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Modeling Strategic Agility Based on Technological Transformation Considering Mediating and Moderating Variables

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1. Round 1

1.1. Reviewer 1

Reviewer:

In the third paragraph of the Introduction, the statement that “technological transformation may strengthen strategic agility by reducing information asymmetry, shortening decision cycles, increasing organizational visibility, and creating more flexible mechanisms for resource deployment” should be developed into a more precise conceptualization of “technological transformation.” At present, it remains unclear whether the construct refers to digital transformation, digitization, digitalization, technological modernization, implementation of customs information systems, artificial intelligence, automation, data analytics, or a broader organizational transformation enabled by technology. Because the reference literature predominantly discusses “digital transformation,” while the manuscript consistently uses “technological transformation,” the authors should justify this terminology and specify whether these concepts are theoretically equivalent. The measurement items should also be aligned with the final definition so that construct operationalization corresponds directly to the conceptual argument.

In the Introduction paragraph beginning with "The relationship between technological transformation and strategic agility, however, is unlikely to be direct or automatic," the manuscript makes an important argument for mediation, but the rationale for the exact ordering of the mediators needs to be more rigorous. Why must strategic decision-making precede structural reconfiguration, why must structural reconfiguration precede ecosystem coordination, and why must ecosystem coordination precede operational agility? Alternative theoretically plausible sequences could be proposed, such as digital transformation directly improving operational agility or ecosystem integration influencing structural adaptation. The authors should develop explicit hypotheses for each causal link and explain why the proposed ordering is theoretically preferable to competing models. If possible, alternative structural models should also be empirically compared to demonstrate that the proposed serial mechanism provides superior explanatory performance.

In the paragraph discussing strategic decision-making, especially the sentence, "Strategic decision-making is especially important because organizations cannot become agile merely by possessing more information; they must also possess the capability to interpret and act upon that information," the construct labeled "strategic decision-making dynamism and intelligence" requires stronger theoretical grounding. This terminology is relatively uncommon and appears to combine at least two potentially distinct concepts: decision-making dynamism and decision intelligence. The manuscript should clarify whether this is a reflective single construct or a multidimensional construct comprising separate but related dimensions. The authors should identify the theoretical literature from which the construct was derived and explain how its 13 questionnaire items capture a unified latent variable. This clarification is particularly important because this construct produced the strongest structural coefficient in the model ($\beta = .490$ in predicting structural reconfiguration), making its conceptual validity central to the study's conclusions.

In the Research Method paragraph, the statement that "Because a complete sampling frame for the study population was unavailable, the characteristics of the population could not be assumed to be homogeneous... A sample of 360 participants was selected using proportionate stratified probability sampling" presents a methodological inconsistency that must be resolved. Probability-based proportionate stratified sampling ordinarily requires a sufficiently defined sampling frame within each stratum so that individuals have known probabilities of selection. If a complete list of customs employees was unavailable, the authors need to explain precisely how probability sampling was operationalized. The manuscript should report the strata used, the number of employees within each provincial stratum, the allocation formula, the mechanism used to select individuals within strata, the number approached, and the response rate. If selection within strata was based on accessibility or administrative availability, the method should instead be described as proportionate quota sampling or another appropriate nonprobability procedure.

In Table 2 and the associated structural-model interpretation, the manuscript states that R^2 values above .19 and Q^2 values above .15 establish satisfactory structural fit. This interpretation requires refinement because R^2 is an explanatory-power measure and Q^2 is a predictive-relevance statistic, not a generic model-fit criterion. The authors should interpret each endogenous construct separately rather than apply one universal threshold. For instance, strategic agility has $R^2 = .202$, indicating that the model explains approximately 20.2% of its variance; this should be explicitly discussed as relatively modest explanatory power. The authors should also report Q^2 generated by an appropriate blindfolding omission distance and, preferably, contemporary out-of-sample predictive assessment such as PLSpredict if prediction is a central objective. Reporting adjusted R^2 and confidence intervals would also strengthen the structural evaluation.

Authors revised the manuscript and uploaded the new document.

1.2. Reviewer 2

Reviewer:

In the Research Method paragraph, the sample-size justification based on "Cohen's (1988) statistical power analysis tables for multivariate tests" is not sufficiently tailored to PLS-SEM and the actual model tested. The statement that a sample exceeding 350 participants can detect small-to-moderate effects is too general and does not report the parameters underlying

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the power calculation. The authors should conduct and report an explicit a priori or sensitivity power analysis specifying the anticipated effect size, alpha level, desired power, and maximum number of predictors entering an endogenous construct. Because the study includes mediation and moderation, the sample-size justification should also consider the statistical power required for interaction and indirect effects. A PLS-SEM-specific justification, potentially complemented by statistical power analysis rather than relying solely on a general table, would make the methodological argument substantially stronger.

In the Research Method paragraph describing the "researcher-developed questionnaire," considerably more detail is required regarding instrument development. The manuscript states that the items were developed through a systematic review, theoretical models, and previous instruments, but does not describe the databases searched, selection criteria, number of sources reviewed, procedures used to generate the initial item pool, number of initial items, item-reduction process, or expert-evaluation procedure. The authors should report how many experts evaluated face and content validity, their areas of expertise, and whether quantitative content-validity indices such as CVR and CVI were calculated. Because all seven central constructs are measured through this newly developed instrument, rigorous documentation of the scale-development procedure is essential. Ideally, exploratory factor analysis in an independent pilot sample should precede confirmatory assessment, or the authors should explain why a purely confirmatory measurement strategy was appropriate.

In the same Research Method paragraph, the questionnaire is described as containing "72 items categorized into seven components," but later six indicators—Items 2, 14, 33, 34, 36, and 45—were deleted because of low loadings. The authors should provide a supplementary table containing all retained and deleted items, their original construct assignments, standardized loadings, t statistics, VIF values, and the theoretical rationale for retaining or deleting them. Indicator removal should not be conducted solely to improve statistical fit because deleting items can compromise content validity. The manuscript should therefore state whether the removal of these six items altered the conceptual coverage of any construct, particularly those with relatively few items, and whether the structural results remained stable before and after item deletion.

In the paragraph on normality, the sentence "In structural equation modeling, assumptions such as normal distribution of the data and a relatively large sample size are required" should be revised because it is overly broad and potentially misleading. Such requirements apply primarily to certain covariance-based SEM estimators; PLS-SEM is variance-based and is commonly selected precisely because it does not impose multivariate normality in the same manner. The authors should distinguish clearly between covariance-based SEM and PLS-SEM and explain why PLS-SEM was selected based on the study objectives, model complexity, prediction orientation, distributional characteristics, and presence of interaction terms. Furthermore, reporting only Shapiro–Wilk and Kolmogorov–Smirnov significance tests with n = 360 is insufficient because such tests become highly sensitive at larger sample sizes. Skewness, kurtosis, and preferably multivariate distribution diagnostics should also be reported.

In the Measurement Model Fit paragraph, the criterion that factor loadings above .40 and absolute t values above 2.58 establish "sufficient validity" should be reconsidered. Contemporary PLS-SEM practice generally expects substantially stronger outer loadings for established reflective constructs, with .708 commonly used as a desirable benchmark, although lower loadings may sometimes be retained for content-validity reasons. The authors should report the complete outer-loading matrix and explain why a .40 threshold was selected. Additionally, the manuscript should clearly state whether all constructs were modeled reflectively, formatively, or as composite constructs. This is critical because reliability, AVE, outer loadings, and Fornell–Larcker-type procedures are appropriate primarily for reflective measurement models; formative constructs require different assessment criteria such as indicator collinearity and weight significance.

In the Measurement Model Fit paragraph, the reported reliability value of "1.000 for technological transformation in customs administration" for both composite reliability and Cronbach's alpha is highly unusual for a construct reportedly measured by approximately ten items. A perfect reliability coefficient may indicate duplicated items, identical response patterns, computational or data-entry errors, or a model-specification problem. This result requires careful verification. The authors should inspect the inter-item correlation matrix, item variances, coding, and measurement model specification and report the actual outer loadings and reliability estimates. If 1.000 is accurate, the construct may suffer from item redundancy rather than representing desirable reliability; extremely high reliability can indicate that items are effectively measuring the same narrow content rather than adequately representing the breadth of the construct.

In Table 1 and the paragraph interpreting discriminant validity, the claim that "the values on the main diagonal were the highest values within their respective columns, indicating acceptable discriminant validity" is contradicted by the displayed matrix. For example, the correlation between strategic decision-making dynamism and intelligence and business ecosystem

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coordination and integration is .789, while the diagonal value for strategic decision-making is .760; similarly, several off-diagonal coefficients such as .808 and .836 appear to exceed corresponding diagonal values. These patterns may indicate serious discriminant-validity problems if the table represents the Fornell–Larcker matrix. The authors must recheck Table 1, verify whether the diagonal elements are square roots of AVE, and perform the heterotrait–monotrait ratio (HTMT), preferably with bootstrapped confidence intervals. Discriminant validity should not be claimed until these apparent violations are resolved, as construct overlap could materially affect all subsequent structural coefficients.

Authors revised the manuscript and uploaded the new document.

2. Revised

Editor's decision after revisions: Accepted.
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