

Dear Author(s),

thank you very much for submitting your manuscript. The paper addresses a relevant and timely research problem at the intersection of wellness tourism, hospitality management, sport-based health promotion, guest experience, loyalty formation, and health-related behavioral intentions. The central idea of examining sport-based wellness programs in Saudi Arabian hotels as more than a mere recreational service is promising. In particular, the attempt to connect wellness program quality, perceived health benefits, enjoyment, staff health communication, guest satisfaction, loyalty intentions, and health behavior change within one structural model is potentially valuable for both hospitality research and applied sport/health science.

First and foremost, it should be acknowledged positively that the manuscript does not reduce hotel wellness programs to a simple service-quality issue. Rather, it tries to reconstruct them as experience-related and health-oriented micro-settings in which service design, instructor communication, affective experience, and behavioral intentions are theoretically interlinked. This is a relevant perspective, especially in view of the growing role of wellness tourism and the strategic importance of health-oriented hospitality services in emerging tourism destinations.

Nevertheless, the manuscript in its current form requires substantial revision. The main weaknesses lie less in the general relevance of the topic or in the basic plausibility of the proposed model but primarily in the conceptual precision of the research contribution, the methodological transparency of the empirical design, the operationalization of central constructs, the interpretation of cross-sectional PLS-SEM findings, and the tendency to formulate causal, public-health, and policy-related conclusions more strongly than the data allow.

## **Major Comments**

### **1. The research contribution needs to be defined more precisely.**

However, the introduction covers a broad range of literature, including wellness tourism, service quality, sport participation, health behavior, guest experience, co-creation, sustainability, SDGs, Vision 2030, etc. While these fields are individually relevant, the manuscript currently integrates them in a rather expansive manner. As a result, the specific originality of the paper remains blurred.

It should be made clearer whether the main contribution lies in applying service-quality theory to sport-based hotel wellness programs, in extending wellness tourism research through health behavior constructs, in introducing staff health communication as a moderator, or in positioning Saudi Arabian hotels as a specific empirical context. These levels are present in the manuscript, but they should be analytically separated more clearly.

Against this background, I recommend restructuring the introduction according to a clearer research logic: state of research—identified gap—theoretical intervention—empirical model—expected contribution. This would help the reader understand what is genuinely new about the study and what is an application of already established satisfaction and loyalty models to a new setting.

## **2. The theoretical framework is relevant but conceptually overloaded.**

The manuscript draws simultaneously on service quality, health belief logic, motor learning, sport pedagogy, guest experience, co-creation, sustainability, SDGs, and Vision 2030. This breadth is intellectually ambitious, but in the current version it sometimes leads to conceptual overextension. Not every theoretical reference is equally necessary for testing the proposed model.

The authors should clarify which theoretical framework actually carries the structural model. For example, service quality theory appears central for WSPQ, expectancy-disconfirmation theory for satisfaction, health belief/self-efficacy logic for perceived health benefits, and affective-motivational approaches for enjoyment. By contrast, ecological dynamics, SDGs, and Vision 2030 are currently used more as contextual and interpretive layers than as directly operationalized theoretical components.

It would therefore be useful to distinguish between (a) core model theory, (b) contextual justification, and (c) broader policy implications. This would make the paper more rigorous and prevent the argument from appearing rhetorically overextended.

## **3. The operationalization of key constructs must be strengthened.**

A central issue concerns the measurement of the constructs. The manuscript states that all constructs were measured using multi-item Likert-type scales adapted from validated instruments. In this regard, the actual item wording is not sufficiently transparent. Without the exact items, it is difficult to assess whether the constructs are validly captured.

This is particularly important for the quality of wellness sport programs, staff health communication, and health behavior change. WSPQ appears to combine service quality, instructor competence, facility quality, responsiveness, and perhaps pedagogical quality. However, it remains unclear whether the items truly capture sport-pedagogical quality or mainly general hotel/service quality. Similarly, staff health communication is theoretically interpreted as applied coaching and health communication competence, but empirical indicators must show whether this construct actually measures health-oriented guidance rather than general friendliness or service interaction.

The greatest concern relates to health behavior change. A cross-sectional, self-report survey can capture perceived change, intention to change, or self-reported behavioral orientation, but it cannot robustly establish actual behavioral change over time. The construct should therefore be renamed or more cautiously framed, for example as “self-reported health behavior intention” or “perceived post-stay health behavior orientation,” unless longitudinal or objective behavioral data are available.

## **4. Sampling and field access need to be described much more transparently.**

The manuscript reports that 412 guests participated in structured sport-based wellness programs in Saudi Arabian hotels. However, the sampling process remains insufficiently detailed. The authors should specify:

- which hotel categories were included;
- How many hotels participated?
- in which regions or cities the data were collected;
- how guests were approached;
- How many guests declined participation?
- whether the sample was convenience-based, purposive, quota-based, or otherwise structured;
- what period of data collection was covered;
- whether domestic and international guests were differentiated;
- and whether relevant demographic or travel-related characteristics were controlled.

This is not merely a formal issue. Since the paper makes claims about Saudi hotels, wellness tourism, loyalty intentions, and health behavior change, the reader must be able to assess the empirical scope of the sample. A transparent sampling table would be very advantageous.

## **5. The cross-sectional design limits causal and behavioral claims.**

At this point, the authors correctly acknowledge limitations in the limitation section, but they must consistently integrate this caution throughout the manuscript. The current wording repeatedly suggests causal effects, such as program quality shaping health behavior change, staff communication amplifying health gains, or wellness programs functioning as public-health micro-settings.

A cross-sectional PLS-SEM design can support theoretically plausible associations between latent constructs. It cannot, however, demonstrate actual health behavior change, long-term lifestyle transformation, or causal public-health effects. The authors should therefore revise causal formulations throughout the abstract, discussion, implications, and conclusion.

In particular, the paper should distinguish between the following:

- association and causation;
- perceived health benefits and objective health benefits;
- self-reported intention and actual behavior change;
- short-term guest experience and long-term lifestyle transformation.

This revision would substantially improve the scientific credibility of the study.

## **6. The PLS-SEM reporting requires inductive and methodological refinement.**

The manuscript presents reliability, AVE, factor loadings, Fornell-Larcker, HTMT,  $R^2$ ,  $Q^2$ , SRMR, NFI,  $f^2$ , mediation, and moderation results. This is generally appropriate for a PLS-SEM study. However, several reporting aspects should be improved.

The authors should report the full measurement model more transparently, including all items, item sources, adaptation procedure, translation/back-translation if applicable, and pretest or pilot procedures. Moreover, the treatment of common method bias should not rely too

strongly on Harman's single-factor test, which is widely regarded as insufficient as a sole diagnostic. The manuscript should also explain whether the constructs were modeled reflectively or formatively and justify this decision theoretically.

The statement that all inter-construct correlations were below a certain value appears inconsistent with the table values and should be checked carefully. In addition, confidence intervals should be reported consistently for direct, indirect, and moderating effects. The moderation plot should be explained more clearly, including how high and low levels of staff health communication were defined.

## **7. Results, interpretation, and implications should be more clearly separated.**

Inductively, the results section is generally understandable, but the manuscript quickly moves from statistical support to broad theoretical and practical claims. Furthermore, the authors should make a clearer distinction between empirical results, interpretation, and normative or managerial conclusions.

For example, the finding that WSPQ predicts perceived health benefits and enjoyment is an empirical result. In this regard, the statement that hotels become sport-for-health micro-settings is an interpretation. Also, the recommendation that hotel managers should integrate CRM systems, AI-driven personalization, and policy alignment with Vision 2030 is a practical or strategic implication. These levels are related, but they should not be presented as if they were all directly demonstrated by the data.

At this point, a more evidence-based discussion structure would be beneficial:

Result 1: WSPQ predicts satisfaction, perceived benefits, and enjoyment.

Interpretation: quality appears to operate through cognitive and affective pathways.

Boundary: This result refers to perceived guest evaluations, not objective health outcomes.

Implication: Hotel wellness programs should focus on both service quality and affective experiences.

## **8. The Saudi Arabia and Vision 2030 context needs stronger empirical anchoring.**

Here, the Saudi context is important and potentially valuable. However, at present it sometimes functions more as a contextual frame than as an empirically analyzed dimension. If the paper claims specific relevance to Saudi Arabian hotels and Vision 2030, the manuscript should more clearly show what is specific about this context.

The authors should explain whether and how cultural expectations, gender norms, family travel patterns, religious or social norms, domestic versus international tourism, luxury hotel positioning, or national tourism strategies shape participation in hotel-based sport wellness programs. Otherwise, the Saudi context risks remaining a geographic label rather than an analytically integrated part of the study.

## **9. The discussion of sustainability and SDGs should be made more cautious.**

The connection between wellness tourism and SDG 3 per se is plausible. However, claims regarding SDG 8, SDG 11, and SDG 12 are currently too broad in relation to the empirical design. The study did not measure employment quality, urban sustainability, community infrastructure, responsible consumption, environmental effects, or destination-level sustainability outcomes.

Therefore, the SDG discussion should either be shortened and framed as contextual relevance or supported with additional empirical or documentary evidence. Otherwise, there is a risk that the sustainability section appears rhetorically attractive but insufficiently substantiated.

#### **10. Language, style, and formal precision require revision.**

Consequently, the English language is generally understandable, but the manuscript would benefit from thorough linguistic and stylistic revision. Several formulations are grammatically imprecise or overly rhetorical. Examples include awkward phrases such as “which should be” and “allowing to assume,” inconsistent abbreviation formatting, inconsistent capitalization, and imprecise expressions such as “wellness arena,” “robust predictive capacity,” or “strategic levers” where the data support more cautious claims.

In addition, the manuscript contains formatting inconsistencies in citations, parentheses, table captions, figure captions, hyphenation, capitalization, and reference style. The title and figure captions should also be carefully checked.

#### **Minor Comments**

- Please provide the exact measurement items, preferably in an appendix.
- Please clarify whether the survey was conducted in English, Arabic, or both, and describe translation/back-translation procedures if applicable.
- Please report demographic characteristics of the sample in a dedicated table.
- Please verify all citations and reference-list entries for completeness, consistency, and accuracy.
- Please avoid overclaiming “health behavior change” unless longitudinal evidence is provided.
- Please revise the term “co-created guest experience” so that it is theoretically and empirically visible in the model, not only in the title and discussion.
- Please improve the readability and formatting of Figures 1–3.
- Please ensure that abbreviations such as WSPQ, PHB, ENR, GSAT, LOY, HBC, and SHC are introduced consistently.

#### **Overall Assessment**

Overall, the manuscript addresses a relevant topic and has the potential to contribute to the discussion on wellness tourism, hotel-based sport services, guest experience, and health-oriented hospitality management. The proposed model is plausible, and the empirical sample appears potentially useful. However, the paper requires substantial revision before it can be considered for publication.

In particular, the authors should sharpen the research contribution, reduce conceptual overextension, disclose the measurement model more transparently, provide more detailed sampling information, interpret the cross-sectional PLS-SEM results more cautiously, avoid unsupported causal and public-health claims, and substantially improve linguistic and formal consistency.