nature portfolio

Peer Review File

Supporting Upper Limb Movements with a Soft Robot after a Brachial Plexus Injury

Corresponding Author: Dr Tommaso Proietti

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)
Overall Assessment
This manuscript presents a promising and potentially important contribution in an underexplored clinical area. Its main strength is the focus on multi-joint proximal assistance for individuals with BPI, with encouraging immediate assistive outcomes. However, stronger support for some of the broader claims would require clearer positioning of the work as assistive versus rehabilitative, more cautious interpretation of the limited functional data, and substantially greater methodological detail regarding device design, control, and personalization. These will strengthen the manuscript's contribution.

1. What are the noteworthy results?
The study addresses an important and relatively underexplored problem: multi-joint proximal assistance for individuals with BPI. The reported improvements in shoulder/elbow movement capacity, static endurance, perceived exertion, and muscle activity suggest that the device may provide meaningful immediate assistive benefit during use. The inclusion of user-reported outcomes is also a strength, as it provides useful information regarding perceived efficacy, satisfaction, and usability. In addition, the study attempts to move beyond isolated impairment-level metrics by including task-related outcomes. Although these data were collected only in a small subset of participants, they suggest potential functional relevance.

2. Will the work be of significance to the field and related fields? How does it compare to the established literature? If the work is not original, please provide relevant references.
I believe the work is potentially significant to the field of wearable robotics and assistive technology. Prior BPI-related robotic studies have largely focused on elbow-centered orthoses/training or hand/grasp devices. Accordingly, the manuscript appears to offer a meaningful degree of originality if the present device delivers multi-joint proximal assistance and evaluates this in a BPI cohort rather than extrapolating from other neurological populations.

That said, the originality should be framed carefully. The significance does not lie simply in the use of a wearable robotic device in BPI, since prior studies have already examined robotic or orthotic support for elbow function and, in some cases, hand function. Rather, the likely contribution here is the integration of soft wearable design, multi-joint support, and immediate assistive testing in BPI. This reviewer, therefore, believes that the manuscript would benefit from comparing the present approach primarily with prior assistive systems.

3. Does the work support the conclusions and claims, or is additional evidence needed?
The current data appear to support claims of immediate assistive benefit during device use, particularly with respect to movement range, endurance, and reduced perceived effort. However, additional evidence is needed to support broader claims regarding daily function, clinical impact, transparency, and the broader significance of the design.

The study largely evaluates the range of motion and endurance. These are meaningful outcomes, but the connection between these measures and everyday functional performance could be explained more clearly. The task-related data are promising, but because they were obtained only in a small subset of participants (N=4), conclusions regarding daily-life benefit should remain cautious.

The manuscript can be clearer by clarifying more explicitly whether the contribution is intended to be assistive, rehabilitative, or both. If the device is intended primarily as an assistive system, then improvements measured during use are appropriate and valuable. However, if the discussion suggests rehabilitative benefit (e.g., as in reference 10), additional evidence would be needed, including testing without the device after repeated training, as in the comparison with other devices.

Further detail is also needed regarding the tuning procedure, personalization method, intention-detection strategy, and response latency, as it can be helpful to assess the robustness and generalizability of the approach.

4. Are there any flaws in the data analysis, interpretation and conclusions? Do these prohibit publication or require revision?
There are several issues in the analysis and interpretation that should be addressed, but they do not necessarily preclude publication, in my opinion.

A primary concern is that the manuscript appears to draw relatively broad conclusions from a dataset that is uneven across outcome measures. Shoulder-related outcomes were evaluated in a larger group (N=14) than elbow-related outcomes (N=8), and task-based functional outcomes were obtained only in a subset (N=4). The rationale and interpretation for these different sample sizes could be further clarified, and the conclusions need to be scaled accordingly.

A second concern is the interpretation of the task-based movement analysis. The conclusions drawn from the SPARC-related results, particularly in Figure 6, do not appear fully convincing and may oversimplify the observed trends. This part of the analysis would benefit from a more cautious and task-specific interpretation.

A third concern relates to claims regarding transparency. Because the endurance and ADL comparisons appear to have been made between powered and unpowered device conditions, rather than between powered assistance and a true no-device condition, these claims may be stronger than the evidence currently supports and should be moderated.
Please see below for detailed major and minor comments.

5. Is the methodology sound? Does the work meet the expected standards in your field?
The methodology appears reasonable for an exploratory assistive-device study, and in that sense, it is broadly consistent with expected standards for an early translational paper in rehabilitation robotics. The inclusion of quantitative movement and performance outcomes together with physiological and user-reported measures is a strength. Testing the device directly in individuals with chronic BPI is also an important positive aspect.

However, several aspects require clearer description or justification, including the participant-specific tuning procedure, control architecture, intention-detection method, timing/latency of assistance, and stopping criteria for the movement tasks. The manuscript could also better justify the selected outcome measures and explain their clinical relevance for this population.

Please see below for detailed major and minor comments.

6. Is there enough detail provided in the methods for the work to be reproduced?
At present, the methods do not appear to provide enough detail for full reproducibility.

In particular, the manuscript could be clearer by providing more information on the control architecture, IMU-based intention-detection method, personalization/tuning procedure, subject-specific pressure selection, latency from intention detection to actuator response, and the exact task definitions and stopping criteria.

A schematic of the system and human-device interface, together with a more detailed technical supplement, would substantially improve reproducibility. A participant-level outcome table would also improve transparency and help readers understand individual variability.

** Below are the major and minor comments for this manuscript.
* Major comments:
1) The study presents encouraging evidence of improved range of motion and endurance with the device. However, the connection between these outcomes and meaningful daily function could be more clearly established. Increased range of motion is important, but in this study, it appears to have been measured primarily during device use. This differs from rehabilitation-oriented studies that assess whether training with the device leads to improvements when participants are tested without it. The manuscript would benefit from a clearer explanation of the intended contribution of the current approach—whether it is primarily assistive, rehabilitative, or both—and how the conclusions should be interpreted accordingly.

2) The manuscript focuses largely on range of motion and endurance outcomes. These are useful metrics, but their clinical meaning for everyday activities in individuals with BPI can enhance the significance of the manuscript - why endurance tasks are important for this population and how the observed gains are expected to translate to improved daily performance.

3) Shoulder motion was evaluated in 14 participants, whereas elbow flexion appears to have been assessed in only 8 participants. The explanation for this difference could enhance the clarity. Also, then the implications for interpreting the results could be discussed. It is also unclear to this reviewer whether the 4 participants who completed the ADL-related evaluation were a subset of the 8 participants assessed for elbow outcomes. Because the functional-task data are based on 4 individuals, the claims regarding daily-life relevance should be presented cautiously. A participant-level summary table of

outcomes would be very helpful to understand consistency and variability across the cohort.

4) The manuscript would benefit from a more detailed description of the tuning mechanism, personalization method, and intention-detection strategy. The text indicates that intention was detected using an IMU, but it is not sufficiently clear how intention was inferred, how assistance was triggered, or how subject-specific parameters such as pressure were selected. A control architecture figure or a detailed supplementary description would substantially improve the technical clarity of the work, particularly given the differences between healthy and BPI participants. In addition, the delay between intention detection and actuator response could be quantified, as this is important for interpreting both performance and usability.

5) Although range of motion is a non-dimensional metric, the effective reachable workspace or travel distance may depend on device size, fit, and geometry. The stopping criteria for the movement tasks could be clearly defined or the control algorithm could be more clearly explained. It would be helpful to specify whether task completion was limited by participant tolerance, device constraints, compensatory movement, pain, loss of assistance, or another predefined criterion.

6) The interpretation of the functional movement analysis, particularly Figure 6C, can be revisited.
This reviewer was not fully convinced by the current interpretation. The manuscript appears to suggest that SPARC deteriorated mainly in the incomplete-lesion participant while being preserved in others. However, my reading of the figure suggests that the head-related task may have deteriorated more broadly across participants. Participant ID12 also seems to show deterioration in the water and block tasks. In addition, the participant IDs shown across panels appear inconsistent (Figure 6AB). The head task also appears to involve a longer delay (Figure 6A), which may deserve further discussion. I encourage the authors to revisit this analysis and provide a more careful, task-specific interpretation.

7) I agree that multi-joint assistance is a potentially important contribution of this work. However, the manuscript could explain more clearly why shoulder assistance is clinically important in this population and how it contributes beyond elbow support alone. For several of the tasks shown, elbow function appears to be the dominant contributor, except perhaps for the head-related task. The authors should discuss the clinical meaning of shoulder assistance, particularly in relation to proximal control and compensation patterns.

8) The discussion presents transparency as an important advantage of the system. However, for the endurance and ADL tasks, the comparisons appear to have been made between powered and unpowered device conditions, rather than between powered assistance and a true no-device condition. Therefore, strong claims about transparency may not yet be fully supported by the current evidence. The relevant discussion should either be moderated or supported with additional data.

* Minor comments
1) The user-reported outcomes are a strength of the manuscript. It would help to connect these more directly to the objective findings, such as reduced fatigue, improved endurance, and increased movement capacity, especially in relation to perceived efficacy and satisfaction.

2) Because weight appears to have emerged as a relevant factor in user feedback, it would be useful to relate this point to the current design and discuss whether weight may influence usability, comfort, or performance.

3) The manuscript discusses limitations related to the human interface. A figure or schematic of the current interface design would help readers understand these limitations more clearly.

4) In the limitation paragraph in the discussion, the fact that the everyday task-related outcomes were evaluated in only 4 participants should be noted.

5) A table listing key outcomes for each participant would help readers understand individual variability, especially given the heterogeneity of the cohort.

6) When making interpretive claims in the Discussion, it would be helpful for the readers if the authors cite the corresponding figures or result sections more explicitly.

7) A supplementary section describing the design, control, intention-detection method, personalization procedure, and subject-specific pressure setting in more detail would make the contribution easier to understand and assess.

Reviewer #2

(Remarks to the Author)
This manuscript evaluates a multi-joint soft exosuit providing pneumatic assistance at the shoulder (gravity compensation) and optionally at the elbow (triggered assistance), controlled via IMU-derived kinematics. In a single-session, within-subject protocol across baseline, robot-off, and robot-on conditions for dynamic range of motion (ROM), and robot-off versus robot-on conditions for the static holding endurance task, 14 adults with chronic traumatic brachial plexus injury (BPI) show substantial immediate improvements in proximal active range of motion and static endurance, accompanied by reductions in perceived exertion and sEMG amplitude. User-reported agency and satisfaction are generally positive. In a small subset of participants with sufficient hand functon(n = 4), ARAT scores improve modestly and simulated activities of daily living appear qualitatively less compensatory.

Overall, the study is well designed for an initial clinical feasibility demonstration in a challenging and under-studied population. The inclusion of a robot-off condition is a notable strength, allowing the authors to separate active assistance effects from passive mechanical constraints. The convergence of kinematic, physiological, and subjective outcome measures provides convincing evidence that the device can provide immediate assistance in gravity-limited tasks. However, several conclusions are currently stronger than the evidence supports, and important methodological details required for reproducibility and the assessment of potential bias should be clarified.

Major comments
1. Functional impact is currently supported by limited quantitative evidence
The main functional endpoint (ARAT) is reported for only four participants, and baseline ARAT scores are already high (47–51/57), suggesting possible ceiling effects. The mean improvement (+4.5 points) is therefore difficult to interpret as broadly "clinically meaningful" (also in the absence of an established MCID for this population). The simulated ADL results are qualitative. Claims regarding clinical or functional impact should therefore be tempered and framed primarily as evidence of immediate assistive feasibility. Please also clarify which comparison condition is used for ARAT (baseline vs robot-off), as the text and figure captions appear inconsistent.

2. Interpretation should reflect the single-session design
The study demonstrates immediate assistance effects only. It does not address learning, day-to-day variability, longer-term adaptation, or potential changes in compensatory strategies with repeated use. While this limitation is acknowledged in the manuscript, the conclusions and abstract should reflect it more clearly.

3. Condition ordering and potential expectancy effects
The manuscript does not clearly state whether the order of experimental conditions was randomized or counterbalanced for the ROM and endurance tasks. Because the robot-on condition is inherently unblinded, subjective measures such as Borg ratings may be susceptible to expectancy effects. The authors should clarify condition ordering and discuss this limitation.

4. Control and intention-detection methods require clearer description
The overall control framework is understandable, but key implementation details are missing. In particular, the elbow "volitional movement detection" used to trigger assistance is not sufficiently specified (signal features, thresholds, filtering, hysteresis, inflation/deflation logic). Providing these details is important for reproducibility and for evaluating robustness in compensation-rich movement patterns typical of BPI.

5. Design considerations for the BPI population
The manuscript emphasizes that this work represents a clinical translation of previously developed soft-assistive modules to a biomechanically and clinically distinct population. While several BPI-specific challenges are discussed, the manuscript would benefit from a clearer description of how these considerations informed the hardware and interface design, especially the redesign of the shoulder module. For example, it would be useful to briefly summarize which aspects of the device architecture or control strategy were specifically adapted for BPI compared with prior ALS or SCI deployments. Making this design rationale more explicit would help clarify the novelty and translational contribution of the work.

6. Interpretation of EMG results
EMG amplitude is normalized to the maximum value observed across all trials/conditions rather than to a standardized MVC. This approach is understandable in an impaired cohort but should be discussed more explicitly, as it may influence cross-condition comparisons. Additionally, the conclusion that unchanged agonist EMG indicates preserved voluntary engagement appears somewhat stronger than the data alone support.

Additional comments:

- The baseline / robot-off / robot-on design provides good evidence of mechanical transparency during dynamic ROM tasks. However, the endurance task only compares robot-off and robot-on conditions; therefore passive transparency during sustained holding cannot be assessed in the same way and should not be generalized too broadly.

- Elbow outcomes are reported for a subset of participants (n = 8); this should be clearly and consistently indicated across text, figures, and statistical reporting.

- The manuscript notes exclusion of EMG channels due to artifacts or very low signal amplitude. Please report the exact per-muscle sample sizes for each analysis.

- The multiple-comparison correction strategy should be clarified.

- The Methods indicate that the system is actuated by an offboard controller. Additional description of the tethered configuration (e.g., tubing/cabling arrangement, pump type and location, noise, and constraints on movement) would help readers interpret translational claims.

- A brief explicit statement regarding adverse events or tolerability (e.g., discomfort, pain exacerbation, skin irritation) would strengthen the feasibility framing.

- There are minor internal inconsistencies that should be corrected, including the response scale reported for the sense-of-

agency questionnaire (1–5 vs 1–7) and some figure cross-references in the user-experience section.

Significance
This study extends soft, multi-joint upper-limb assistance to an under-studied clinical population and demonstrates that gravity-compensating support can improve reachable workspace and reduce effort in individuals with heterogeneous BPI impairments. The technical concept itself is not entirely new; therefore the primary contribution lies in the BPI-specific application and clinical demonstration. With clearer methodological reporting and more cautious interpretation of functional outcomes, the manuscript would represent a valuable feasibility study in this area.

Reviewer expertise
My evaluation focuses on experimental design, assistive-robotics methodology, and interpretation of kinematic and EMG outcomes. I am less able to assess the detailed clinical implications of nerve-transfer procedures within the heterogeneous BPI cohort. I acknowledge that I used a secure enterprise AI tool to assist in drafting my comments and cross-checking them against the manuscript.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
The revised manuscript is substantially improved. The authors have clarified the intended scope of the study, added methodological details, provided participant-level outcome information, and better positioned the work as an early translational assistive-device study rather than a rehabilitation or long-term efficacy trial. The revised framing, namely that the exosuit provides immediate assistive support during use, is appropriate.

The main contribution of the work is the early clinical translation of a multi-joint soft exosuit to individuals with chronic traumatic brachial plexus injury, a heterogeneous population with substantial proximal weakness and compensatory movement strategies. In contrast to prior devices that have largely focused on isolated elbow assistance or distal hand function, this system targets the proximal shoulder–elbow chain, which is critical for functional reach. The study demonstrates immediate improvements, particularly in active ROM and endurance.

The revisions also help clarify that the study evaluates the device as an assistive technology, not as a rehabilitation intervention. This distinction is important. The current results support immediate within-session benefit while the device is worn and powered, but they do not establish long-term therapeutic effects, carry-over after device removal, or home-use feasibility.
Several points still require attention before the manuscript is fully clear.

The participant subgroup definitions still need clarification. The manuscript states that shoulder outcomes were evaluated in all 14 participants, whereas elbow outcomes were assessed only in 8 participants because the remaining 6 had near-physiological elbow control. This rationale is reasonable, but the manuscript should provide the baseline elbow ROM values used to define the elbow subgroup. In addition, the ARAT/ADL subgroup should be clearly defined as a separate subgroup selected based on sufficient residual hand function. The current supplementary caption appears to state that the ARAT/ADL participants were a subset of the elbow group, but the participant IDs in Table S1 appear inconsistent with that statement. This should be corrected.

The functional relevance claims should remain cautious. The ARAT and simulated ADL results are encouraging, but they are based on only four participants. Although the authors have softened the wording in several places, this reviewer recommends checking the manuscript again to ensure that these findings are consistently framed as proof-of-concept evidence that the device may support selected functional tasks in participants with sufficient residual hand function, rather than as evidence of generalizable daily-life benefit across the full BPI cohort.

The portability and real-world translation should be described more precisely. The wearable portion appears lightweight, but the pneumatic control unit is off-board and benchtop-mounted. Thus, the current system is wearable but not fully portable or independent for daily use. The authors appropriately mention future onboard actuation, but the manuscript should avoid overstating feasibility for home or community use until untethered operation, independent donning/doffing, and longer-term usability are evaluated.

The added methodology improves transparency, especially regarding IMU-based intention detection, subject-specific tuning, pneumatic control, and pressure regulation. However, full reproducibility would still require more explicit details about calibration decision rules, pressure-map interpolation, filtering, controller gains, and actuator output characterization. If these details cannot be included, the authors should state that the added methods improve methodological transparency rather than ensuring full reproducibility.

Overall, the revised manuscript is stronger, and the study makes a meaningful early translational contribution. The work is best viewed as a proof-of-concept clinical demonstration of immediate assistive benefit from multi-joint soft robotic support in chronic BPI. With clearer subgroup definitions, more cautious functional claims, and a sharper statement of the main contribution, the manuscript would be substantially improved.

Reviewer #2

(Remarks to the Author)
The authors have responded thoughtfully to the comments of both reviewers, and the manuscript has improved substantially in clarity, balance, and methodological detail. I believe the revised version is considerably strengthened.

A few points, however, may still benefit from clarification or minor further revision:

First, it would be helpful if the authors could clarify how assistance is terminated or reduced once IMU-based initiation has occurred, particularly for the elbow triggered-passive mode. While the revised manuscript explains initiation more clearly, the stop or deactivation logic remains somewhat difficult to follow. The authors state that "Movement termination was manually defined via the GUI.", but it remains unclear how this would translate to a scenario where ADL that require continuously varying elbow angles are supported.

Second, given that functional ADL testing was feasible in only 4 participants, it may be worth noting in the Discussion that integration with a hand-assistance module could represent an important next step for improving broader functional relevance.

Third, the expression “clinically meaningful” still appears several times throughout the manuscript; in the opinion of this reviewer, that wording would be better moderated, particularly in the absence of an established MCID for the ARAT in this population.

Fourth, I was not sure that my earlier comment regarding noise from the pneumatic pump and valves was fully addressed. In a clinical or at-home setting, pump noise could affect usability and acceptability, and it may therefore be useful to discuss this point briefly or acknowledge it as a limitation.

Finally, the newly added tracked text would benefit from a careful grammar and style check before final acceptance.

Version 2:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
The revised manuscript is improved. The authors have clarified the scope of the study, added useful methodological and participant-level details, and more appropriately framed the work as an early translational assistive-device study rather than a rehabilitation or long-term efficacy trial.

The study’s main contribution is the early clinical demonstration of a multi-joint soft exosuit for individuals with chronic traumatic brachial plexus injury. The focus on the proximal shoulder–elbow chain is clinically relevant, and the immediate improvements in active range of motion and endurance are encouraging.

The authors have addressed several prior concerns. The subgroup definitions are now clearer, including the distinction between shoulder-only participants, participants tested with the elbow module, and the separate ARAT/ADL subgroup selected based on sufficient residual hand function. Supplementary Table S1 is helpful in this regard.

I also appreciate the more cautious framing of the ARAT and simulated ADL findings. Because these results are based on only four participants, they should continue to be presented as proof-of-concept evidence in selected participants rather than as generalizable evidence of daily-life benefit.

The discussion of portability and real-world translation is also improved. Since the current system still relies on an off-board pneumatic control unit, claims regarding home or community feasibility should remain cautious until untethered operation, independent donning/doffing, and longer-term usability are evaluated.

Overall, the revised manuscript is clearer and more appropriately framed as an early proof-of-concept demonstration of immediate assistive benefit from multi-joint soft robotic support in chronic traumatic brachial plexus injury.

Reviewer #2

(Remarks to the Author)
The authors have further improved the manuscript considering the comments of both reviewers. This is now a strong proof-of-concept study supporting the feasibility and immediate assistive effects of an exosuit to support proximal arm function in BPI.

Only few points remain to be addressed:

1) The wording regarding clinical significance is now appropriately stated as proof of concept evidence and clinical feasibility evaluation. However, there is still a statement about “clinical translation” in the introduction which should be adapted to reflect proof-of-concept/feasibility evaluation (“Therefore, this new clinical translation…”). Following the same reasoning, in the same sentence, the word “establishing” should be replaced with “testing” or “examining”).

2) The authors now mention the manual venting of the actuators after movement completion in the supplemental material. However, this should already be mentioned in the methods.

3) Line 432: There is an issue with the added sentence, which is currently incomplete (starting with “are encouraging for future...”).

Reviewer #1

Overall Assessment

This manuscript presents a promising and potentially important contribution in an underexplored clinical area. Its main strength is the focus on multi-joint proximal assistance for individuals with BPI, with encouraging immediate assistive outcomes. However, stronger support for some of the broader claims would require clearer positioning of the work as assistive versus rehabilitative, more cautious interpretation of the limited functional data, and substantially greater methodological detail regarding device design, control, and personalization. These will strengthen the manuscript's contribution.

[Answer 1.1]

We thank the reviewer for this constructive and encouraging evaluation of our manuscript. We appreciate the recognition of the importance of multi-joint proximal assistance for individuals with BPI and have carefully addressed the reviewer's concerns by clarifying the assistive scope of the study, moderating claims based on the limited functional dataset, and adding further methodological detail on device design, control, and personalization. We believe these revisions have improved both the rigor and clarity of the manuscript.

1. What are the noteworthy results?

The study addresses an important and relatively underexplored problem: multi-joint proximal assistance for individuals with BPI. The reported improvements in shoulder/elbow movement capacity, static endurance, perceived exertion, and muscle activity suggest that the device may provide meaningful immediate assistive benefit during use. The inclusion of user-reported outcomes is also a strength, as it provides useful information regarding perceived efficacy, satisfaction, and usability. In addition, the study attempts to move beyond isolated impairment-level metrics by including task-related outcomes. Although these data were collected only in a small subset of participants, they suggest potential functional relevance.

[Answer 1.2]

We thank the reviewer for highlighting these key strengths of our study, including the multi-joint proximal assistance approach, the convergence of objective and user-reported outcomes, and the inclusion of task-related functional data.

2. Will the work be of significance to the field and related fields? How does it compare to the established literature? If the work is not original, please provide relevant references.

I believe the work is potentially significant to the field of wearable robotics and assistive technology. Prior BPI-related robotic studies have largely focused on elbow-centered orthoses/training or hand/grasp devices. Accordingly, the manuscript appears to offer a meaningful degree of originality if the present device delivers multi-joint proximal assistance and evaluates this in a BPI cohort rather than extrapolating from other neurological populations. That said, the originality should be framed carefully. The significance does not lie simply in the use of a wearable robotic device in BPI, since prior studies have already examined robotic or orthotic support for elbow function and, in some cases, hand function. Rather, the likely contribution here is the integration of soft wearable design, multi-joint support, and immediate assistive testing in BPI. This reviewer, therefore, believes that the manuscript would benefit from comparing the present approach primarily with prior assistive systems.

[Answer 1.3]

We thank the reviewer for this thoughtful assessment of the manuscript's significance and positioning. We agree that the contribution should be framed not as the mere application of a wearable robotic device in BPI, but as the combination of soft wearable design, multi-joint proximal assistance, and immediate assistive testing in a clinically challenging BPI cohort. To better situate the work in the established literature, we revised the Introduction and Discussion to compare the present approach primarily with prior assistive systems for BPI, including elbow-centered orthoses and hand/grasp devices, and we clarified that the main novelty of this study lies in proximal shoulder-elbow assistance delivered as immediate assistive support during use.

3. Does the work support the conclusions and claims, or is additional evidence needed? The current data appear to support claims of immediate assistive benefit during device use, particularly with respect to movement range, endurance, and reduced perceived effort. However, additional evidence is needed to support broader claims regarding daily function, clinical impact, transparency, and the broader significance of the design. The study largely evaluates the range of motion and endurance. These are meaningful outcomes, but the connection between these measures and everyday functional performance could be explained more clearly. The task-related data are promising, but because they were obtained only in a small subset of participants (N=4), conclusions regarding daily-life benefit should remain cautious.

[Answer 1.4]

We thank the reviewer for their comment. We agree that the present study primarily supports claims of immediate assistive benefit during device use, particularly for range of motion, endurance, perceived exertion, and reduction of muscle activity. We have therefore revised the manuscript to make clearer that the task-related results indicate promising functional relevance, but that they were obtained in a small subset of participants and should not be interpreted as definitive evidence of broad daily-life benefit. We also clarified in the Discussion that the study does not evaluate longer-term rehabilitative or carry-over effects, and that the clinical significance of the findings should be interpreted in the context of a single-session feasibility study.

The manuscript can be clearer by clarifying more explicitly whether the contribution is intended to be assistive, rehabilitative, or both. If the device is intended primarily as an assistive system, then improvements measured during use are appropriate and valuable. However, if the discussion suggests rehabilitative benefit (e.g., as in reference 10), additional evidence would be needed, including testing without the device after repeated training, as in the comparison with other devices. Further detail is also needed regarding the tuning procedure, personalization method, intention-detection strategy, and response latency, as it can be helpful to assess the robustness and generalizability of the approach.

[Answer 1.5]

We thank the reviewer for this important clarification. We agree that the present study should be framed as an assistive system rather than a rehabilitative intervention. Accordingly, we have revised the manuscript to make explicit that the exosuit was evaluated as a user-in-the-loop assistive device for immediate task support during use, and not as a training system intended to induce post-use rehabilitative effects. We have also removed or softened wording that could suggest longer-term rehabilitation outcomes.

4. Are there any flaws in the data analysis, interpretation and conclusions? Do these prohibit publication or require revision? There are several issues in the analysis and interpretation that should be addressed, but they do not necessarily preclude publication, in my opinion.
A primary concern is that the manuscript appears to draw relatively broad conclusions from a dataset that is uneven across outcome measures. Shoulder-related outcomes were evaluated in a larger group (N=14) than elbow-related outcomes (N=8), and task-based functional outcomes were obtained only in a subset (N=4). The rationale and interpretation for these different sample sizes could be further clarified, and the conclusions need to be scaled accordingly.

[Answer 1.6]

The different sample sizes reflect participant-specific inclusion criteria based on baseline motor capabilities: shoulder outcomes were evaluated in all 14 participants due to consistent proximal impairment; elbow flexion was assessed only in 8 participants who exhibited baseline elbow ROM <90° (the remaining 6 had near-physiological elbow control and used only the shoulder module); and ARAT/ADL tasks were completed by only 4 participants with sufficient residual hand function for object manipulation. These criteria are now explicitly stated in the Results section, and we have scaled our conclusions accordingly by emphasizing that functional task results represent proof-of-concept observations rather than generalizable findings.

A second concern is the interpretation of the task-based movement analysis. The conclusions drawn from the SPARC-related results, particularly in Figure 6, do not appear fully convincing and may oversimplify the observed trends. This part of the analysis would benefit from a more cautious and task-specific interpretation.

[Answer 1.7]

We thank the reviewer for this careful reading of Figure 6. We agree that the SPARC interpretation should be more cautious and data-driven. We have revised the text to emphasize that smoothness changes were heterogeneous and task-dependent: smoothness was generally preserved or improved in participants with smaller ARAT gains (+2 points), while reductions occurred in those achieving larger functional improvements (+7 points), and the head task showed consistent smoothness reductions likely due to exploration of novel movement solutions. This more nuanced interpretation better reflects the observed trends.

A third concern relates to claims regarding transparency. Because the endurance and ADL comparisons appear to have been made between powered and unpowered device conditions, rather than between powered assistance and a true no-device condition, these claims may be stronger than the evidence currently supports and should be moderated. Please see below for detailed major and minor comments.

[Answer 1.8]

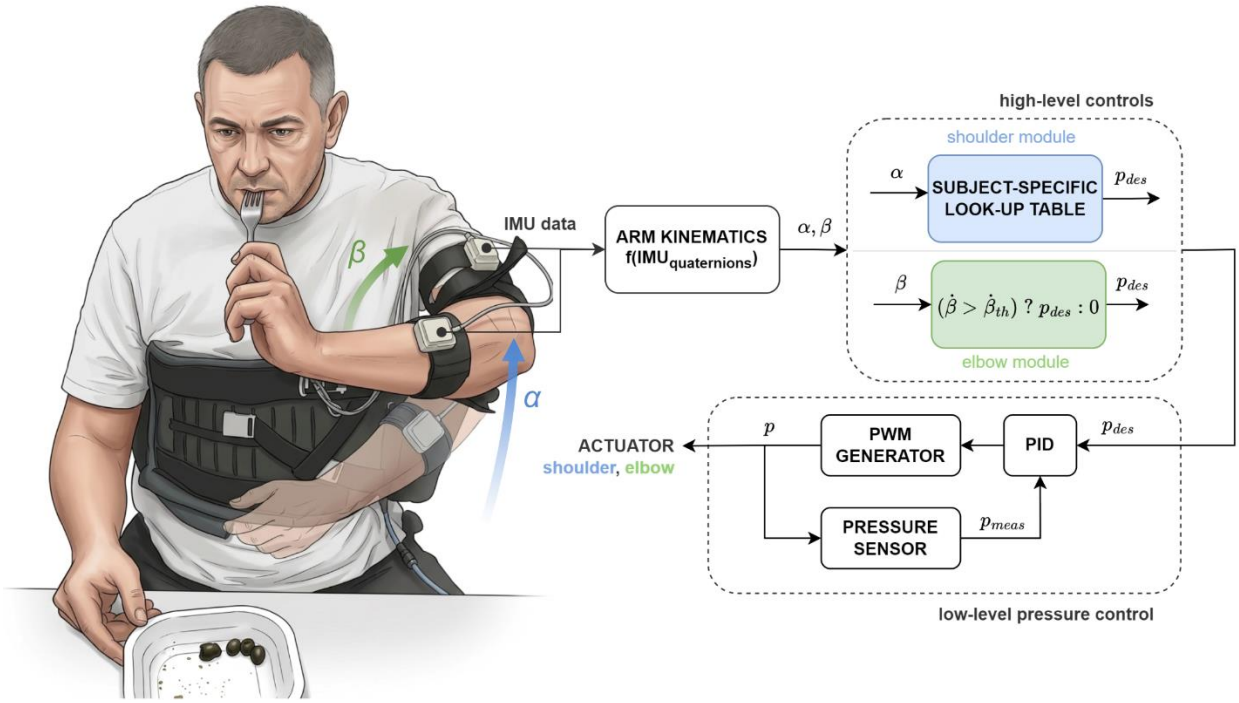
We thank the reviewer for the feedback. Mechanical transparency is directly demonstrated in the ROM task (Fig. 2A), where no significant ROM reduction was observed between baseline (no device) and robot-off conditions across shoulder flexion, abduction, and elbow flexion ($p>0.05$, post-hoc tests). This confirms the device does not restrict movement when unpowered. Therefore, for endurance and ADL tasks, comparisons were made only between powered and unpowered conditions (both with device worn), which is standard practice when evaluating assistive benefit (see works from other groups, e.g. Lotti et al. *Soft Robotics* 2023, Georgarakis et al. *Nature Machine*

Intelligence 2022). However, we acknowledge this limitation in the new version of the Discussion.

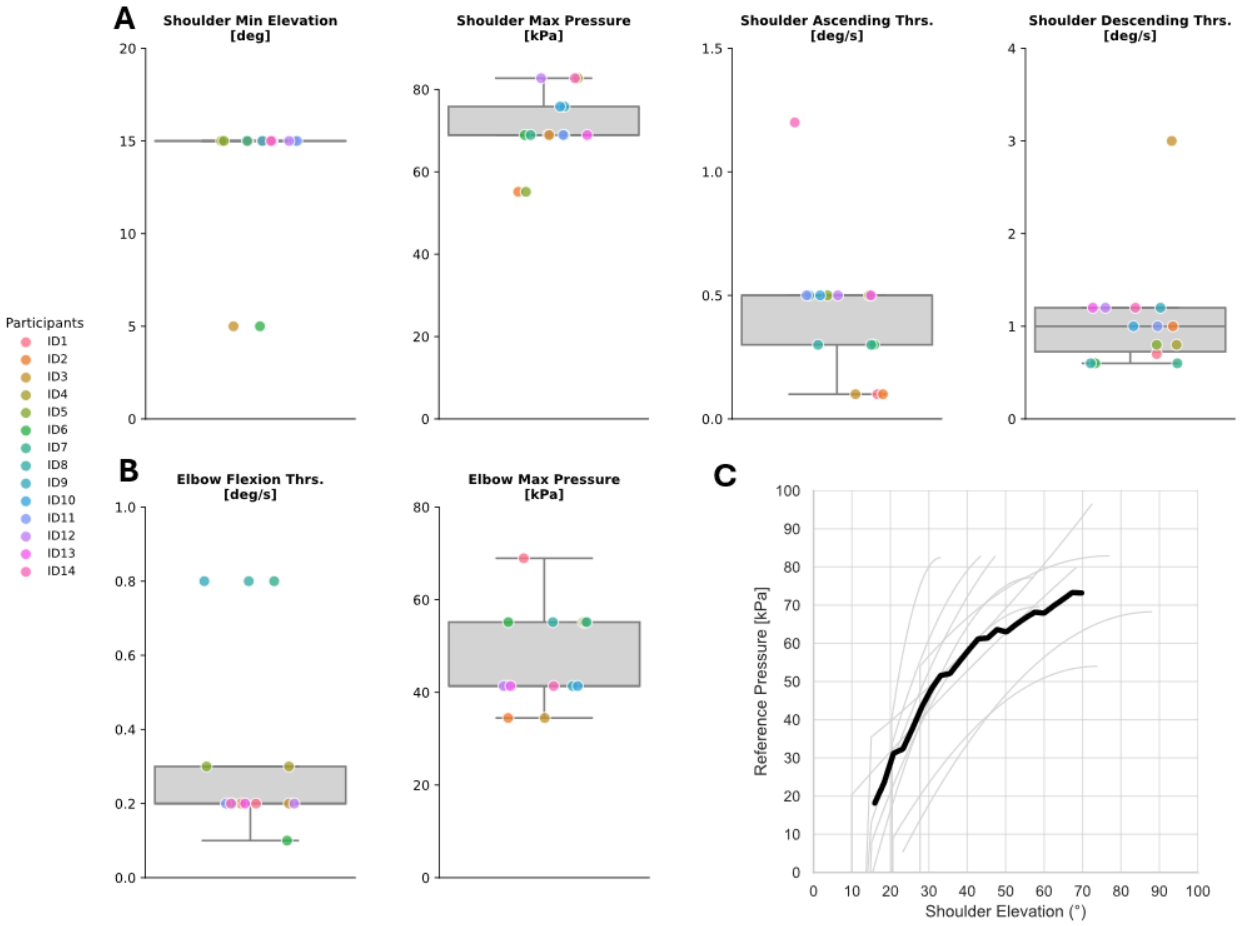
5. Is the methodology sound? Does the work meet the expected standards in your field?
The methodology appears reasonable for an exploratory assistive-device study, and in that sense, it is broadly consistent with expected standards for an early translational paper in rehabilitation robotics. The inclusion of quantitative movement and performance outcomes together with physiological and user-reported measures is a strength. Testing the device directly in individuals with chronic BPI is also an important positive aspect. However, several aspects require clearer description or justification, including the participant-specific tuning procedure, control architecture, intention-detection method, timing/latency of assistance, and stopping criteria for the movement tasks. The manuscript could also better justify the selected outcome measures and explain their clinical relevance for this population.
Please see below for detailed major and minor comments.

[Answer 1.9]

We thank the reviewer for recognizing that the methodology meets expected standards for an early translational assistive-device study. In response to the missing methodological details, we added two new Supplementary Figures (Fig. S1 and S2, attached here for reference) and a dedicated Supplementary Methods section describing the kinematics-based control architecture, subject-specific tuning procedure, pressure regulation loop, and pneumatic control box hardware. These additions clarify how the IMU-derived intention signal is mapped to actuator assistance and possibly address the concern regarding low reproducibility.



[NEW] Supplementary Figure S1: Control architecture of the soft exosuit. The system uses three inertial measurement units (IMUs) to estimate upper-limb kinematics from torso, upper-arm, and forearm orientation. At the high-level control layer, shoulder assistance is generated through a subject-specific look-up table that maps arm kinematics to the desired shoulder pressure p_{des}. The look-up table is built via a simple calibration requiring inflation of the actuator while the participant is passive. Elbow assistance, instead, is triggered when the estimated elbow motion exceeds a subject-specific angular velocity threshold $\dot{\beta}_{th}$. The low-level pressure controller regulates actuator pressure using closed-loop feedback from the pressure sensor and a PID controller, which drives the PWM generator and pneumatic actuator.



[NEW] Supplementary Figure S2: Calibration parameters across participants. (A) Shoulder-related configuration parameters: distribution of minimum shoulder elevation (below this value, the assistance did not start), maximum calibration pressure (safety saturation), and angular velocity thresholds for ascending and descending movement detection. (B) Elbow-related configuration parameters: distribution of flexion angular velocity thresholds ($\dot{\beta}_{th}$) and maximum flexion pressures (corresponding to p_{des}). In panels A and B, gray boxplots represent the population distribution (interquartile range and median), while colored markers identify individual participants (see legend). (C) Shoulder look-up table profiles (inflation). Light gray lines represent individual trials, while the solid black line indicates the population average within the monotonic functional range (15°–70°).

6. Is there enough detail provided in the methods for the work to be reproduced? At present, the methods do not appear to provide enough detail for full reproducibility. In particular, the manuscript could be clearer by providing more information on the control architecture, IMU-based intention-detection method, personalization/tuning procedure, subject-specific pressure selection, latency from intention detection to actuator response, and the exact task definitions and stopping criteria. A schematic of the system and human-device interface, together with a more detailed technical supplement, would substantially improve reproducibility. A participant-level outcome table would also improve transparency and help readers understand individual variability.

[Answer 1.10]

We thank the reviewers for requesting further methodological detail. Please, consider our answer 1.9.

Below are the major and minor comments for this manuscript.

Major comments:

1) The study presents encouraging evidence of improved range of motion and endurance with the device. However, the connection between these outcomes and meaningful daily function could be more clearly established. Increased range of motion is important, but in this study, it appears to have been measured primarily during device use. This differs from rehabilitation-oriented studies that assess whether training with the device leads to improvements when participants are tested without it. The manuscript would benefit from a clearer explanation of the intended contribution of the current approach—whether it is primarily assistive, rehabilitative, or both—and how the conclusions should be interpreted accordingly.

[Answer 1.11]

We thank the reviewer for this important clarification. We addressed this concern in our answer 1.5.

2) The manuscript focuses largely on range of motion and endurance outcomes. These are useful metrics, but their clinical meaning for everyday activities in individuals with BPI can enhance the significance of the manuscript - why endurance tasks are important for this population and how the observed gains are expected to translate to improved daily performance.

[Answer 1.12]

We thank the reviewer for this excellent suggestion. We have added explicit clinical justification for ROM and endurance measures in the Discussion, explaining their direct relevance to BPI: proximal weakness limits functional reach (ROM) while fatigue during sustained arm elevation drives compensatory trunk/scapular overuse and pain (endurance). These impairment-level gains during assistance directly translate to expanded workspace and reduced effort for reach-related ADLs like feeding and shelf access.

3) Shoulder motion was evaluated in 14 participants, whereas elbow flexion appears to have been assessed in only 8 participants. The explanation for this difference could enhance the clarity. Also, then the implications for interpreting the results could be discussed. It is also unclear to this reviewer whether the 4 participants who completed the ADL-related evaluation were a subset of the 8 participants assessed for elbow outcomes. Because the functional-task data are based on 4 individuals, the claims regarding daily-life relevance should be presented cautiously. A participant-level summary

table of outcomes would be very helpful to understand consistency and variability across the cohort.

[Answer 1.13]

We thank the reviewer for highlighting the need for clearer participant allocation rationale. The inclusion criteria are now explicitly stated in Results. To further clarify individual variability, we added Supplementary Table S1 (attached below for reference) showing participant-level baseline characteristics and key outcomes (ROM, endurance, ARAT where applicable). Functional claims are now appropriately scaled as proof-of-concept from the N=4 subset.

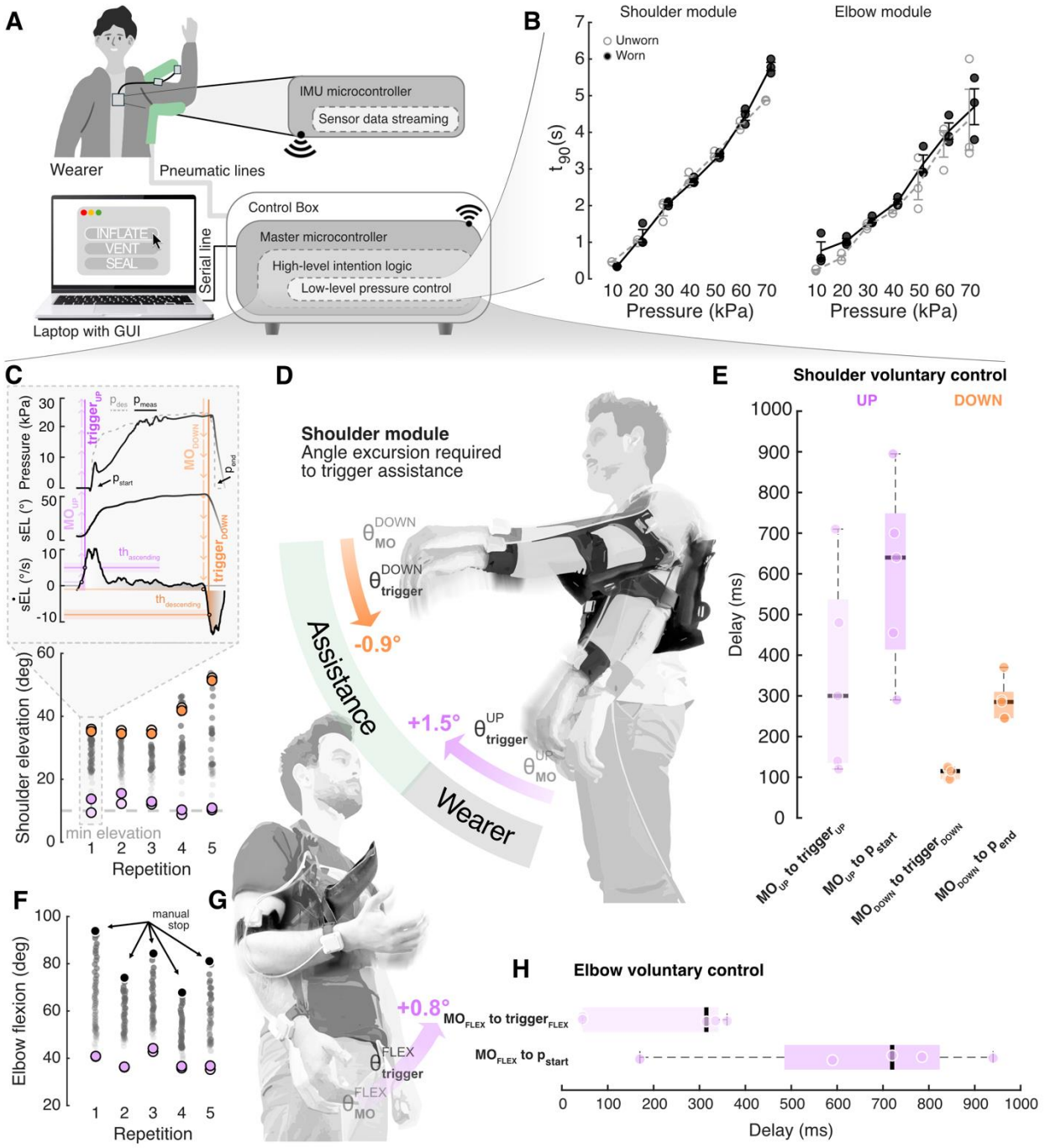
	ROM Task			Endurance Task							
ID	Shoulder Flexion	Shoulder Abduction	Elbow Flexion	Duration	AD	MD	PD	PECT	BIC	TRIC	BORG	ARAT
ID1	6.8	-2.0	643.6	26.6	-28.7	-31.1	0.5	-27.2	-59.1	-97.6	-6	-
ID2	168.0	114.8	148.4	0.0	8.5	4.2	3.6	1.9	-18.1	-17.2	-1	-
ID3	132.9	121.3	133.4	121.5	-50.0	-79.8	-84.9	-60.0	-91.5	-79.0	-8	-
ID4	95.5	141.8	-	84.9	-55.8	-30.1	-19.7	-	-52.2	-47.0	-6	2
ID5	155.7	146.9	203.8	39.3	-47.6	-50.2	-56.5	-69.0	-77.4	-54.8	-3	-
ID6	45.9	118.6	-	137.8	-40.5	-26.8	-21.8	-47.4	-7.8	-	0	7
ID7	16.2	40.5	-	87.6	-46.3	-33.5	-43.5	-64.9	-32.3	-52.5	-5	3
ID8	90.6	100.9	13.0	13.7	-71.2	-79.7	-45.4	-74.8	-91.5	-	-7	-
ID9	89.9	104.8	366.3	150.9	40.6	-79.3	-75.8	-79.1	-76.2	-72.9	-2.5	-
ID10	76.6	42.7	-	0.0	-50.5	-67.4	-65.0	-42.6	-64.3	-73.1	-1	-
ID11	108.4	140.5	162.3	135.6	-8.3	22.0	3.9	-77.3	-55.7	121.2	-6	-
ID12	41.0	117.3	-	124.9	-31.6	-48.8	-24.9	-	-19.8	-33.0	-5	6
ID13	13.5	32.2	-	114.4	-	-	-	-	-	-	-2	-
ID14	300.8	297.8	768.9	180.0	-	-	-	-	-	-	-	-
MEDIAN	**90.3**	**116.1**	**183.1**	**101.0**	**-43.4**	**-41.2**	**-34.2**	**-62.5**	**-57.4**	**-53.6**	**-5**	-

[New] Supplementary Table S1: Participant-level key outcomes. Shown are robot-off/robot-on changes for shoulder/elbow ROM (% change), endurance task duration (Δ seconds) and associated EMG activity (% change vs MVC, AD = anterior deltoid, MD = middle deltoid, PD = posterior deltoid, PECT = pectoralis major, BIC = biceps, TRIC = triceps), and Borg perceived exertion (absolute change), and ARAT scores (absolute change). Elbow outcomes (N=8) reflect participants with baseline elbow ROM <90°; ARAT/ADL tasks (N=4) reflect participants with sufficient hand function (subset of elbow group).

4) The manuscript would benefit from a more detailed description of the tuning mechanism, personalization method, and intention-detection strategy. The text indicates that intention was detected using an IMU, but it is not sufficiently clear how intention was inferred, how assistance was triggered, or how subject-specific parameters such as pressure were selected. A control architecture figure or a detailed supplementary description would substantially improve the technical clarity of the work, particularly given the differences between healthy and BPI participants. In addition, the delay between intention detection and actuator response could be quantified, as this is important for interpreting both performance and usability.

[Answer 1.14]

We thank the reviewer for requesting further methodological detail, which is largely addressed by our response to your previous comment 1.9. Additionally, to quantitatively characterize system responsiveness, we performed dedicated experiments to assess both actuator dynamics and control latency. Specifically, we measured the low-level pressure controller dynamics by evaluating the step-response rise time (t_{90}), defined as the time to reach 90% of the target pressure (0→70 kPa), for each actuation module (shoulder, elbow). We also quantified the temporal delay between the onset of the wearer's movement and the moment the control is triggered, i.e., when the pressure setpoint is assigned to the low-level controller, accounting for kinematics processing within the 100 Hz control loop. These temporal dynamics are now included in Supplementary Fig. S3 (attached here) and described in Supplementary Methods. Together, they provide a quantitative characterization of both actuator response and intention-to-actuation delay, supporting the responsiveness and practical usability of the control strategy used.



[NEW] Supplementary Figure S3: System architecture and temporal performance of the control strategy. (A) System overview and control architecture. The wearable exosuit integrates IMU sensors connected to a dedicated microcontroller that streams kinematic data via TCP/IP to the control box. The control box, interfaced with a laptop GUI, executes both the high-level intention detection logic and the low-level pressure regulation, driving the pneumatic actuation modules.

(B) Rise time (t_{90}) of the shoulder and elbow modules, defined as the time required to reach 90% of the target pressure, measured across pressure levels (10-70 kPa, 10 kPa increments). For each condition, three repetitions are shown along with mean ± standard deviation. Tests were performed both in unworn condition, and with the actuator worn by a representative subject (male, 174 cm, 70 kg).

(C – E) High-level voluntary control for the shoulder module. (C) Top: representative time-series of actuator pressure, shoulder elevation (sEL), and shoulder angular velocity (sEL•), illustrating a typical control cycle from rest to upward movement onset (MO_{UP}), trigger of assistance upon exceeding the ascending velocity threshold ($trigger_{UP}$), pressure setpoint assignment via the subject-specific look-up table, and subsequent downward movement onset (MO_{DOWN}), $trigger_{DOWN}$, and assistance termination. Bottom: scatter plot of shoulder elevation trajectories across five repetitions, highlighting initial and final angles during voluntary up-down movements. (D) Illustration of the median angular excursion required by the user to trigger assistance during upward and downward movements, computed as the difference between movement onset and controller trigger. (E) Temporal delays: (1) from MO to threshold crossing (trigger), and (2) from MO to pressure onset (p_{start}), defined as the first instant at which actuator pressure exceeds 0 kPa. Analogous metrics are reported for both upward and downward phases.

(F – H) High-level voluntary control for the elbow module. (F) Scatter plot of elbow flexion trajectories across repetitions, from flexion onset (MO_{FLEX}) to trigger ($trigger_{FLEX}$) and subsequent attainment of the personalized pressure setpoint. Movement termination was manually defined via the GUI. (G) Illustration of the angular excursion required to trigger elbow flexion assistance. (H) Temporal delays between MO_{FLEX} and (1) $trigger_{FLEX}$ (threshold crossing), and (2) p_{start} (onset of actuator pressurization).

Thresholds and pressure setpoints used in these experiments correspond to the median values observed across participants in the BPI study. Movement speed was natural and not strictly controlled, to reflect realistic operating conditions consistent with the experimental protocol.

5) Although range of motion is a non-dimensional metric, the effective reachable workspace or travel distance may depend on device size, fit, and geometry. The stopping criteria for the movement tasks could be clearly defined or the control algorithm could be more clearly explained. It would be helpful to specify whether task completion was limited by participant tolerance, device constraints, compensatory movement, pain, loss of assistance, or another predefined criterion.

[Answer 1.15]

We thank the reviewer for this important clarification. In the ROM tasks, no predefined stopping criterion related to pain, tolerance, device constraints, or loss of assistance was applied. Participants were instructed to reach as far as possible under the corresponding condition, and the repetition ended when they could not further increase the movement (each ROM was made by three consecutive repetitions). We have clarified this in the Methods and Supplementary Methods, and we also note that the effective reachable workspace may vary with anthropometry and fit, because device placement and snugness influence how assistance is transmitted.

6) The interpretation of the functional movement analysis, particularly Figure 6C, can be revisited. This reviewer was not fully convinced by the current interpretation. The manuscript appears to suggest that SPARC deteriorated mainly in the incomplete-lesion participant while being preserved in others. However, my reading of the figure suggests that the head-related task may have deteriorated more broadly across participants. Participant ID12 also seems to show deterioration in the water and block tasks. In addition, the participant IDs shown across panels appear inconsistent (Figure 6AB). The head task also appears to involve a longer delay (Figure 6A), which may deserve further

discussion. I encourage the authors to revisit this analysis and provide a more careful, task-specific interpretation.

[Answer 1.16]

We thank the reviewer for the meaningful comment. Please, consider our answer 1.7.

7) I agree that multi-joint assistance is a potentially important contribution of this work. However, the manuscript could explain more clearly why shoulder assistance is clinically important in this population and how it contributes beyond elbow support alone. For several of the tasks shown, elbow function appears to be the dominant contributor, except perhaps for the head-related task. The authors should discuss the clinical meaning of shoulder assistance, particularly in relation to proximal control and compensation patterns.

[Answer 1.17]

We thank the reviewer for highlighting this important clinical aspect. Shoulder assistance is indeed clinically critical in BPI because proximal deficits severely limit functional reach and contribute to compensatory trunk/scapular overuse, which exacerbates pain, subluxation risk, and fatigue (supported by current refs: 29-31). Even when elbow/hand function is relatively preserved post-nerve transfer, insufficient shoulder elevation restricts workspace and ADL performance (e.g., feeding, reaching overhead). We have expanded the Discussion to clarify this and explain how shoulder support augments reach while reducing compensatory patterns.

8) The discussion presents transparency as an important advantage of the system. However, for the endurance and ADL tasks, the comparisons appear to have been made between powered and unpowered device conditions, rather than between powered assistance and a true no-device condition. Therefore, strong claims about transparency may not yet be fully supported by the current evidence. The relevant discussion should either be moderated or supported with additional data.

[Answer 1.18]

We thank the reviewer for this observation. Please, consider our answer 1.8.

* Minor comments

1) The user-reported outcomes are a strength of the manuscript. It would help to connect these more directly to the objective findings, such as reduced fatigue, improved

endurance, and increased movement capacity, especially in relation to perceived efficacy and satisfaction.

[Answer 1.19]

We thank the reviewer for this helpful suggestion. We have revised the Discussion to more directly link the user-reported outcomes with the objective performance measures. In particular, we now explicitly relate the positive agency, satisfaction, and perceived efficacy ratings to the observed reductions in effort and fatigue, increased endurance, and enlarged movement capacity during assistance. This better highlights how the subjective experience of the device aligns with the objective functional gains.

2) Because weight appears to have emerged as a relevant factor in user feedback, it would be useful to relate this point to the current design and discuss whether weight may influence usability, comfort, or performance.

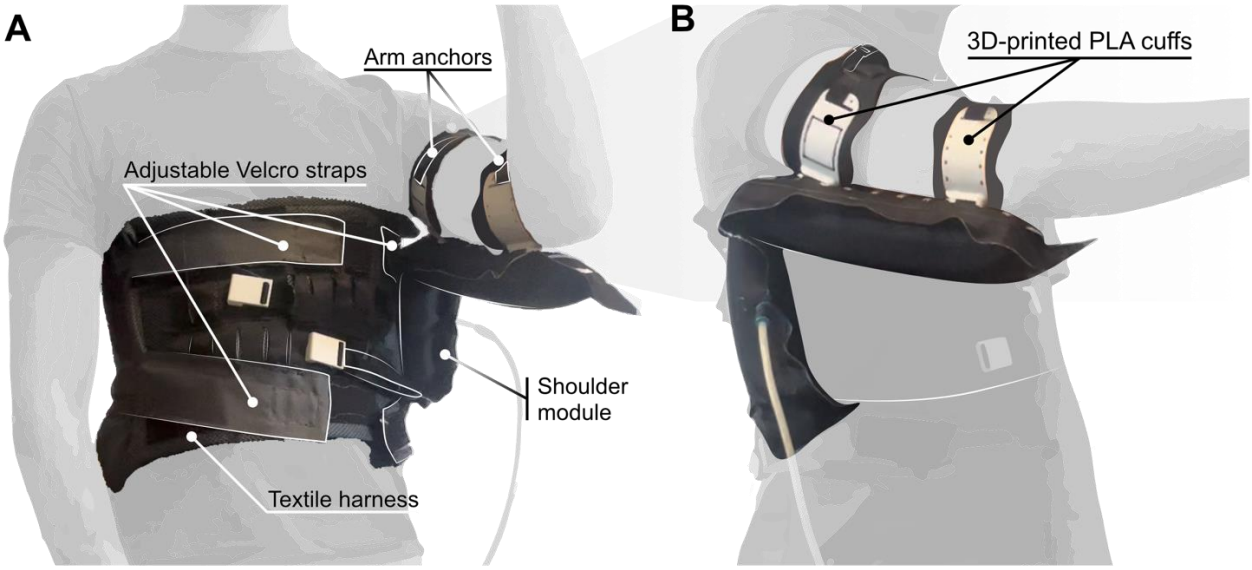
[Answer 1.20]

We thank the reviewer for this insightful observation. We have expanded the Discussion to explicitly relate the high QUEST weight rating to the current soft exosuit design. We also discuss design implications for independent home use.

3) The manuscript discusses limitations related to the human interface. A figure or schematic of the current interface design would help readers understand these limitations more clearly.

[Answer 1.21]

We thank the reviewer for this helpful suggestion. We have added a new Supplementary Fig. S4 (attached here) that provides a clearer visualization of the current human-robot interface. Specifically, it illustrates the custom textile harness used to anchor the shoulder module at the level of the trunk, as well as the 3D-printed cuffs to interface with the upper arm.



[NEW] Supplementary Figure S4: Human-robot interface of the shoulder module. (A) Overview of the interface design, showing the shoulder module connected to a custom textile harness worn on the trunk, featuring adjustable Velcro straps for fit and load distribution. The module is anchored to the upper arm through dedicated arm anchors. (B) Detailed view of the 3D-printed arm cuffs used as rigid anchors to interface with the upper arm. The cuffs incorporate an integrated neoprene lining, ensuring safe and comfortable contact with the skin. While effective in enabling load transfer, these cuffs represent a critical rigid interconnection within an otherwise compliant system and were occasionally prone to mechanical failure during testing. This highlights the need for further design refinement of this component in future iterations.

4) In the limitation paragraph in the discussion, the fact that the everyday task-related outcomes were evaluated in only 4 participants should be noted.

[Answer 1.22]

We thank the reviewer and we acknowledge this clear limitation.

5) A table listing key outcomes for each participant would help readers understand individual variability, especially given the heterogeneity of the cohort.

[Answer 1.23]

We thank the reviewer for the suggestion. Please, consider our answer 1.13.

6) When making interpretive claims in the Discussion, it would be helpful for the readers if the authors cite the corresponding figures or result sections more explicitly.

[Answer 1.24]

We thank the reviewer and modified the discussion accordingly.

7) A supplementary section describing the design, control, intention-detection method, personalization procedure, and subject-specific pressure setting in more detail would make the contribution easier to understand and assess.

[Answer 1.25]

We thank the reviewer and this is addressed in previous answer 1.9.

Reviewer #2

This manuscript evaluates a multi-joint soft exosuit providing pneumatic assistance at the shoulder (gravity compensation) and optionally at the elbow (triggered assistance), controlled via IMU-derived kinematics. In a single-session, within-subject protocol across baseline, robot-off, and robot-on conditions for dynamic range of motion (ROM), and robot-off versus robot-on conditions for the static holding endurance task, 14 adults with chronic traumatic brachial plexus injury (BPI) show substantial immediate improvements in proximal active range of motion and static endurance, accompanied by reductions in perceived exertion and sEMG amplitude. User-reported agency and satisfaction are generally positive. In a small subset of participants with sufficient hand functon(n = 4), ARAT scores improve modestly and simulated activities of daily living appear qualitatively less compensatory.

Overall, the study is well designed for an initial clinical feasibility demonstration in a challenging and under-studied population. The inclusion of a robot-off condition is a notable strength, allowing the authors to separate active assistance effects from passive mechanical constraints. The convergence of kinematic, physiological, and subjective outcome measures provides convincing evidence that the device can provide immediate assistance in gravity-limited tasks. However, several conclusions are currently stronger than the evidence supports, and important methodological details required for reproducibility and the assessment of potential bias should be clarified.

[Answer 2.1]

We thank the reviewer for the positive feedback and for helping us in making our submission better and more complete.

Major comments

1. Functional impact is currently supported by limited quantitative evidence
The main functional endpoint (ARAT) is reported for only four participants, and baseline ARAT scores are already high (47–51/57), suggesting possible ceiling effects. The mean improvement (+4.5 points) is therefore difficult to interpret as broadly “clinically meaningful” (also in the absence of an established MCID for this population). The simulated ADL results are qualitative. Claims regarding clinical or functional impact should therefore be tempered and framed primarily as evidence of immediate assistive feasibility. Please also clarify which comparison condition is used for ARAT (baseline vs robot-off), as the text and figure captions appear inconsistent.

[Answer 2.2]

We thank the reviewer for this careful assessment of the functional data limitations. We fully agree that the ARAT results represent proof-of-concept rather than broadly generalizable clinical impact. We have tempered all claims to frame these as "evidence of immediate assistive feasibility in participants with sufficient hand function" rather than definitive clinical impact. We also clarified that ARAT comparisons were consistently robot-off vs robot-on (both device worn), as shown in Fig. 6B and now explicitly stated in Results and figure caption.

2. Interpretation should reflect the single-session design. The study demonstrates immediate assistance effects only. It does not address learning, day-to-day variability, longer-term adaptation, or potential changes in compensatory strategies with repeated use. While this limitation is acknowledged in the manuscript, the conclusions and abstract should reflect it more clearly.

[Answer 2.3]

We thank the reviewer for this important remark. We agree that the study should be interpreted as a single-session demonstration of immediate assistive effects only, and not as evidence of learning, day-to-day adaptation, or longer-term rehabilitative change. To make this scope explicit, we revised the Abstract, Discussion, and Conclusions to state that the present work evaluates immediate assistance during device use, while longer-term effects with repeated use remain to be tested in future longitudinal studies.

3. Condition ordering and potential expectancy effects. The manuscript does not clearly state whether the order of experimental conditions was randomized or counterbalanced for the ROM and endurance tasks. Because the robot-on condition is inherently unblinded, subjective measures such as Borg ratings may be susceptible to expectancy effects. The authors should clarify condition ordering and discuss this limitation.

[Answer 2.4]

We thank the reviewer for raising this important methodological point. The ROM task was performed in a quasi-randomized order: first, always the baseline (as the robot was not worn in this condition), then randomized robot off or robot on conditions. The endurance task was also randomized in order (either first robot off or robot on) but came always after the ROM task. We have clarified this in the Methods and added a limitation noting that subjective ratings such as Borg may be influenced by expectancy effects because the robot-on condition cannot be blinded. This does not alter the primary objective findings, but it is now explicitly acknowledged in the manuscript.

4. Control and intention-detection methods require clearer description. The overall control framework is understandable, but key implementation details are missing. In particular,

the elbow “volitional movement detection” used to trigger assistance is not sufficiently specified (signal features, thresholds, filtering, hysteresis, inflation/deflation logic). Providing these details is important for reproducibility and for evaluating robustness in compensation-rich movement patterns typical of BPI.

[Answer 2.5]

We thank the reviewer for highlighting the need for clearer control framework details. This concern has been comprehensively addressed in our responses to Reviewer #1 answer 1.9 (where we added Supplementary Methods, Fig. S1 (control architecture), and Fig. S2 (subject-based calibration) detailing the intention-detection strategy, volitional movement triggers, tuning procedures, and hardware specifications), and answer 1.14, where we added Fig. S3, which provides a quantitative temporal performance description of both actuator dynamics and controller activation following the wearer’s movement onset.

5. Design considerations for the BPI population. The manuscript emphasizes that this work represents a clinical translation of previously developed soft-assistive modules to a biomechanically and clinically distinct population. While several BPI-specific challenges are discussed, the manuscript would benefit from a clearer description of how these considerations informed the hardware and interface design, especially the redesign of the shoulder module. For example, it would be useful to briefly summarize which aspects of the device architecture or control strategy were specifically adapted for BPI compared with prior ALS or SCI deployments. Making this design rationale more explicit would help clarify the novelty and translational contribution of the work.

[Answer 2.6]

We thank the reviewer for this thoughtful comment. We agree that the device was not designed de novo specifically for BPI; rather, this study represents a translational refinement of our previously developed soft shoulder module, adapted after prior testing in healthy and neurological cohorts. The most relevant design change is that the updated shoulder module shifts the assisted direction from predominantly shoulder abduction in the previous version to shoulder flexion in the sagittal plane, which is more functionally relevant for forward reach and hand positioning in BPI. In addition, the anchoring and load-transfer geometry were redesigned to improve comfort and reduce trunk compression, while preserving the same low-level kinematics-based control principle. We have revised the manuscript to make this translational rationale explicit and to clarify that the novelty lies in evaluating this redesigned module in a clinically distinct BPI population.

6. Interpretation of EMG results. EMG amplitude is normalized to the maximum value observed across all trials/conditions rather than to a standardized MVC. This approach is

understandable in an impaired cohort but should be discussed more explicitly, as it may influence cross-condition comparisons. Additionally, the conclusion that unchanged agonist EMG indicates preserved voluntary engagement appears somewhat stronger than the data alone support.

[Answer 2.7]

We thank the reviewer for this important methodological point. Because standardized MVCs were not always reliable or feasible in this impaired cohort, EMG amplitude was normalized to the maximum value observed across all trials and conditions within the session, allowing consistent within-participant comparison while avoiding dependence on a potentially weak or inconsistent MVC reference. We have clarified this normalization choice in the Methods and toned down the interpretation of unchanged agonist EMG: rather than claiming that it proves preserved voluntary engagement, we now state more cautiously that the EMG data are consistent with assistance-induced kinematic gains occurring without obvious increases in agonist activation.

Additional comments:

- The baseline / robot-off / robot-on design provides good evidence of mechanical transparency during dynamic ROM tasks. However, the endurance task only compares robot-off and robot-on conditions; therefore passive transparency during sustained holding cannot be assessed in the same way and should not be generalized too broadly.

[Answer 2.8]

We thank the reviewer for this precise observation. We agree that while dynamic mechanical transparency is directly demonstrated in the ROM task (baseline vs robot-off, Fig. 2A), sustained transparency during static holding was not directly assessed against a true no-device condition in the endurance task. We have added a clarification in the Discussion.

- Elbow outcomes are reported for a subset of participants (n = 8); this should be clearly and consistently indicated across text, figures, and statistical reporting.

[Answer 2.9]

We thank the reviewer and now explicitly report number of participants for each result, across text, figures and statistics.

- The manuscript notes exclusion of EMG channels due to artifacts or very low signal amplitude. Please report the exact per-muscle sample sizes for each analysis.

[Answer 2.10]

We thank the reviewer and now explicitly report number of participants for each result, across text, figures and statistics.

- The multiple-comparison correction strategy should be clarified.

[Answer 2.11]

We thank the reviewer for this suggestion. We have clarified the multiple-comparison strategy in the Methods: omnibus Friedman tests were used for outcomes with three conditions, followed by pairwise Wilcoxon signed-rank tests when appropriate, and all post-hoc p-values were corrected within each outcome family using the Benjamini-Hochberg false discovery rate procedure. For the EMG analyses, correction was applied across the set of muscle-wise comparisons within each task, and significance is now consistently reported as FDR-adjusted p-values.

- The Methods indicate that the system is actuated by an offboard controller. Additional description of the tethered configuration (e.g., tubing/cabling arrangement, pump type and location, noise, and constraints on movement) would help readers interpret translational claims.

[Answer 2.12]

We thank the reviewer. We previously addressed this in comment #4.

- A brief explicit statement regarding adverse events or tolerability (e.g., discomfort, pain exacerbation, skin irritation) would strengthen the feasibility framing.

[Answer 2.13]

We thank the reviewer for this helpful suggestion. We have added an explicit tolerability statement to the manuscript noting that no adverse events occurred during testing and that participants did not report discomfort, pain exacerbation, or skin irritation. This further supports the feasibility and short-term safety of the exosuit in this cohort.

- There are minor internal inconsistencies that should be corrected, including the response scale reported for the sense-of-agency questionnaire (1–5 vs 1–7) and some figure cross-references in the user-experience section.

[Answer 2.14]

We thank the reviewer for noticing these inconsistencies. We have corrected the response scale reported for the sense-of-agency questionnaire to match the actual 1–5 Likert scale used in the study, and we have revised the figure cross-references in the user-experience section for consistency.

Reviewer #1

The revised manuscript is substantially improved. The authors have clarified the intended scope of the study, added methodological details, provided participant-level outcome information, and better positioned the work as an early translational assistive-device study rather than a rehabilitation or long-term efficacy trial. The revised framing, namely that the exosuit provides immediate assistive support during use, is appropriate.

The main contribution of the work is the early clinical translation of a multi-joint soft exosuit to individuals with chronic traumatic brachial plexus injury, a heterogeneous population with substantial proximal weakness and compensatory movement strategies. In contrast to prior devices that have largely focused on isolated elbow assistance or distal hand function, this system targets the proximal shoulder–elbow chain, which is critical for functional reach. The study demonstrates immediate improvements, particularly in active ROM and endurance.

The revisions also help clarify that the study evaluates the device as an assistive technology, not as a rehabilitation intervention. This distinction is important. The current results support immediate within-session benefit while the device is worn and powered, but they do not establish long-term therapeutic effects, carry-over after device removal, or home-use feasibility.

We thank the reviewer for appreciating our effort and the positive feedback, and for helping us improve the quality of our contribution thanks to their original comments.

Several points still require attention before the manuscript is fully clear.

The participant subgroup definitions still need clarification. The manuscript states that shoulder outcomes were evaluated in all 14 participants, whereas elbow outcomes were assessed only in 8 participants because the remaining 6 had near-physiological elbow control. This rationale is reasonable, but the manuscript should provide the baseline elbow ROM values used to define the elbow subgroup.

We thank the reviewer for this important suggestion. We now include a new Supplementary Table S1 reporting baseline shoulder and elbow data for all 14 participants. This table makes explicit the baseline elbow ROM values used to define the elbow-assistance subgroup. We also clarified in the Supplementary Table S1 caption that participants with near-physiological elbow ROM (>150°) were not tested with the elbow module and therefore only used the shoulder assistance during the experimental protocol.

In addition, the ARAT/ADL subgroup should be clearly defined as a separate subgroup selected based on sufficient residual hand function. The current supplementary caption

appears to state that the ARAT/ADL participants were a subset of the elbow group, but the participant IDs in Table S1 appear inconsistent with that statement. This should be corrected.

We thank the reviewer for noticing this inconsistency. We corrected the text to clearly define the ARAT/ADL subgroup as a separate subset of 4 participants selected on the basis of sufficient residual hand function to manipulate objects.

The functional relevance claims should remain cautious. The ARAT and simulated ADL results are encouraging, but they are based on only four participants. Although the authors have softened the wording in several places, this reviewer recommends checking the manuscript again to ensure that these findings are consistently framed as proof-of-concept evidence that the device may support selected functional tasks in participants with sufficient residual hand function, rather than as evidence of generalizable daily-life benefit across the full BPI cohort.

We thank the reviewer and revised the Abstract, Results, Discussion, and Conclusions to more consistently frame the ARAT and simulated ADL findings as proof-of-concept evidence obtained in only 4 participants with sufficient residual hand function.

The portability and real-world translation should be described more precisely. The wearable portion appears lightweight, but the pneumatic control unit is off-board and benchtop-mounted. Thus, the current system is wearable but not fully portable or independent for daily use. The authors appropriately mention future onboard actuation, but the manuscript should avoid overstating feasibility for home or community use until untethered operation, independent donning/doffing, and longer-term usability are evaluated.

We agree with the reviewer and revised the manuscript to more precisely describe the current translational stage of the system. We toned down statements regarding home and community use, framing these as future directions that will require onboard actuation, independent donning/doffing, and longer-term usability testing.

The added methodology improves transparency, especially regarding IMU-based intention detection, subject-specific tuning, pneumatic control, and pressure regulation. However, full reproducibility would still require more explicit details about calibration decision rules, pressure-map interpolation, filtering, controller gains, and actuator output characterization. If these details cannot be included, the authors should state that the added methods improve methodological transparency rather than ensuring full reproducibility.

We thank the reviewer. While figure S2 already shows the main calibration decision rules, including pressure-map interpolations, we made the control firmware openly available as supplementary material/repository (https://doi.org/10.5281/zenodo.20442078) to further facilitate methodological transparency and reuse.

Overall, the revised manuscript is stronger, and the study makes a meaningful early translational contribution. The work is best viewed as a proof-of-concept clinical demonstration of immediate assistive benefit from multi-joint soft robotic support in chronic BPI. With clearer subgroup definitions, more cautious functional claims, and a sharper statement of the main contribution, the manuscript would be substantially improved.

We really thank the reviewer for their effort in helping us make this work better and more carefully framed within the literature of assistive soft robotics.

Reviewer #2

The authors have responded thoughtfully to the comments of both reviewers, and the manuscript has improved substantially in clarity, balance, and methodological detail. I believe the revised version is considerably strengthened.

We thank the reviewer for appreciating our effort and the positive feedback, and for helping us improve the quality of our contribution thanks to their original comments.

A few points, however, may still benefit from clarification or minor further revision:

First, it would be helpful if the authors could clarify how assistance is terminated or reduced once IMU-based initiation has occurred, particularly for the elbow triggered-passive mode. While the revised manuscript explains initiation more clearly, the stop or deactivation logic remains somewhat difficult to follow. The authors state that "Movement termination was manually defined via the GUI.", but it remains unclear how this would translate to a scenario where ADL that require continuously varying elbow angles are supported.

We agree with the reviewer that the previous wording was not sufficiently clear. We have now explicitly stated in the Supplementary Methods that, in the present implementation, the actuators were manually vented via the graphical user interface once the movement/repetition was completed. We also clarify that this strategy was acceptable for the controlled experimental setting of the present study, but is not yet suitable for continuously varying real-life ADLs, particularly for the elbow triggered-passive mode, and therefore represents an important target for future controller development.

Second, given that functional ADL testing was feasible in only 4 participants, it may be worth noting in the Discussion that integration with a hand-assistance module could represent an important next step for improving broader functional relevance.

We thank the reviewer for this valuable suggestion and added this point to the Discussion. Specifically, we now note that integrating the current proximal-support exosuit with a hand-assistance module could represent an important next step to broaden functional relevance, especially given that ARAT/ADL testing was feasible only in the subset of participants with sufficient residual hand function.

Third, the expression “clinically meaningful” still appears several times throughout the

manuscript; in the opinion of this reviewer, that wording would be better moderated, particularly in the absence of an established MCID for the ARAT in this population.

We thank the reviewer and revised the manuscript to moderate this wording throughout. In particular, we removed the expression “clinically meaningful” and replaced it with more neutral wording focused on immediate assistive effects observed within-session, without implying established clinical significance or minimum clinically important differences in this population.

Fourth, I was not sure that my earlier comment regarding noise from the pneumatic pump and valves was fully addressed. In a clinical or at-home setting, pump noise could affect usability and acceptability, and it may therefore be useful to discuss this point briefly or acknowledge it as a limitation.

We thank the reviewer for raising this important point. Noise is a relevant consideration for pneumatic systems in general, and we agree that it may affect usability and acceptability in clinical or home settings. In our case, the system produced an ambient noise level of 52 dB and reached peaks of 57.5 dB when the pumps were active, which is comparable to common household appliances. During the experimental sessions, no participants or clinicians reported noise-related discomfort or concerns.

Finally, the newly added tracked text would benefit from a careful grammar and style check before final acceptance.

We thank the reviewer for this suggestion. We carefully revised the newly added tracked text, as well as the surrounding sections of the manuscript and supplementary materials, to improve grammar, syntax, and style consistency throughout.

Dear Editor,

Please find attached the additional requested point-by-point response.

Please note that the revisions requested by the two Reviewers in the last round of review (and addressed in the attached response) had already been incorporated into the latest version of the manuscript that was previously uploaded. Therefore, no further changes related to those comments were made in the manuscript at this stage.

Best regards,

Tommaso Proietti

*** Please provide a point-by-point response to the last reviewer reports.**

Reviewer #1 (Remarks to the Author):

The revised manuscript is improved. The authors have clarified the scope of the study, added useful methodological and participant-level details, and more appropriately framed the work as an early translational assistive-device study rather than a rehabilitation or long-term efficacy trial.

The study's main contribution is the early clinical demonstration of a multi-joint soft exosuit for individuals with chronic traumatic brachial plexus injury. The focus on the proximal shoulder–elbow chain is clinically relevant, and the immediate improvements in active range of motion and endurance are encouraging.

The authors have addressed several prior concerns. The subgroup definitions are now clearer, including the distinction between shoulder-only participants, participants tested with the elbow module, and the separate ARAT/ADL subgroup selected based on sufficient residual hand function. Supplementary Table S1 is helpful in this regard.

I also appreciate the more cautious framing of the ARAT and simulated ADL findings. Because these results are based on only four participants, they should continue to be presented as proof-of-concept evidence in selected participants rather than as generalizable evidence of daily-life benefit.

The discussion of portability and real-world translation is also improved. Since the current system still relies on an off-board pneumatic control unit, claims regarding home or community feasibility should remain cautious until untethered operation, independent donning/doffing, and longer-term usability are evaluated.

Overall, the revised manuscript is clearer and more appropriately framed as an early proof-of-concept demonstration of immediate assistive benefit from multi-joint soft robotic support in chronic traumatic brachial plexus injury.

We thank the Reviewer for appreciating our effort in following their suggestions and making our contribution stronger and clearer.

Reviewer #2 (Remarks to the Author):

The authors have further improved the manuscript considering the comments of both

reviewers. This is now a strong proof-of-concept study supporting the feasibility and immediate assistive effects of an exosuit to support proximal arm function in BPI.

Only few points remain to be addressed:

1) The wording regarding clinical significance is now appropriately stated as proof of concept evidence and clinical feasibility evaluation. However, there is still a statement about “clinical translation” in the introduction which should be adapted to reflect proof-of-concept/feasibility evaluation (“Therefore, this new clinical translation…”). Following the same reasoning, in the same sentence, the word “establishing” should be replaced with “testing” or “examining”).

We thank the Reviewer for appreciating our efforts. We updated the residual statement following their suggestion.

Old sentence: Therefore, this new clinical translation extends prior soft-assistive concepts to a biomechanically and clinically distinct population with heterogeneous motor patterns and heavy reliance on compensatory strategies, establishing whether gravity-compensating parallel assistance remains effective in the presence of altered movement solutions.

New sentence: Therefore, this clinical translation extends prior soft-assistive concepts to a biomechanically and clinically distinct population with heterogeneous motor patterns and heavy reliance on compensatory strategies, allowing assessment of whether gravity-compensating parallel assistance remains effective in the presence of altered movement solutions.

2) The authors now mention the manual venting of the actuators after movement completion in the supplemental material. However, this should already be mentioned in the methods.

The sentence “The actuators were manually vented via the graphical user interface once each movement/repetition was completed” is now present in the Methods, section Robot Design and Control.

3) Line 432: There is an issue with the added sentence, which is currently incomplete (starting with “are encouraging for future...”).

We thank the Reviewer for spotting this error, that we fixed as follow:

Old version: The offboard pneumatic control unit (2.1 kg) was not worn but remained benchtop-mounted, further minimizing ambulatory burden. are encouraging for future

translation beyond the laboratory; however, the current system remains tethered to an off-board benchtop control unit and is not yet suitable for independent home use.

New version: The offboard pneumatic control unit (2.1 kg) was not worn but remained benchtop-mounted, further minimizing ambulatory burden. These weight-related strengths are encouraging for future translation beyond the laboratory; however, the current system remains tethered to an off-board benchtop control unit and is not yet suitable for independent home use.

*** Please reference all figures in the main text.**

We included explicit reference to Fig. 4A (line 158), 4B (160), while reference to the general Fig. 4 was already existing at line 132. All the other figures are already referenced in the current text (including the four supplementary ones).

*** Please remove the columns for age, sex, height and weight from Table 1, since this information is also provided in aggregate in the "Testing Population" section of the Methods.**

Done.

*** In figure legends, please designate the n number (i.e. the sample size), using the wording "n=X samples/repetitions/independent experiments", clearly indicating what n corresponds to. This information is missing in Supplementary Figure S3, for example.**

Thanks, we added the N information in Fig. S3. Everywhere else we believe to have already followed the suggested policy.

*** Please clarify how Supplementary Figure S1 was generated.**

Fig. S1 was hand-drawn with open-source vectorial software (Inkscape) from a picture of a real patient (you can see similarities with the real pics in Fig. 6). The face in Fig. S1 was invented. However, to remove the even potential residual doubt of a similar face with a real patient, we blurred the illustrated face in Fig. S1. The block scheme was then added using Microsoft Powerpoint and LaTeX for the formulas.

*** Please provide all exact p-values (even if not statistically different) in the source data file.**

Done.

*** With regards to the previous point #5, please simply confirm in the Methods section of the manuscript that you have obtained consent from all participants and research team members to publish their images in photos and videos. Additionally, please send us these documents in Italian. We'll keep them with our records, but they will not be published.**

Sentence added (lines 401-402). Regarding the consents, thanks for understanding our position.