Anonymous Referee #1
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This paper focuses on the core challenges in the design and metrological assessment of ultra-low-noise inertial sensors for geoscientific applications, proposing a causal and uncertainty-aware digital twin framework.However, there is room for optimization in experimental validation, parameter sensitivity analysis, and the expression of certain technical details. It can meet the publication requirements after targeted revisions.

I thank the reviewer for the positive overall assessment of the manuscript and for the constructive suggestions provided.
In the revised version, I have addressed all the points raised through targeted modifications aimed at improving clarity, rigor, and positioning of the proposed framework. In particular:

- The scope and limitations of the mechanical modeling have been clarified by explicitly defining the frequency range of validity and boundary conditions of the single-degree-of-freedom approximation.
- The role of the framework as a design-stage tool has been better contextualised, and its relationship with experimental validation has been explicitly discussed, including possible future integration with real sensor data.
- Additional recent literature (2024–2025) has been incorporated to strengthen the connection with current developments in digital twins and ultra-low-noise sensing technologies.
- The derivation of key equations has been made more transparent by including intermediate steps and appropriate references to standard formulations.
- A structured comparison with conventional methodologies has been introduced, both analytically and through a dedicated schematic representation (Fig. 9), to better highlight the methodological contribution of the proposed approach.

These revisions collectively improve the completeness and clarity of the manuscript, and I believe they fully address the reviewer's concerns.

1.The mechanical subsystem in the paper is modeled as a single-degree-of-freedom inertial plant, which can capture the dominant dynamics but fails to clearly state the applicable frequency range and boundary conditions of this simplified model.

I agree that explicitly defining the domain of validity of the single-degree-of-freedom (SDOF) approximation improves the physical clarity and applicability of the framework.
In the revised manuscript, I have clarified both the applicable frequency range and the implicit boundary conditions underlying the SDOF representation. Specifically, I now state that the model is valid within the frequency band where the fundamental mode dominates the mechanical response, i.e. sufficiently below the first higher-order structural resonance and above regimes where rigid-body or environmental coupling effects (e.g., tilt or foundation compliance) become significant.
I have also explicitly specified the boundary conditions assumed in the model, including rigid coupling between the sensor frame and the ground, negligible rotational degrees of freedom, and operation within the linear regime around a stable equilibrium configuration.
These clarifications have been added in Section 3 to ensure that the limitations and applicability of the mechanical model are clearly defined.

2.The paper only verifies the framework's effectiveness through simulation analysis, lacking experimental data support from actual sensor prototypes. It is suggested to supplement experimental validation based on real inertial sensors.

I fully agree that experimental validation is a crucial aspect in the development and assessment of inertial sensors.
The framework proposed in this study is intentionally conceived as a design-stage, physics-based digital twin aimed at evaluating achievable performance, identifying limiting mechanisms, and guiding sensor architecture choices prior to hardware realisation. For this reason, the manuscript focuses on internally consistent, causality-constrained modeling and uncertainty-aware analysis rather than on the validation of a specific prototype.

To clarify this point, I have strengthened the discussion in the revised manuscript by explicitly stating the intended scope of the framework and its role as a pre-implementation tool. I have also added a dedicated paragraph in the Discussion section highlighting the absence of experimental validation as a limitation and outlining how the proposed digital twin could be integrated with real sensor data for future validation and calibration.

In addition, I have reinforced the connection with experimentally validated sensor technologies by expanding the references to state-of-the-art inertial sensors and by clarifying that the parameter ranges adopted in the simulations are consistent with realistic instrument designs reported in the literature.

I believe that these clarifications better position the contribution within the appropriate methodological context, while acknowledging the importance of future experimental validation.

3.The references lack sufficient citations of relevant research in the past 2 years (2024-2025), especially the latest applications of digital twins in the field of inertial sensors and advances in ultra-low-noise readout technology. It is suggested to supplement high-impact literature from the past 2 years to reflect the cutting-edge and timeliness of the research.

I agree that strengthening the connection with the most recent literature improves the timeliness and positioning of the manuscript.

In the revised version, I have expanded the reference list to include additional recent studies (2024–2025) covering both digital twin methodologies in metrology and recent advances in ultra-low-noise inertial sensing and readout technologies. These references have been integrated directly into the Introduction and relevant technical sections to better contextualise the proposed framework within current research trends.

In particular, I have reinforced the discussion of recent developments in digital twins for metrology and uncertainty-aware modeling, as well as recent experimental and technological advances in interferometric and low-noise readout systems.

These additions ensure that the manuscript more clearly reflects the current state of the art and highlights the relevance of the proposed framework in the context of ongoing developments.

4.Excessive steps are omitted in the derivation of some formulas. For example, the conversion steps from the equation of motion (1) to the frequency-domain expression (2) and the derivation logic of the self-noise power spectral density formula (3) are not elaborated. It is suggested to supplement the core derivation steps of key formulas, or cite relevant literature to illustrate the derivation basis, enhancing the rigor of the theoretical part.

I agree that providing additional intermediate steps and references improves the transparency and rigor of the theoretical framework.

In the revised manuscript, I have supplemented the derivation of the key equations by explicitly outlining the main transformation steps from the time-domain equation of motion to its frequency-domain representation, and by clarifying the assumptions underlying the self-noise power spectral density formulation.

In addition, I have included appropriate references to standard formulations in linear systems theory and noise propagation in inertial sensors to support the derivation of the self-noise expression.

These additions improve the readability and traceability of the theoretical development while preserving the compact structure of the manuscript.

5.The performance comparison section only compares with theoretical limits and lacks quantitative comparison with existing similar sensor design methods (such as traditional noise budgeting and simplified digital twin models).

I agree that explicitly positioning the proposed framework with respect to existing design methodologies strengthens the contribution.

In the revised manuscript, I have expanded the Discussion to include a structured comparison between the proposed causal and uncertainty-aware digital-twin framework and conventional approaches, including traditional noise budgeting methods and simplified (non-causal or subsystem-based) digital twin models.

This comparison is formulated in terms of the ability to (i) capture system-level coupling effects, (ii) enforce physical causality and realisability, (iii) propagate uncertainty consistently across the full measurement chain, and (iv) provide actionable performance metrics such as crossover frequencies and near-plateau bandwidth.

To further clarify these differences, I have introduced a dedicated schematic comparison (Fig. 9), which summarises the key methodological distinctions and their implications for performance evaluation and design optimisation.

While the comparison remains analytical rather than based on a specific benchmark dataset, it provides a quantitative and conceptually grounded assessment of the advantages and limitations of the proposed approach relative to existing methods.

These additions clarify the methodological contribution of the framework and its practical relevance for sensor design.

Anonymous Referee #2
Citation: https://doi.org/10.5194/egusphere-2026-628-RC2

The manuscript “Causal and uncertainty-aware digital-twin framework for ultra–low-noise geoscientific inertial sensors” by Antonino D’Alessandro is well written and easy to follow. The digital twin framework developed in the manuscript appears sound and useful to me. The assumptions and potential shortcomings in the numerical models are clearly stated, e.g., no nonlinear effects or noise correlations are included.
I am not an expert on inertial sensors and cannot therefore comment adequately on the novelty of the approach presented here but trust the other reviewers to judge this aspect. I have no objections to the manuscript being accepted for publication once a few details are fixed.

I thank the reviewer for the positive assessment of the manuscript and for recognising the clarity of the presentation and the soundness of the proposed framework. I also appreciate the acknowledgment of the explicit discussion of assumptions and limitations. All specific comments aimed at improving clarity and presentation have been carefully addressed in the revised manuscript.

Moderate issue:
Line 154: Isn’t this a reference to figure 1 rather than figure 2? And speaking of figure 2, this figure showcasing open loop / closed loop performance isn’t discussed in the text.

The reference to Fig. 2 at line 154 is correct; however, I agree that the wording was potentially misleading, as the term “schematic representation” may be more naturally associated with Fig. 1. I have therefore revised the sentence to clarify that Fig. 2 illustrates the frequency-domain response of the system rather than its conceptual architecture.
In addition, I acknowledge that Fig. 2 was not explicitly discussed in the original text. In the revised manuscript, I have added a dedicated explanation describing the open-loop and closed-loop responses, highlighting the suppression of the mechanical resonance and the role of force-feedback control in shaping the system dynamics.
These modifications improve the clarity and consistency between the figures and the main text.

Minor issues
Line 154: Isn’t this a reference to figure 1 rather than figure 2? And speaking of figure 2, this figure showcasing open loop / closed loop performance isn’t discussed in the text.

The reference to Fig. 2 at line 154 is correct; however, I agree that the wording was potentially misleading, as the term “schematic representation” could be more naturally associated with Fig. 1, which illustrates the conceptual architecture of the system. I have therefore revised the sentence to clarify that Fig. 2 refers to the frequency-domain response of the system rather than its structural layout.
In addition, I acknowledge that Fig. 2 was not explicitly discussed in the original manuscript. In the revised version, I have added a dedicated description of the figure, highlighting the differences between open-loop and closed-loop responses, the suppression of the mechanical resonance under force-feedback control, and the corresponding behaviour of the force-balance transfer function.
These modifications improve the clarity and consistency between the figures and the main text.

Line 185: Would it be possible to add a reference to the “analytical treatments that neglect realisability constraints”?

I agree that supporting this statement with appropriate references improves the rigor and traceability of the argument.
In the revised manuscript, I have added references to classical treatments in linear systems and signal processing where idealised or non-causal representations are commonly employed, as well as to literature on inertial sensor modeling where simplified or unconstrained formulations are used for analytical tractability.

These additions clarify the context in which realisability constraints may be relaxed and better position the proposed framework with respect to existing analytical approaches.

Line 209, equation 1, a_g(t) is ground acceleration to be measured. In figure 1, this quantity is denoted by u-double-dot. Is there a reason for this choice (shift?) of notation?

The quantities $a_g(t)$in Eq. (1) and $\ddot{u}$ in Fig. 1 represent the same physical quantity, namely the ground acceleration. The use of different notations reflects a distinction between the formal system representation adopted in the equations and the more traditional kinematic notation used in the conceptual schematic.
To avoid ambiguity, I have clarified this point in the revised manuscript by explicitly stating the equivalence between the two notations.

Line 243: spaces are missing after mathematical notation.

I have corrected the spacing after mathematical expressions at the indicated location and performed a careful revision of the manuscript to ensure consistent formatting throughout.

Section 8 and 9. There is a lot of similarity between the discussion and the conclusion. It might be preferred to shorten one of these sections. If space is made available in the discussion section, it could be interesting and worthwhile to add an example or two with design considerations where the results from figure 4-7 come into play.

I agree that improving the balance between the Discussion and Conclusions sections enhances the clarity and impact of the manuscript.
In the revised version, I have reduced redundancy by streamlining the Conclusions section, focusing it on the main consolidated results and broader implications. The Discussion has been correspondingly refined to emphasise interpretation rather than repetition.
In addition, I have expanded the Discussion by introducing illustrative design-oriented examples that demonstrate how the results derived from Figs. 4–7 can inform practical sensor design decisions. These examples highlight the role of crossover frequencies, dominant noise regimes, and near-plateau bandwidth in guiding trade-offs between mechanical, electronic, and digital subsystems.
These modifications improve the overall structure of the manuscript and strengthen the connection between the theoretical framework and its practical applications.

Figure 6 and 7. In both these figures, panel (a) is missing the x-label “Frequency [Hz]”.

In the current figure layout, panel (a) shares the horizontal axis with panel (b), where the label “Frequency [Hz]” is already provided. For this reason, the label was not repeated in panel (a) to avoid redundancy.

The abbreviation ENOB is used several times in the manuscript, but it is never defined.

The abbreviation ENOB (Effective Number of Bits) has now been defined at its first occurrence in the manuscript.