabi frequency for a realistic qubit device? How does it compare with the ultrafast manipulation speed of 1.2 GHz observed in Ge NW?

An estimation of specific Rabi frequencies is generally tricky to make, since it is proportional to the electrical driving strength F_{ac} at the qubit location, which is hard to estimate theoretically. However around $F_z = 4 \text{ V}/\mu\text{m}$, the spin-dipole length $|\langle 0|y|1\rangle| \sim 1 \text{ nm}$ and the Larmor frequency is 5.5 GHz for $\mathbf{B} = (0.05 \text{ T})\mathbf{e}_y$. Then multiplying $|\langle 0|y|1\rangle|$ by $eF_{ac}/\hbar$ with $F_{ac} = 0.001 \text{ V}/\mu\text{m}$ gives $\Omega \approx 1.5 \text{ GHz}$. Similar estimates can be made from Fig. 3 in the main text, with $\omega_B \approx 4.40 \text{ GHz}$ at $B = 0.05 \text{ T}$. These numbers are not too far from the ultra-fast Rabi driving reported in Ge/Si core/shell nanowires (Nat. Nanotechnol. 16, 308-312 (2021)), notably with driving frequencies approaching the same order of magnitude as the Larmor frequency. This gives us confidence that our estimations are realistic.

7. The used sizes of quantum dots, such as l_x, l_y , are smaller than a planar Ge hole qubit (100 nm), which is unfavorable for device fabrication. The authors are suggested to choose sizes within experimental availability.

We acknowledge the confusion the sizes can bring. The lengths used throughout the calculations are not representative of the physical gate sizes, as they depend on voltage applied and the fictitious (bare electron) effective mass used, and not only on the gate size. They represent an effective confinement length to estimate the effects of the electric field from top gates. We did not refer to them as 'radius' or 'diameter' for the same reason. This approach makes abstraction of gate patterns and voltages, while giving a qualitatively good description of the potential landscape near the center of the dot. A possibly more meaningful measure of confinement would be the orbital subband spacing between the QD ground state doublet and the next excited orbital. For instance, a length ~ 40 nm gives a subband spacing of roughly 0.1 meV, which is comparable to what is expected in HH systems, or slightly smaller. Given the larger in-plane effective masses of LHs compared to HHs, and consequently smaller orbital spacings, we believe that our dot sizes are appropriate in this context.

8. On page 4, “QC effective parameters $g_{\perp,\parallel}^c$ ” should be “ $g_{\perp}^c$ ”.

We thank the referee for pointing this out. It is now corrected.

The authors are suggested to thoroughly address the above points before the paper can be recommended for publication.

We thank the Referee for the constructive comments. We believe we have addressed all their questions and the updated manuscript is now improved and ready for publication.

2nd round

The authors have answered my questions properly. I agree its publication in its current form.

III. REPORT OF THE 3RD REFEREE

1st round

In “Fully Tunable Strong Spin-Orbit Interactions in Light-Hole Germanium Quantum Channels”, Dr. Del Vecchio and coauthors present an asymmetric Ge/GeSn quantum well in which light-hole spin qubits can be defined. The vertical asymmetry of the well can be partially compensated by an applied vertical electric field, leading to operating (“idle”) points where the spin-orbit coupling is effectively turned off. This could be advantageous as an idling regime with reduced sensitivity to noise. Away from this point, the system exhibits strong tunability and large manipulability enabled by the strong SOI of light-hole states. The work builds on previous contributions by some of the authors on Ge/GeSn heterostructures and takes a further step by considering elongated quantum dots.

Overall, I find the manuscript interesting and potentially suitable for publication. At the same time, several elements appear closely related to the authors’ prior work on Ge/GeSn heterostructures, and the degree of conceptual advance relative to those papers is not fully clear from the current presentation. Below I provide comments that, if addressed, could strengthen the manuscript:

We would first like to thank the referee for the valuable comments on our work. Regarding the advance of this work relative to our previous works, which was also pointed out by the 2nd referee, we modified a sentence near the end of the introduction to better highlight the new aspects addressed in the present paper.

1. *If I understand correctly, the main mechanism behind the idling point is the restoration of inversion symmetry via a vertical electric field. This seems rather generic, in the sense that any sufficiently asymmetric device could exhibit similar behavior. I therefore wonder about the robustness of this effect. In practice, additional mechanisms can asymmetrically affect linear and cubic spin-orbit terms (alloy disorder, strain, reduced symmetry at interfaces, etc.). Is the idling point robust against such mechanisms? I think this should be discussed.*

We first wish to thank the reviewer for the constructive comments. We agree that additional mechanisms at the interfaces may affect the Rashba SOI in the heterostructure. We are aware of works such as Phys. Rev. B 69, 155321 (2004) that discuss such effects in III-V materials. However, incorporating this into $k \cdot p$ theory requires additional parameters that are not yet known for GeSn alloys. We did not want to introduce additional parameters into our analysis, and focused instead on the breaking of spatial symmetry since this effect is known to occur in group IV alloys and is well documented. We believe that a discussion on additional mechanisms affecting SOI would be more appropriate in a separate, dedicated publication. We added a sentence at the end of the Section “The proposed heterostructure” to justify this point. Regarding the corrections from shear strains, which was also pointed out by the 2nd referee, we estimate that the main effect on the LH will be similar to HHs by renormalizing the magnitude of the linear Rashba coefficient, see for instance Phys. Rev. Lett. 131, 097002 (2023) (Fig. S7 and Eq. S17). However this correction is about $\beta_1 \sim 0.01$ meV nm, which is much smaller than the values we report for LHs ($\beta_1 \sim 1$ meV nm).

2. *The light-hole nature of the ground state appears to depend strongly on the Sn content. The manuscript seems to treat the GeSn alloy within a virtual-crystal approximation, whereas alloy disorder is unavoidable. Beyond corrections to effective masses, linear/cubic spin-orbit couplings, and g-factors, could alloy disorder also affect (or even prevent) the light-hole character of the ground state? If so, this should be mentioned.*

Alloy disorder may change the character of the light hole ground state to some extent, but we do not think it would qualitatively change the energy spectrum, notably since most of the wavefunction is located in pure Ge. Disorder at the Ge/GeSn interface or at the oxide may as well affect spin orbit properties, but these would be hard to accurately predict. For instance the quality of the oxide interface generally depends on the nature of the oxide, on the amount of impurities at the GeSn surface and on the oxide growth technique. We believe that expanding these details would hinder on the generality of the work. A comment on this has been added at the end of the Section “The proposed heterostructure”.

3. *The results are obtained using 6-band kp theory. However, if one includes the split-off band in Ge, should the conduction band also be included? The conduction band is farther away in energy than the split-off band, but they are of the same order of magnitude.*

We agree that the conduction band has a contribution that is about the same order of magnitude as the split-off band. Some of our previous calculations (e.g. Phys. Rev. B 110, 045409 (2024)) indicate that its total contribution is typically around 5%, in comparison to around 10% for the split-off band. However, most of the contributions for Rashba SOI comes from the SO band – for instance we calculate $\beta_1 \approx -1.26$ meV nm using the 6-band model, while with 8-band we get $\beta_1 \approx -1.56$ meV nm. We decided to not include it in our calculations for the sake of analytical simplicity.

4. *More generally, I find tunability through a vertical electric field experimentally delicate. Wouldn't one risk switching to a different occupation state while tuning the field?*

We thank the Referee for raising this point. We believe that tunability through the vertical field is not too problematic once the device is calibrated into a specific charge configuration. This is because, once a specific charge sector is reached, the dot can be decoupled from nearby reservoirs by increasing the barrier gate voltages. In this situation, the charges remain well isolated within the dot and the vertical gate field can be adjusted towards or away from the zero-SOI lines without changing the charge occupancy. Moreover, the large $\partial\Omega/\partial F_z$ near the β -line also ensures that a change in operation mode does not require a large variation of F_z , thus relaxing the constraints on the applied gate voltage.

5. *I think the novelties of the paper, such as the evaluation of elongated dots in Ge/GeSn heterostructures should be emphasized more clearly throughout the manuscript to better distinguish it from prior works.*

We thank the reviewer for pointing this out. We added a comment at the end of the last paragraph just before the Results section to put more emphasis on this.

6. *When $\gamma_{\parallel}$ is introduced after Eq. (1), it is described as the effective mass. I believe it is instead the inverse of the (dimensionless) in-plane effective mass, consistent with what is written for the vertical one near the end of page 2.*

We thank the reviewer for pointing this out. It is now corrected

7. *On page 2, the manuscript states: "The Schrieffer-Wolff transformation employed to derive Eq. (1) provides exact formulas...". What is meant by "exact" here? Don't these parameters receive higher-order corrections?*

We acknowledge the confusion with perturbation theory providing exact results. The SWT procedure gives exact formulas for the effective parameters as long as the order of the transformation matches that of the k -power associated with that parameter. For instance, the effective mass is quadratic in k , and thus requires only a 2nd order SWT to be described exactly. This is because the full $k \cdot p$ Hamiltonian is built in a way such that it is diagonal at $\mathbf{k}_{\parallel} = 0$, while the off-diagonal part (the perturbation) is *at least* linear in k (see equations F1 – we also recall that $\alpha_0/l_B^2 = \mu_B B$ and B is quadratic in k). A 3rd order SWT or higher would not give any contributions to the effective mass since those would provide corrections that are at least cubic in k . Since the effective Hamiltonian presented in the main text is cubic in k , all the effective parameters therein are described exactly by a 3rd order SWT.

2nd round

I thank the authors for their responses to the previous comments and for the revisions made to the manuscript. The authors have addressed several of the points raised in the first round. However, two relevant points regarding the robustness of the light-hole (LH) ground state and the influence of inhomogeneous strain remain insufficiently addressed.

1. *In the first round, I raised the issue of whether alloy disorder could affect the light-hole character of the ground state. The authors respond by stating that they do not expect a qualitative change in the energy spectrum since most of the wavefunction resides in pure Ge. However, my concern was more specific: can alloy disorder in the $\text{Ge}_{1-x}\text{Sn}_x$ buffer layer introduce significant local fluctuations in the biaxial strain? Such fluctuations could, in turn, lead to substantial changes in the HH-LH splitting. Given that the LH ground state relies on a delicate balance between tensile strain and confinement, I believe the authors should clarify whether these strain fluctuations are expected to be small enough or if they could potentially alter the LH character of the state in certain regions of the device. A more quantitative response would be appreciated as the current response is purely qualitative.*

We thank the reviewer for raising this important point regarding the possible impact of alloy disorder on the LH ground state. We agree that the stability of the LH character depends critically on the strain configuration and HH-LH splitting. However, based on both our previous experimental investigations of GeSn alloys and the results presented in this work, we do not find evidence that alloy disorder introduces strain fluctuations large enough to qualitatively modify the LH nature of the ground state.

First, our experimental observations on Sn-rich GeSn alloys did not indicate any measurable strain fluctuations. In an earlier study on GeSn alloys with Sn compositions around 18% (directly relevant to the present manuscript),

we investigated the optical emission using different excitation wavelengths probing different depths of the material [Atomically uniform Sn-rich GeSn semiconductors with 3.0–3.5 μm room-temperature optical emission, Appl. Phys. Lett. 112, 251903 (2018)]. We found that the emission wavelength remained unchanged regardless of the excitation depth. Since the emission energy is highly sensitive to strain, this observation indicates the absence of measurable strain variations along the growth direction (z -direction).

In the same study, atom probe tomography measurements demonstrated that the GeSn alloy exhibits a random alloy distribution without measurable compositional clustering or local compositional fluctuations at the atomic scale. These observations strongly suggest that the alloy disorder does not generate substantial local strain inhomogeneities.

More recently, we investigated GeSn-based heavy-hole quantum wells through magnetotransport measurements [GeSn quantum wells as a platform for spin-resolved hole transport, Communications Materials 5, 71 (2024)]. In that work, the experimental observations related to the spin-orbit interaction strength and subband structure were quantitatively reproduced using $k \cdot p$ calculations assuming a uniform biaxial strain across the quantum well. The excellent agreement between experiment and theory indicates that the strain landscape can be accurately described within the framework of homogeneous biaxial strain, without invoking additional local strain fluctuations associated with alloy disorder.

Taken together, these experimental observations indicate that any local strain fluctuations induced by alloy disorder are sufficiently small compared to the overall engineered tensile strain and confinement energies governing the HH-LH splitting. Therefore, while alloy disorder may introduce minor local perturbations, we do not expect it to alter the qualitative LH character of the ground state or produce spatial regions with a different HH/LH ordering.

We also investigated in the past the epitaxial growth of tensile strain Ge quantum wells on GeSn [A Light-Hole Germanium Quantum Well on Silicon, Advanced Materials 43, 2201192 (2022)]. In that work, we performed reciprocal space mapping using high-resolution X-ray diffraction to evaluate the strain in the grown Ge/GeSn heterostructure. The effect of the coherent growth of Ge on the GeSn layers was clear in the XRD (004) symmetric scan (Fig. S2a in that work). In the absence of the Ge layer the GeSn-related peaks are observed between 64.2° and 65.5° . When the 12.5 nm-thick Ge quantum well is introduced in the stacking, these GeSn-related peaks shift to 64.0° and $\sim 64.25^\circ$. In addition, Pendellosung fringes are observed below 64.0° , which originate from the phase shift between the scattered waves induced by the variation in the out-of-plane lattice parameter across the GeSn/Ge/GeSn stacking. This is an additional indication that the growth of Ge is pseudomorphic with no detectable fluctuations.

2. *Regarding the effects of non-uniform strain raised by Referee 2, the authors argue that corrections to the Rashba interaction are small, citing Phys. Rev. Lett. 131, 097002 (2023). However, the system analyzed in that PRL paper assumes a Ge quantum well where the HH-LH energy separation is relatively large. In the device proposed in this manuscript, the LH and HH states can be much closer in energy. Since the spin-orbit length (and thus the Rashba correction) is inversely proportional to the HH-LH energy difference, it is likely that the corrections from inhomogeneous strain would be significantly enhanced in the authors' proposed structure compared to the one in the cited PRL. Can we really discard these corrections in a regime where the HH-LH gap is small? I am not asking for a detailed simulation including such effects but discussing the impact of this type of correction is relevant.*

We thank the referee for bringing this point, and agree about the relevance of such effects. The LH-HH splitting energy in Ge/SiGe heterostructures is expected to be around 50–75 meV, depending on the amount of residual epitaxial strain in the SiGe barriers and on the well thickness. This range is not much larger than the splitting for the system analysed in our manuscript; we agree that for small Sn contents and thin wells, confinement competes with strain and can make the LH-HH splitting arbitrarily small. This is confirmed by the bold black line in Fig. 1c, for instance. However, this region is avoided by ensuring a Ge layer thickness > 10 nm. In this case the LH-HH splitting is around ~ 30 meV near the operation point $F_z \approx 4.4 \text{ V}/\mu\text{m}$. This is smaller than Ge/SiGe systems but still in the same order of magnitude. Given the argument above and that given to referee II, we believe such effects can be neglected in our manuscript. We added a sentence at the end of the section “planar ground state properties” to better emphasize that we focus on systems with $\Delta E \gtrsim 20$ meV such that the LH ground state remains well separated from the nearest HH level.

3rd round

I thank the authors for their detailed replies to my questions. I recommend the manuscript for publication.

Responses to Reviewers' Comments

We thank the reviewers for their valuable and encouraging reports. In the following, we address their comments and provide additional insights into the key contributions reported in the manuscript.

I. REPORT OF THE 1ST REFEREE

1st round

The paper "Fully Tunable Strong Spin-Orbit Interactions in Light Hole Germanium Quantum Channels" by Del Vecchio et al. proposes and theoretically analyzes a novel Ge heterostructure that allows for switchable spin-orbit interactions via gate tuning. If successful, this would represent a significant advance in the field of semiconductor spin qubits, addressing a key challenge in achieving both fast qubit manipulation and long coherence times. The authors present a solid physical model and detailed calculations supporting their claims, which form the strongest part of the manuscript. However, the dense technical content, compromising space for in-depth discussion of experimental feasibility, limits the possible impact of the work. To support my recommendation for major revisions, I highlight the following key concerns:

1. *The paper is difficult to follow for potential readers; for instance, the origin of Hamiltonian (1) is not clearly given. The authors should provide the necessary references or derive it in the Supplementary Information (SI) to ensure reproducibility.*

We would first like to thank the referee for the constructive comments. The effective Hamiltonian (1) is derived from the full 6-band $k \cdot p$ Hamiltonian presented in the supplementary information. For the sake of clarity, we reorganized a large portion of the supplemental material and added more information in the "perturbation theory" section.

2. *The main outcomes of the paper are the parameters leading to the g - and β -lines presented in Figs. 2 and 3. While the motivation for on/off switching is given in the introduction, the implication of the g -line for leakage prevention is mentioned only briefly in the conclusion. The authors should expand on the practical implications of these findings (leakage mechanisms in particular) for qubit operation and qubit coherence.*

We thank the referee for bringing up this point. The g -lines effectively correspond to a gapless qubit, similarly to the gapless spin qubit studied in the recent work Phys. Rev. Lett. 135, 197001 (2025). Such qubits present many advantages for integration notably the lack of a global clock and full electric baseband control, and how they eliminate leakage channels through the large energy gap to the next excited states. Since these are discussed at length in that reference, we believe we do not need to expand much further on this in our work. We nevertheless added a comment just before the conclusion to elaborate a bit more on this point.

3. *In particular, while the robustness of the β -line is tested against θ , l_x , and l_y , sensitivity to electric field fluctuations is not discussed. In fact, the results presented in Fig. 3 suggest that the β -line is very steep in the F_z direction, which would make it highly sensitive to charge noise. The authors should comment on this point. Note that this implies an "off-state" with zero spin-orbit interaction is not necessarily a low-decoherence state if the noise sensitivity is high.*

We agree that the Rabi frequency graphs indicate a steep derivative with respect to the gate field. However we emphasize that it is not necessarily an indication of a higher sensitivity to charge noise. The dephasing rate of the qubit is mostly sensitive to longitudinal noise in the qubit subspace, or components of the noise that change the Larmor frequency. Regarding this, Fig. 2c and 2e suggest that close to the zero-SOI operation point ($F_z \approx 4.4 \text{ V}/\mu\text{m}$), the in-plane and out-of-plane components of the g -factor have very small slopes with respect to F_z and thus we do not expect a significant increase in charge noise sensitivity. In fact, the longitudinal component of the z operator, $|\langle 1|z|1\rangle - \langle 0|z|0\rangle|/2$, shows a minimum with respect to F_z near the zero-SOI operation point (see Fig. 1 below. We added a sentence in the main text just before the conclusion to address this point.)

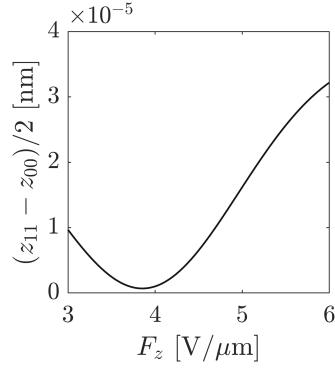


FIG. 1: Longitudinal component of the z operator. $l_x = l_y = 40$ nm, $\mathbf{B} = (0.05 \text{ T})\mathbf{e}_y$.

4. *There appears to be an inconsistency between Eq. (3) and the SI regarding the sign of $\gamma_{\parallel}^p$ (The authors should check Eq. 3 vs Eq. D11, for instance).*

We agree there is a sign inconsistency between equation D11 and equation 3. This is because we decided to use in the main text the usual convention of positive hole effective mass for convenience. On the other hand, in the supplemental material, the theory describes the hole effective mass within the electron picture and is therefore negative. $-\gamma_{\parallel} > 0$ and the square root in equation D11 is real. We added a comment at the beginning of the supplemental material to clarify this point.

5. *The abstract emphasizes: "The large in-plane g -factor also facilitates the integration of superconductors." Yet, after reading the manuscript, it is still not clear to me how this integration would be facilitated. In fact, a similar statement is repeated without reference or explanation at the end of the results section. The authors should clarify the physical mechanism behind this statement.*

We thank the referee for pointing this out. A large in-plane g -factor facilitates the integration of superconductors since they generally are substantially more resilient against in-plane magnetic fields compared to out-of-plane magnetic fields. For instance, Fig. 3 and Fig. 4a in Phys. Rev. Appl. 5, 044004 (2016) provide an indication that the quality factor of nanowire resonators in out-of-plane magnetic fields decreases much faster with B than in in-plane fields. In the updated manuscript, we included this reference to the main text.

6. *Finally, the authors assume a particular content of Sn in the GeSn buffer layer (16%) to achieve the light-hole ground state. The authors should put this number in the context of current experimental capabilities and comment on the feasibility of growing such structures with low defect densities.*

We thank the reviewer for bringing this issue, which is important from a practical standpoint. The growth of GeSn can be challenging given the metastable nature of this alloy and its large lattice parameter relative to that of silicon. However, by using nonequilibrium epitaxy growth processes it is possible to grow high-quality GeSn layers enabling their use in a range of photonic and optoelectronic applications. For an overview, see for Appl. Phys. Lett. 118, 110502 (2021). As described in that paper, device-quality GeSn heterostructures can be grown on silicon wafers by chemical vapor deposition using GeH_4 and SnCl_4 as precursors. Germanium interlayer is typically grown first on silicon to manage the large lattice mismatch. Following this process, our group grows routinely GeSn materials. We have demonstrated in the past the growth of GeSn at a concentration of 18% (see Appl. Phys. Lett. 112, 251903 (2018)). Thus, the proposed heterostructure is achievable using current epitaxy processes. We added a comment regarding this in the main text at the beginning of the Section "Light hole ground state requirements".

7. *Suggestion: To improve the readability and impact of the manuscript, the authors could consider moving some of the technical details to the SI and expand on the physical implications of their findings in the main text.*

We thank the referee for the suggestion. We added a few lines after equation 1 in the main text on the physical origins of the Rashba parameters appearing in the LH effective Hamiltonian, which may help differentiate LH ground states from conventional HH ground states.

Overall, the manuscript presents a novel pathway to achieve tunable spin-orbit interactions in Ge qubits. However, due to the concerns outlined above, I am unable to recommend publication in Communications Physics in the current form. I look forward to reviewing a revised manuscript that addresses these critical issues and incorporates the suggestions to improve clarity and impact.

We thank the Referee for their constructive feedback. We believe we have addressed all their comments and the updated manuscript is now improved and ready for publication.

2nd round

I thank the authors for their detailed responses and revision of the manuscript. I am satisfied with the explanations provided, and I think the revised manuscript has improved, in particular with the clarification of the role of longitudinal noise sensitivity at the zero-SOI operation point and the discussion of the feasibility of growing GeSn heterostructures. By reading the revised manuscript and the discussion with all the referees I am also more convinced of the novelty and potential impact of the work, which is now better distinguished from the authors' previous contributions. I therefore recommend publication in Communications Physics.

II. REPORT OF THE 2ND REFEREE

1st round

Patrick et al. present a theoretical study of tunable strong spin-orbit interaction (SOI) in light-hole (LH) Ge qubits using model Hamiltonian analysis. They propose a Ge heterostructure consisting of a GeSn buffer layer and a top barrier layer to introduce tensile biaxial strains and the resulting LH as the ground state, which exhibits k-linear SOI tenability that can be suppressed to zero in 2D planar, 1D channel, and 0D quantum dot geometries. They further report the tunable out-of-plane g-factor for 1D and 0D devices. Additionally, the Rabi frequency for the LH Ge qubit is estimated. The study is systematic and is related to a hot topic. However, partially claimed novel results of this manuscript have already been discussed in previous works by the same authors, such as strong SOI in LHs and the use of LH to carry a qubit (Refs. 56 and 57).

We thank the Referee for their assessment and for finding our work systematic and potentially impactful as related to an hot topic. In the updated version of the manuscript near the end of the Introduction, we better emphasize the novelty of this work compared to previous literature by noting the focus on squeezed dots within heterostructures with built-in vertical asymmetry.

1. *On the other hand, the introduction of Sn element into Si-based spin qubit is incompatible with current CMOS technology, sacrificing the most important advantage over other qubit platforms. In addition, the validity of the assumption in the model needs to be further verified.*

We respectfully but strongly disagree with this statement. The proposed GeSn-based heterostructure is compatible with silicon and silicon processing. Indeed, GeSn is a group IV isovalent alloy regarded as CMOS-compatible because it can be grown on silicon substrates using processes (e.g., CVD or MBE) that are compatible with standard silicon fabrication workflows and thermal budgets. Its integration leverages the same material platform as silicon and germanium. Importantly, qubits based on GeSn can benefit from nuclear spin-free isotopes and rely on the same underlying material system and fabrication approaches as other silicon-based qubits. This ensures that advanced quantum functionalities can be developed without breaking compatibility with scalable, industry-standard semiconductor manufacturing. For these reasons, the current manuscript focuses on this system.

2. *The claimed novelty of this work relies on the tunability of the SOI, which in turn depends critically on assuming an asymmetric confinement profile. This assumption is weakly justified. The proposed device is highly likely to introduce non-uniform strain instead of a uniform one. On the other hand, existing experimental and theoretical studies on Ge quantum wells are, to my knowledge, based on symmetric confinement profiles, and no clear experimental evidence for the proposed asymmetric confinement is provided. More importantly, the specific proposals suggested to achieve asymmetric confinement are not experimentally realistic and lack sufficient elaboration. A very thin Si capping layer would lead to significant gate leakage due to tunneling. Direct contact between an oxide layer and Ge is generally unfavorable in Ge-based heterostructures because the oxide of Ge is unstable and the Ge/oxide interface is known to host orders-of-magnitude higher defects than Si/SiO₂. In addition, placing the qubit closer to the gate would substantially enhance charge noise arising from gate-oxide interface defects, which is widely regarded as the dominant noise source in Ge qubits.*

We thank the referee for bringing up this detail and agree that Ge oxide is poor. However, high-quality oxides can be deposited by ALD, as shown in recent work: T. Aso et al. Materials Science in Semiconductor Processing 190, 109372 (2025). We stress that the basic requirement is a top barrier such that its band offset ΔE is large enough to prevent the light hole from leaking to the surface at strong vertical gate field, i.e. $\Delta E \gtrsim eF_z d$, where d is the thickness of the barrier. We put an emphasis on *thin* upper barriers to alleviate the requirement on ΔE . On hindsight, we agree that "a few ångströms" thick is far too restrictive, and could increase charge noise. Assuming the device operates at gate fields lower than $\sim 10 \text{ V}/\mu\text{m}$, a $d \lesssim 10 \text{ nm}$ upper barrier that prevents leakage of the hole to the surface would require a band offset $\gtrsim 100 \text{ meV}$ only, which is still satisfied using Si, or even a Si-rich SiGe alloy. We modified the main text to put more emphasis on the *no leaking* requirement, rather than on *very thin* barriers. We also added the above article in the list of references. Effects of non-uniform strain may also be present at the quantum well layer, but will fall-off with distance for thicker barriers. Nevertheless, its effects on the LH will likely be similar to HHs by rescaling the strength of the linear Rashba term. According to Phys. Rev. Lett. 131, 097002 (2023) (see Fig. S7 and Eq. S17 therein), shear strains introduce spin-orbit lengths $\ell_{\text{so}}^{-1} \sim 0.32 \mu\text{m}^{-1}$ in the Ge QW underneath a 50 nm top barrier. This translates to $\beta_1 \sim \hbar^2/(m^* \ell_{\text{so}}) \sim 0.01 \text{ meV nm}$, which is much smaller than the $\beta_1 \sim 1 \text{ meV nm}$ we report for LHs. Therefore, we believe the presence of shear strains would not have qualitative effects on our results, even considering thinner top barriers.

3. The authors use a Sn content of 10% in plotting the energy confinement profile in Fig.1(b), but substitute it with 16% in the following content. Why not use a consistent value and write it clearly throughout the manuscript rather than using x in $\text{Ge}_{1-x}\text{Sn}_x$?

We decided to plot the envelope functions at 10% Sn content in Fig. 1 only for better visuals, i.e. one could easily see the three types of confined levels (LHs, HHs at the Ge/GeSn interface and those at the surface barrier) with the best proportions. We added a sentence in the caption of Fig. 1 in the main text to clarify this.

4. In Fig. 3(b, c), the SOI-off points show large $\partial\Omega/\partial F_z$, indicating strong sensitivity to charge noise. Given the magnified charge noise in the proposed device, idling the qubit at $\Omega = 0$ under these conditions may negate the intended benefit of turning off the SOI. A quantitative or qualitative discussion of charge noise and its impact on qubit performance at the SOI-off point is required.

We agree that the Rabi frequency plots indicate a steep incline with respect to the gate field. However it is not necessarily an indication of a higher sensitivity to charge noise. The dephasing rate of the qubit is mostly sensitive to longitudinal noise in the qubit subspace, or components of the noise that change the Larmor frequency. Regarding this, Fig. 2c and 2e suggest that close to the zero-SOI operation point ($F_z \approx 4.4 \text{ V}/\mu\text{m}$), the in-plane and out-of-plane components of the g -factor have very small inclines with respect to F_z and thus we do not expect a significant increase in charge noise sensitivity. In fact, the longitudinal component of the z operator, $|\langle 1|z|1\rangle - \langle 0|z|0\rangle|/2$, shows a minimum with respect to F_z near the zero-SOI operation point (see the figure just below). We added a sentence in the main text in the paragraph just before the conclusion to clarify this point.

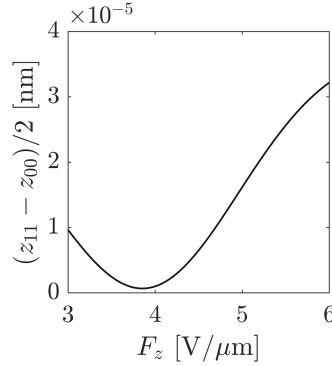


FIG. 2: Longitudinal component of the z operator. $l_x = l_y = 40 \text{ nm}$, $\mathbf{B} = (0.05 \text{ T})\mathbf{e}_y$.

5. The magnetic field is specified as $B_0 = 0.05 \text{ T}$. In Fig. 3, the field appears normalized, indicating that the results are independent of magnetic field magnitude within a reasonable range. The reason for choosing this specific value of magnetic field should be clarified.

The value of 0.05 T was chosen within a range of small magnetic fields such that the Rabi frequency vs B remains in a linear regime. Then dividing the Rabi frequency by $\mu_B B/\hbar$ allowed us to report values that are not specific to a particular magnetic field strength. It also ensures the lowest quantum dot doublet corresponds to the lowest spin-split orbital. For instance, we choose the characteristic lengths $l_{x,y}$ such that the orbital spacing $\Delta E \sim 0.1 \text{ meV}$. Then assuming an average g -factor $g \sim 5$, we find $B \ll \Delta E/(g\mu_B) \sim 0.34 \text{ T}$.

6. How is the specific value of Rabi frequency for a realistic qubit device? How does it compare with the ultrafast manipulation speed of 1.2 GHz observed in Ge NW?

An estimation of specific Rabi frequencies is generally tricky to make, since it is proportional to the electrical driving strength F_{ac} at the qubit location, which is hard to estimate theoretically. However around $F_z = 4 \text{ V}/\mu\text{m}$, the spin-dipole length $|\langle 0|y|1\rangle| \sim 1 \text{ nm}$ and the Larmor frequency is 5.5 GHz for $\mathbf{B} = (0.05 \text{ T})\mathbf{e}_y$. Then multiplying $|\langle 0|y|1\rangle|$ by $eF_{ac}/\hbar$ with $F_{ac} = 0.001 \text{ V}/\mu\text{m}$ gives $\Omega \approx 1.5 \text{ GHz}$. Similar estimates can be made from Fig. 3 in the main text, with $\omega_B \approx 4.40 \text{ GHz}$ at $B = 0.05 \text{ T}$. These numbers are not too far from the ultra-fast Rabi driving reported in Ge/Si core/shell nanowires (Nat. Nanotechnol. 16, 308-312 (2021)), notably with driving frequencies approaching the same order of magnitude as the Larmor frequency. This gives us confidence that our estimations are realistic.

7. The used sizes of quantum dots, such as l_x, l_y , are smaller than a planar Ge hole qubit (100 nm), which is unfavorable for device fabrication. The authors are suggested to choose sizes within experimental availability.

We acknowledge the confusion the sizes can bring. The lengths used throughout the calculations are not representative of the physical gate sizes, as they depend on voltage applied and the fictitious (bare electron) effective mass used, and not only on the gate size. They represent an effective confinement length to estimate the effects of the electric field from top gates. We did not refer to them as 'radius' or 'diameter' for the same reason. This approach makes abstraction of gate patterns and voltages, while giving a qualitatively good description of the potential landscape near the center of the dot. A possibly more meaningful measure of confinement would be the orbital subband spacing between the QD ground state doublet and the next excited orbital. For instance, a length ~ 40 nm gives a subband spacing of roughly 0.1 meV, which is comparable to what is expected in HH systems, or slightly smaller. Given the larger in-plane effective masses of LHs compared to HHs, and consequently smaller orbital spacings, we believe that our dot sizes are appropriate in this context.

8. On page 4, “QC effective parameters $g_{\perp,\parallel}^c$ ” should be “ $g_{\perp}^c$ ”.

We thank the referee for pointing this out. It is now corrected.

The authors are suggested to thoroughly address the above points before the paper can be recommended for publication.

We thank the Referee for the constructive comments. We believe we have addressed all their questions and the updated manuscript is now improved and ready for publication.

2nd round

The authors have answered my questions properly. I agree its publication in its current form.

III. REPORT OF THE 3RD REFEREE

1st round

In “Fully Tunable Strong Spin-Orbit Interactions in Light-Hole Germanium Quantum Channels”, Dr. Del Vecchio and coauthors present an asymmetric Ge/GeSn quantum well in which light-hole spin qubits can be defined. The vertical asymmetry of the well can be partially compensated by an applied vertical electric field, leading to operating (“idle”) points where the spin-orbit coupling is effectively turned off. This could be advantageous as an idling regime with reduced sensitivity to noise. Away from this point, the system exhibits strong tunability and large manipulability enabled by the strong SOI of light-hole states. The work builds on previous contributions by some of the authors on Ge/GeSn heterostructures and takes a further step by considering elongated quantum dots.

Overall, I find the manuscript interesting and potentially suitable for publication. At the same time, several elements appear closely related to the authors’ prior work on Ge/GeSn heterostructures, and the degree of conceptual advance relative to those papers is not fully clear from the current presentation. Below I provide comments that, if addressed, could strengthen the manuscript:

We would first like to thank the referee for the valuable comments on our work. Regarding the advance of this work relative to our previous works, which was also pointed out by the 2nd referee, we modified a sentence near the end of the introduction to better highlight the new aspects addressed in the present paper.

1. *If I understand correctly, the main mechanism behind the idling point is the restoration of inversion symmetry via a vertical electric field. This seems rather generic, in the sense that any sufficiently asymmetric device could exhibit similar behavior. I therefore wonder about the robustness of this effect. In practice, additional mechanisms can asymmetrically affect linear and cubic spin-orbit terms (alloy disorder, strain, reduced symmetry at interfaces, etc.). Is the idling point robust against such mechanisms? I think this should be discussed.*

We first wish to thank the reviewer for the constructive comments. We agree that additional mechanisms at the interfaces may affect the Rashba SOI in the heterostructure. We are aware of works such as Phys. Rev. B 69, 155321 (2004) that discuss such effects in III-V materials. However, incorporating this into $k \cdot p$ theory requires additional parameters that are not yet known for GeSn alloys. We did not want to introduce additional parameters into our analysis, and focused instead on the breaking of spatial symmetry since this effect is known to occur in group IV alloys and is well documented. We believe that a discussion on additional mechanisms affecting SOI would be more appropriate in a separate, dedicated publication. We added a sentence at the end of the Section “The proposed heterostructure” to justify this point. Regarding the corrections from shear strains, which was also pointed out by the 2nd referee, we estimate that the main effect on the LH will be similar to HHs by renormalizing the magnitude of the linear Rashba coefficient, see for instance Phys. Rev. Lett. 131, 097002 (2023) (Fig. S7 and Eq. S17). However this correction is about $\beta_1 \sim 0.01$ meV nm, which is much smaller than the values we report for LHs ($\beta_1 \sim 1$ meV nm).

2. *The light-hole nature of the ground state appears to depend strongly on the Sn content. The manuscript seems to treat the GeSn alloy within a virtual-crystal approximation, whereas alloy disorder is unavoidable. Beyond corrections to effective masses, linear/cubic spin-orbit couplings, and g-factors, could alloy disorder also affect (or even prevent) the light-hole character of the ground state? If so, this should be mentioned.*

Alloy disorder may change the character of the light hole ground state to some extent, but we do not think it would qualitatively change the energy spectrum, notably since most of the wavefunction is located in pure Ge. Disorder at the Ge/GeSn interface or at the oxide may as well affect spin orbit properties, but these would be hard to accurately predict. For instance the quality of the oxide interface generally depends on the nature of the oxide, on the amount of impurities at the GeSn surface and on the oxide growth technique. We believe that expanding these details would hinder on the generality of the work. A comment on this has been added at the end of the Section “The proposed heterostructure”.

3. *The results are obtained using 6-band kp theory. However, if one includes the split-off band in Ge, should the conduction band also be included? The conduction band is farther away in energy than the split-off band, but they are of the same order of magnitude.*

We agree that the conduction band has a contribution that is about the same order of magnitude as the split-off band. Some of our previous calculations (e.g. Phys. Rev. B 110, 045409 (2024)) indicate that its total contribution is typically around 5%, in comparison to around 10% for the split-off band. However, most of the contributions for Rashba SOI comes from the SO band – for instance we calculate $\beta_1 \approx -1.26$ meV nm using the 6-band model, while with 8-band we get $\beta_1 \approx -1.56$ meV nm. We decided to not include it in our calculations for the sake of analytical simplicity.

4. *More generally, I find tunability through a vertical electric field experimentally delicate. Wouldn't one risk switching to a different occupation state while tuning the field?*

We thank the Referee for raising this point. We believe that tunability through the vertical field is not too problematic once the device is calibrated into a specific charge configuration. This is because, once a specific charge sector is reached, the dot can be decoupled from nearby reservoirs by increasing the barrier gate voltages. In this situation, the charges remain well isolated within the dot and the vertical gate field can be adjusted towards or away from the zero-SOI lines without changing the charge occupancy. Moreover, the large $\partial\Omega/\partial F_z$ near the β -line also ensures that a change in operation mode does not require a large variation of F_z , thus relaxing the constraints on the applied gate voltage.

5. *I think the novelties of the paper, such as the evaluation of elongated dots in Ge/GeSn heterostructures should be emphasized more clearly throughout the manuscript to better distinguish it from prior works.*

We thank the reviewer for pointing this out. We added a comment at the end of the last paragraph just before the Results section to put more emphasis on this.

6. *When $\gamma_{\parallel}$ is introduced after Eq. (1), it is described as the effective mass. I believe it is instead the inverse of the (dimensionless) in-plane effective mass, consistent with what is written for the vertical one near the end of page 2.*

We thank the reviewer for pointing this out. It is now corrected

7. *On page 2, the manuscript states: "The Schrieffer-Wolff transformation employed to derive Eq. (1) provides exact formulas...". What is meant by "exact" here? Don't these parameters receive higher-order corrections?*

We acknowledge the confusion with perturbation theory providing exact results. The SWT procedure gives exact formulas for the effective parameters as long as the order of the transformation matches that of the k -power associated with that parameter. For instance, the effective mass is quadratic in k , and thus requires only a 2nd order SWT to be described exactly. This is because the full $k \cdot p$ Hamiltonian is built in a way such that it is diagonal at $\mathbf{k}_{\parallel} = 0$, while the off-diagonal part (the perturbation) is *at least* linear in k (see equations F1 – we also recall that $\alpha_0/l_B^2 = \mu_B B$ and B is quadratic in k). A 3rd order SWT or higher would not give any contributions to the effective mass since those would provide corrections that are at least cubic in k . Since the effective Hamiltonian presented in the main text is cubic in k , all the effective parameters therein are described exactly by a 3rd order SWT.

2nd round

I thank the authors for their responses to the previous comments and for the revisions made to the manuscript. The authors have addressed several of the points raised in the first round. However, two relevant points regarding the robustness of the light-hole (LH) ground state and the influence of inhomogeneous strain remain insufficiently addressed.

1. *In the first round, I raised the issue of whether alloy disorder could affect the light-hole character of the ground state. The authors respond by stating that they do not expect a qualitative change in the energy spectrum since most of the wavefunction resides in pure Ge. However, my concern was more specific: can alloy disorder in the $\text{Ge}_{1-x}\text{Sn}_x$ buffer layer introduce significant local fluctuations in the biaxial strain? Such fluctuations could, in turn, lead to substantial changes in the HH-LH splitting. Given that the LH ground state relies on a delicate balance between tensile strain and confinement, I believe the authors should clarify whether these strain fluctuations are expected to be small enough or if they could potentially alter the LH character of the state in certain regions of the device. A more quantitative response would be appreciated as the current response is purely qualitative.*

We thank the reviewer for raising this important point regarding the possible impact of alloy disorder on the LH ground state. We agree that the stability of the LH character depends critically on the strain configuration and HH-LH splitting. However, based on both our previous experimental investigations of GeSn alloys and the results presented in this work, we do not find evidence that alloy disorder introduces strain fluctuations large enough to qualitatively modify the LH nature of the ground state.

First, our experimental observations on Sn-rich GeSn alloys did not indicate any measurable strain fluctuations. In an earlier study on GeSn alloys with Sn compositions around 18% (directly relevant to the present manuscript),

we investigated the optical emission using different excitation wavelengths probing different depths of the material [Atomically uniform Sn-rich GeSn semiconductors with 3.0–3.5 μm room-temperature optical emission, Appl. Phys. Lett. 112, 251903 (2018)]. We found that the emission wavelength remained unchanged regardless of the excitation depth. Since the emission energy is highly sensitive to strain, this observation indicates the absence of measurable strain variations along the growth direction (z -direction).

In the same study, atom probe tomography measurements demonstrated that the GeSn alloy exhibits a random alloy distribution without measurable compositional clustering or local compositional fluctuations at the atomic scale. These observations strongly suggest that the alloy disorder does not generate substantial local strain inhomogeneities.

More recently, we investigated GeSn-based heavy-hole quantum wells through magnetotransport measurements [GeSn quantum wells as a platform for spin-resolved hole transport, Communications Materials 5, 71 (2024)]. In that work, the experimental observations related to the spin-orbit interaction strength and subband structure were quantitatively reproduced using $k \cdot p$ calculations assuming a uniform biaxial strain across the quantum well. The excellent agreement between experiment and theory indicates that the strain landscape can be accurately described within the framework of homogeneous biaxial strain, without invoking additional local strain fluctuations associated with alloy disorder.

Taken together, these experimental observations indicate that any local strain fluctuations induced by alloy disorder are sufficiently small compared to the overall engineered tensile strain and confinement energies governing the HH-LH splitting. Therefore, while alloy disorder may introduce minor local perturbations, we do not expect it to alter the qualitative LH character of the ground state or produce spatial regions with a different HH/LH ordering.

We also investigated in the past the epitaxial growth of tensile strain Ge quantum wells on GeSn [A Light-Hole Germanium Quantum Well on Silicon, Advanced Materials 43, 2201192 (2022)]. In that work, we performed reciprocal space mapping using high-resolution X-ray diffraction to evaluate the strain in the grown Ge/GeSn heterostructure. The effect of the coherent growth of Ge on the GeSn layers was clear in the XRD (004) symmetric scan (Fig. S2a in that work). In the absence of the Ge layer the GeSn-related peaks are observed between 64.2° and 65.5° . When the 12.5 nm-thick Ge quantum well is introduced in the stacking, these GeSn-related peaks shift to 64.0° and $\sim 64.25^\circ$. In addition, Pendellosung fringes are observed below 64.0° , which originate from the phase shift between the scattered waves induced by the variation in the out-of-plane lattice parameter across the GeSn/Ge/GeSn stacking. This is an additional indication that the growth of Ge is pseudomorphic with no detectable fluctuations.

2. *Regarding the effects of non-uniform strain raised by Referee 2, the authors argue that corrections to the Rashba interaction are small, citing Phys. Rev. Lett. 131, 097002 (2023). However, the system analyzed in that PRL paper assumes a Ge quantum well where the HH-LH energy separation is relatively large. In the device proposed in this manuscript, the LH and HH states can be much closer in energy. Since the spin-orbit length (and thus the Rashba correction) is inversely proportional to the HH-LH energy difference, it is likely that the corrections from inhomogeneous strain would be significantly enhanced in the authors' proposed structure compared to the one in the cited PRL. Can we really discard these corrections in a regime where the HH-LH gap is small? I am not asking for a detailed simulation including such effects but discussing the impact of this type of correction is relevant.*

We thank the referee for bringing this point, and agree about the relevance of such effects. The LH-HH splitting energy in Ge/SiGe heterostructures is expected to be around 50–75 meV, depending on the amount of residual epitaxial strain in the SiGe barriers and on the well thickness. This range is not much larger than the splitting for the system analysed in our manuscript; we agree that for small Sn contents and thin wells, confinement competes with strain and can make the LH-HH splitting arbitrarily small. This is confirmed by the bold black line in Fig. 1c, for instance. However, this region is avoided by ensuring a Ge layer thickness > 10 nm. In this case the LH-HH splitting is around ~ 30 meV near the operation point $F_z \approx 4.4 \text{ V}/\mu\text{m}$. This is smaller than Ge/SiGe systems but still in the same order of magnitude. Given the argument above and that given to referee II, we believe such effects can be neglected in our manuscript. We added a sentence at the end of the section “planar ground state properties” to better emphasize that we focus on systems with $\Delta E \gtrsim 20$ meV such that the LH ground state remains well separated from the nearest HH level.

3rd round

I thank the authors for their detailed replies to my questions. I recommend the manuscript for publication.