EDITOR'S COMMENTS

Rebalancing Novelty with Rigor and Relevance in Information Systems Research

By: **Heshan Sun, Senior Editor**
Wen Wen, Associate Editor
Jason Bennett Thatcher, Senior Editor
Michael Chau, Senior Editor
Susan Brown, Editor-in-Chief

This editorial is inspired by a growing concern within the information systems (IS) community that novelty has become a dominant criterion for publication, despite being unevenly interpreted and poorly specified. In our editorial experience, when authors are told that their papers lack novelty, they often receive little concrete guidance on what precisely counts as novelty, how it varies across paradigms, or how it should be balanced with rigor and relevance.

At about the same time the IS discipline was founded, Murray S. Davis (1971) published his classic work, "That's Interesting! Towards a Phenomenology of Sociology and a Sociology of Phenomenology," which asserted that for research to be interesting it should challenge assumptions and violate expectations. His ideas have exerted a profound influence on IS scholarship. In Davis's terms, many of the most celebrated IS papers would be considered "interesting" because they challenge orthodoxy, expose unexpected mechanisms, or offer surprising arguments and/or empirical findings. Experienced IS authors and editors thus learned to focus on providing interesting, often counterintuitive insights. Yet, expecting every paper to have a surprise can lead to problems.

In other disciplines, Davis's main argument has been questioned. For example, Tsang (2022) argued that an "obsession with interestingness" promotes such detrimental outcomes as post-hoc theorizing, discouraging replication, and undermining doctoral education. Similarly, Tihanyi (2020) warned that chasing interestingness may crowd out the more important task of advancing theories that influence society or resolve important debates.

We believe that IS researchers should take this critique seriously. The issue is that novelty is often misunderstood—too often equated with interestingness, surprise, or complexity, and insufficiently anchored in conceptual clarity, methodological rigor, or practical relevance. Misinterpreting novelty creates predictable problems: Authors struggle to position their work, reviewers offer vague or inconsistent critiques, and editors receive submissions that are "interesting" but weak in rigor, relevance, or both. We believe that there needs to be greater clarity around the concept of novelty so that it can be discussed in a more balanced manner, along with the rigor and relevance of the research.

In this editorial, we adopt a paradigm-inclusive definition of novelty, which refers to the originality of concepts, relationships, or methods in a study and manifests in various forms, including theoretical extensions, new empirical phenomena, and design innovations (Colquitt & Zapata-Phelan, 2007; Crossan & Apaydin, 2010; Hevner et al., 2004; Locke & Golden-Biddle, 1997). To support more consistent editorial practices, this editorial aims to: (1) clarify what novelty means in IS research and how it manifests across paradigms; (2) make visible the underlying tensions among novelty, rigor, and relevance of research and the associated risks; and (3) offer practical strategies for authors, reviewers, and editors to better align expectations for novelty with how we evaluate scholarly contributions.

What Is Novelty?

We start with clarifying how novelty manifests across common research paradigms (Table 1). These distinctions are illustrative, not prescriptive, as many IS papers blend paradigms, and novelty often cuts across boundaries.

In behavioral and theory-testing IS research, novelty is often defined in terms of the theoretical contribution: the development of new constructs, the identification of new relationships, or the extension and refinement of existing theories through boundary conditions or integrative mechanisms (Colquitt & Zapata-Phelan, 2007).

Table 1. Novelty in IS Research

Paradigm	What counts as novelty?	What counts as impact?	Risks from novelty-rigor tension	Risks from novelty-relevance tension
Behavioral / theory testing	• New constructs • New theoretical relationships • New boundary or mediation mechanisms • Extensions to new IS-relevant domains	• Sharpening or expanding the theory used in IS • Building cumulative explanations of user/org behavior • Practical relevance of predictors, effects, or conditions	• Introducing constructs only marginally distinct from existing ones (jangle fallacies) • Overfitting or data mining dressed up as theory • Novelty without explanatory depth or theoretical implications	• Repackaging "old wine in new bottles" • Retesting existing theories in new tech contexts with few theoretical advances. • Studying marginal, infrequently occurring effects with little practical value • Building for novelty without problem grounding • Overcontextualization • Novel datasets without relevant problem or theoretical insight • Findings with no enduring value
Empirical / econ-IS	• New theoretical relationships • New IT phenomena • New research designs enabling better identification of theoretical relationships	• Clear explanation of IT phenomena • Informing managerial/policy decisions • Portable findings across contexts	• Methodological fireworks without core or valid insight • Identification of spurious correlation	
Design science research (DSR)	• Novel artifact or class of artifacts • New design principles or architecture • New design method or evaluation framework	• Reusable design knowledge • Influence on artifact building or standards • Integration into future DSR programs	• Overengineering or overfitting with little generalizability • Inflated performance evaluation through hyperparameter hacking •	
Interpretive / qualitative	• New conceptual framing • New process theory or emergent constructs • Rich insight into under-theorized practices	• Deep understanding of sociotechnical meaning-making • Surfacing new assumptions or tensions • Relevance for practice or governance	• Context too narrow or idiosyncratic • Descriptive findings without abstraction • Forcing novelty without grounding in data or literature	
General			• Post hoc theorizing • Theory laundering • Overfitting, *p*-hacking, or data dredging masquerading as theorizing	

In empirical and economics-style IS research (econ-IS), besides proposing and testing new theoretical relationships, novelty may emerge from studying new technologies with significant implications for a specific problem domain (Gupta, 2017) or from introducing a context or design that enables the clean identification of theoretical relationships or yields findings with important practical or policy implications (Gopal et al., 2024).

In design science research (DSR), novelty takes the form of new artifacts, architectures, or design methods that solve substantive problems and improve existing solutions. However, novelty is not merely functional—it must also be generalizable (Gregor & Hevner, 2013).

In interpretive and qualitative IS research, novelty often lies in developing new conceptual framings, process theories, or constructs that deepen understanding of sociotechnical phenomena (Walsham, 1995). It may involve surfacing undertheorized practices, revealing hidden mechanisms, or reframing existing concepts in ways that alter how researchers and practitioners interpret digital contexts (Klein & Myers, 1999). Rich description is essential in this paradigm, and novelty emerges when such insights travel beyond the focal case, offering concepts, mechanisms, or boundary conditions that can be applied in other settings (Leonardi, 2011).

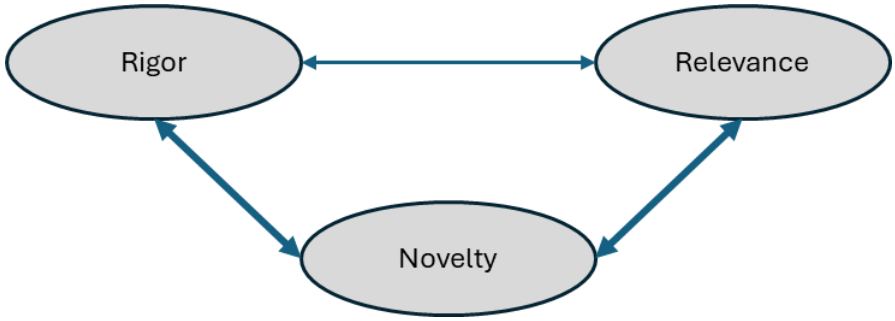

Figure 1. The Novelty-Rigor-Relevance Triad

Table 2. Definitions of Core Concepts and Tensions in the Novelty-Rigor-Relevance Triad

Dimension	***Definition***
Novelty	Novelty refers to the originality of concepts, relationships, or methods in a study and includes theoretical extensions, new empirical phenomena, or design innovations (Colquitt & George, 2011; Gregor & Hevner, 2013)
Rigor	The theoretical and empirical validity of a study and its methodological soundness. Rigor involves coherence between theory, operationalization, data collection, measurement, and analysis (Benbasat & Zmud, 1999).
Relevance	The practical or societal significance of the research. Relevance includes salience to digital practice, stakeholder utility, or public policy implications (Benbasat & Zmud, 1999).
Tension	***Definition***
Novelty-rigor tension	Occurs when the pursuit of novelty leads to speculative theorizing, overfitting, or under-validated claims.
Novelty-relevance tension	Arises when novel settings, constructs, findings, or designs lack theoretical relevance, practical resonance, or empirical generalizability.
Rigor-relevance tension	Surfaces when methodologically precise research becomes inaccessible or irrelevant to practitioners. Highly technical work may miss opportunities to inform real-world IS problems (Benbasat & Zmud, 1999).

The Tensions and Risks Introduced by Chasing Novelty

To examine the risks of chasing novelty, we build on the longstanding debate in IS research concerning the trade-off between rigor and relevance (Benbasat & Zmud, 1999). Similar to McGrath's (1981) trade-offs across generalizability, realism, and precision, we propose a three-horned dilemma involving novelty, rigor, and relevance. This triadic framework (Figure 1) surfaces two additional tensions (bold lines) beyond that between rigor and relevance, with Table 2 defining each framework element.

Risks From the Tension Between Novelty and Rigor

A first category of risk emerges when the pursuit of novelty undermines a study's theoretical or empirical validity and leads to a conflation between what is novel and what is true. Novelty invites researchers to pursue uncharted conceptual ground—but that pursuit often introduces epistemic risks and methodological strain. In behavioral and quantitative theory-testing research, the pressure to demonstrate novelty can lead authors to propose new constructs or relationships without sufficient conceptual grounding. A common pitfall is introducing constructs that are only marginally distinct from existing ones—i.e., "jangle fallacies" (Rai, 2017). Another risk lies in the fact that the abundance of large and complex datasets today makes it increasingly easy to detect statistically significant but theoretically weak patterns (Lin et al., 2013).

Similarly, in econ-IS research, the pressure to demonstrate novelty can lead to conflating the use of new or sophisticated models with genuine contribution. Simply applying complex methods developed elsewhere to a specific IS context does not, by itself, create novelty. Indeed, overengineered methods or models may obscure core insights or conceal what is true—producing methodological fireworks without conceptual fire (Van Maanen et al., 2007).

In design science, the pressure to demonstrate novelty can lead researchers to optimize artifact performance at the expense of generalizability as a component of rigor. A design may become unnecessarily complex in the pursuit of novelty. An example is the practice of hyperparameter tuning in machine learning models, where changing learning rates, batch sizes, or activation functions may yield superficially superior results without substantive innovation. Such "hyperparameter hacking" (Cooper et al., 2021) inflates performance metrics without advancing underlying design knowledge.

In interpretive and qualitative research, the pursuit of novelty can lead to overinterpreting idiosyncratic findings or forcing theoretical abstraction onto data that are not rich or broad enough to support it. Conceptual novelty in qualitative work should be grounded in thick description, contextual sensitivity, and analytic rigor—not retrofitted to data for the sake of publication (Klein & Myers, 1999).

A temptation across all paradigms is post hoc theorizing—that is, discovering an interesting or surprising empirical pattern and then retrospectively "theorizing" to justify it. This practice risks confirmation bias and theory laundering (Hyland, 2003), as researchers may, consciously or not, focus only on evidence that supports a novel claim. In extreme cases, overfitting, *p*-hacking, or data dredging can masquerade as theorizing (Mertens & Recker, 2020). Decoupling novelty from robust conceptual development and methodological discipline undermines rigor and produces misleading or false theories.

Clearly, novelty is most valuable when paired with rigor—supported by conceptual clarity, methodological transparency, and humility. Without this balance, we risk mistaking what is new for what is true—and in doing so, eroding the foundations of cumulative science.

Risks From the Tension Between Novelty and Relevance

The second major category of risk emerges across paradigms when the pursuit of novelty compromises theoretical relevance or empirical generalizability and leads to conflating what is novel and what is useful. A fundamental evaluative question for any novelty claim is: *Is it substantively necessary for understanding the phenomenon?* If the focal issue is adequately explained using existing theories, the marginal gain of introducing a novel theoretical framework may be limited. Authors should avoid repackaging "old wine in new bottles" (Spell, 2001)—that is, recasting established theories in novel packaging without advancing substantive insight—and clearly position their contribution in relation to the current literature. A recurring concern is retesting established theories in new technological contexts with few advances in theory. The fast pace of innovation can create the illusion of theoretical novelty when the conceptual apparatus is unchanged.

One risk is that pursuing novelty, by nature, may steer researchers toward rare or less observable phenomena of limited value. In many domains, well-established mechanisms or practitioner heuristics, albeit well-known and "boring," explain phenomena effectively. Hence, focusing on novelty may mean a narrow focus on marginal, infrequently occurring phenomena with little practical value.

A parallel risk in design science involves building for novelty rather than need. A complex artifact, especially one based on machine learning, may appear innovative, but its contribution is limited if it addresses a trivial or ill-posed problem. Overengineering to demonstrate computational novelty without grounding the design in real-world relevance creates the illusion of impact. Design novelty must be justified not only by technical sophistication, but by its potential to generate reusable design principles relevant across contexts and time.

Another risk comes from overcontextualization in empirical IS research. Proprietary datasets or field experiments can yield exciting empirical novelty, but studies confined to a single firm or platform may be difficult to generalize or apply elsewhere. When the empirical setting dominates the contribution, findings may be so tied to local idiosyncrasies that they offer little relevance to a broader phenomenon, provide limited managerial insight, or add only marginally to existing theory (Gupta, 2017).

Finally, novelty may be fleeting: what appears original today may quickly become obsolete. The rapid and continuous rate of change of information technology, to some degree, reduces the relevance of IS research (Benbasat & Zmud, 1999). In fast-moving technological domains, novelty must be situated not only in what is new, but in what endures. Sustainable contributions generate insights beyond a single moment, dataset, or context— they offer insights that future researchers can reuse, extend, or challenge.

Guidance for Authors, Reviewers, and Editors

Guidance for Authors

Recognizing the risks outlined in the preceding sections, we translate the *novelty-rigor-relevance triadic framework* into a set of actionable strategies for authors, summarized in Table 3 and organized around the tensions between novelty-rigor and novelty-relevance.

Novelty-rigor strategies emphasize pairing originality with sound methods, conceptual clarity, and transparency. Triangulating theories, methods, and data can strengthen the credibility of claims and reduce risks such as overfitting or post hoc theorizing (Venkatesh et al., 2016). Sharing design logic, data, and performance metrics enables replication and evaluation across contexts—critical for design science artifacts, empirical identification strategies, and interpretive process models. Justifying methodological complexity helps prevent "methodological fireworks." As Gopal et al. (2024) explain, authors should articulate why a sophisticated approach is needed and what insight it provides. Explicitly acknowledging rigor trade-offs is also necessary. For example, in behavioral or econometric work, studying emerging phenomena may require working without perfect data or ideal identification settings. Stating those limitations and explaining how robustness checks address them supports transparency and builds trust across paradigms.

Novelty-relevance strategies focus on grounding originality in clear practical or theoretical importance. This begins with clarifying why the research matters and to whom—whether the audience is academics, practitioners, or policymakers (Rai, 2017). A "contribution audit" can help distinguish genuine novelty from incremental extensions by articulating the shift in understanding, its significance, and its intended beneficiaries. Methodological novelty should enhance interpretability and inference, not simply add analytical complexity; for example, in econ-IS, a novel causal identification design should sharpen claims that are relevant to managerial or policy decisions. Validating contributions through stakeholder engagement, real-world testing, or applicability checks (Rosemann & Vessey, 2008; Van de Ven, 2007) strengthens their relevance and timelessness. In addition, authors need to clearly communicate what is novel in the current research compared to what has been done in previous research, which often requires a comprehensive literature review of the extant research. It is also essential to ensure that the novelty of research leads to enduring and reusable knowledge (e.g., constructs, relationships, design principles, etc.).

Guidance for Reviewers

Reviewers assess whether claims of novelty are credible, coherent, and supported by evidence—while balancing them with rigor and relevance. The role is not to determine acceptance or rejection but to evaluate novelty in the context of the paper's paradigm and intended contribution. Novelty should be judged on its own terms—not against personal preferences or a one-size-fits-all standard.

Identifying and articulating novelty: A helpful review begins by clearly recognizing and restating the paper's novelty claim. If, after reading, you cannot explain what is novel, why it matters, and how it is justified, the paper has not done enough. In such cases, identify what would help you explain the novelty to others—this is the basis for constructive feedback.

Be paradigm-aware: Evaluate the contribution relative to the paper's paradigm. Applying criteria from a different paradigm (e.g., expecting statistical identification in interpretive research or design artifacts in behavioral theory testing) risks misjudging its value. Ask whether the paper's novelty aligns with what counts as a contribution in its paradigm—such as new constructs and relationships (behavioral), novel identification strategies (econ-IS), innovative designs with superior performance (DSR), or transferable conceptual framings (interpretive).

Balancing novelty with rigor and relevance: Novelty without rigor can undermine credibility; novelty without relevance risks being inconsequential. Evaluate whether the paper's originality is supported by sound theorizing, methodological transparency, and conceptual clarity. In empirical work, prioritize insight over technical execution (Sudhir, 2016) and methodological clarity over methodological complexity—ask whether the findings advance the understanding of something that matters, not just whether they are statistically significant.

Table 4 summarizes the reviewer guidance, providing reviewers with a quick reference for aligning their comments with novelty expectations while maintaining rigor and relevance.

Table 3. Author Guidance

Tension	Strategy	How it addresses novelty	How it manages the tension	Core action / key question
Novelty-rigor	Triangulate findings	Demonstrates novelty through convergence of results across multiple theories, methods, or data sources; applies across paradigms.	Strengthens credibility and reduces risk of novelty being a methodological artifact.	Use diverse approaches to confirm findings and guard against overfitting or post hoc theorizing.
	Increase transparency	Makes novelty verifiable by sharing design logic, data, and performance metrics; applies across paradigms.	Enhances trust in novel contributions through replicability and openness.	Provide sufficient detail for replication and independent evaluation.
	Justify methodological choices	Aligns methods with the type of novelty claimed, ensuring complexity serves discovery; applies across paradigms.	Prevents technical sophistication from substituting for conceptual contribution.	Use methodological complexity only when it yields meaningful insight.
	Consider rigor trade-offs explicitly	Connects novelty to theoretical and empirical aims while acknowledging data and design limitations; applies across paradigms.	Mitigates risks of reduced rigor in novel settings by making trade-offs explicit.	Explain theoretical goals, empirical design, and robustness checks given data or context constraints.
Novelty-relevance	Clarify the problem	Anchors novelty in a problem valued within the relevant paradigm and audience.	Ensures originality addresses significant theoretical or practical issues.	Define why the phenomenon or construct matters and to whom.
	Conduct a contribution audit	Classifies novelty type—conceptual, methodological, contextual—and differentiates it from incremental extensions.	Ensures originality makes a substantive difference for theory or practice.	Identify the type of novelty, distinguish from incremental work, and state the main take-away.
	Balance method with interpretability	Ensures methodological novelty aids understanding rather than obscures it; applies across paradigms.	Prevents innovative methods from reducing accessibility or applicability.	Use methods that improve inference and interpretation without unnecessary complexity.
	Demonstrate relevance through validation	Links novelty to stakeholder use, real-world testing, or theoretical application; applies across paradigms.	Connects contributions to practical impact or theoretical advancement.	Use stakeholder feedback, field testing, or applicability checks to confirm relevance.
	Bridge from familiar to novel	Positions novelty in relation to established theories or debates; applies across paradigms.	Reduces adoption barriers by connecting originality to known frameworks.	Explain how the new contribution extends or revises existing knowledge.
	Build for function and future use (DSR)	Links artifact novelty to solving a real problem, producing design principles and ensuring enduring improvements.	Ensures DSR novelty has utility beyond the immediate instantiation.	Show how the design addresses enduring challenges and produces reusable principles.
	Link insight to broader systems/governance	Expands the scope of novel insights in qualitative and quantitative research to larger governance, inclusion, or system-level issues.	Broadens the applicability and significance of findings.	Relate findings to broader theoretical or governance concerns.

Table 4. Reviewer Guidance			
Strategy	**How it addresses novelty**	**How it manages the tension**	**Core action / key question**
Identify clear novelty	Makes novelty explicit and justified for the relevant paradigm: e.g., new constructs/boundary conditions (behavioral), new identification strategies or phenomena (econ-IS), innovative designs with superior performance (DSR), or transferable conceptual framings (interpretive).	Addresses both tensions by ensuring novelty is visible and connected to either rigor (clear evidence) or relevance (clear significance).	Identify what could help you explain what is novel in the paper (if this is challenging), why it matters, and how it is justified.
Be paradigm-aware	Matches novelty judgment to the paradigm: theory-building (behavioral), identification validity (econ-IS), design performance (DSR), insight transferability (interpretive).	Addresses both tensions by ensuring novelty is evaluated against the right conceptual and practical benchmarks.	Evaluate the paper on its own terms. If you are using contribution standards from a different paradigm, this risks misapplying the standard.
Balance across the triad	Places novelty in relation to rigor and relevance, accounting for paradigm-specific trade-offs.	Ensures that novelty is considered in light of potential rigor or relevance trade-offs.	Ask whether the paper's novelty is supported by sufficient rigor and meaningful relevance.

Guidance for Associate Editors and Senior Editors

Editors synthesize reviews, weigh the manuscript's strengths and weaknesses, and decide whether its novelty merits further development or publication. At *MIS Quarterly*, associate editors make recommendations and work collaboratively with senior editors, but final decisions rest with senior editors. Strong editorial practice means evaluating the work on its terms, calibrating expectations across novelty, rigor, and relevance, and ensuring that reviewer feedback is constructive and aligned with the paper's paradigm.

Judging novelty in context: Evaluate the work on its terms—its paradigm, contribution type, and intended audience. Novelty should be assessed against what counts as a contribution in its paradigm: for example, theoretical extension or boundary conditions (behavioral), new empirical phenomena or identification strategies (econ-IS), reusable design knowledge (DSR), or transferable conceptual reframing (interpretive).

Calibrating expectations across novelty, rigor, and relevance: Few papers excel in all three dimensions. If a manuscript offers strong novelty and either rigor or relevance—and the third is improvable—it may still be worth developing. The editorial task is to identify improvable dimensions and guide authors toward balance rather than perfection.

Supporting clarity in novelty positioning: If you cannot summarize the paper's novelty in one or two sentences, the authors have not made it clear enough. Request precise statements that identify what is new, why it matters, and how it scales to the paper's goals.

Filtering and reframing reviewer feedback: Editors must ensure that reviewer input helps clarify and support the paper's novelty, rather than imposing off-paradigm expectations or co-authoring tendencies. If a reviewer's suggestions would change the paper's genre or contribution type, reinterpret or filter these in the decision letter.

Thinking beyond fit: Assess the potential for contribution, not just similarity, to past publications. For unconventional papers—whether novel in method, context, or theory—the right question is "Can this be made to work?" rather than "Does this look like what we have published before?"

Table 5 distills these editorial responsibilities into actionable strategies, linking each to how novelty should be judged in balance with rigor and relevance for different paradigms.

Table 5. Editor Guidance			
Strategy	**How it addresses novelty**	**How it manages the tension**	**Core action**
Judge for what it is	Measures novelty against its paradigm: conceptual/theoretical extension (behavioral), identification/phenomenon (econ-IS), reusable design knowledge (DSR), conceptual reframing (interpretive).	Addresses both tensions by ensuring novelty is judged with the correct rigor standards and relevance expectations.	AE recommends, SE decides, both should work together to evaluate the work on its own terms—paradigm, contribution type, audience.
Calibrate expectations	Accepts that novelty can coexist with developing rigor or relevance; trade-offs vary (e.g., DSR prototypes may have high relevance but evolving theory).	Manages novelty-rigor by tolerating imperfect methods in novel settings; manages novelty-relevance by prioritizing meaningful contributions.	Assess balance and improvability. If a paper offers novelty plus either rigor or relevance—and the third can be improved—it may be worth developing. Note that few papers excel in all three dimensions;
Clarify novelty	Guides authors to define novelty type for their paradigm—construct/theoretical (behavioral), methodological/phenomenal (econ-IS), artifact/design principle (DSR), conceptual/processual (interpretive).	Addresses novelty-rigor by tightening claims with clear evidence; addresses novelty-relevance by clarifying its importance.	Request a statement of what is new and why it matters if you cannot summarize the paper's novelty in 1-2 sentences. This is an indication that the authors have not made it clear enough.
Filter reviewer feedback	Shields against misconstrued and off-paradigm criticism—e.g., behavioral work faulted for not building artifacts; interpretive work faulted for lacking statistical analysis.	Ensures that the novelty is evaluated for its intended audience and impact. This primarily concerns novelty-relevance.	Reframe or omit off-paradigm or co-authoring types of suggestions.
Think beyond fit	Recognizes unconventional novelty that meets paradigm standards even if it doesn't match past *MIS Quarterly* output.	Manages novelty-relevance by encouraging unconventional but significant contributions.	Ask: "Can this be made to work?" rather than "Does this look like what we have published before?"

Conclusion

We would be remiss if we did not close by reaffirming the importance of novelty. As a leader in the IS scholarly community, *MIS Quarterly* values novelty as a driver of intellectual progress. However, it is essential to distinguish between novelty that is necessary to warrant publication and novelty that is "interesting" but lacks rigor and/or relevance. Novel datasets, novel methods, novel designs, or novel contexts can create opportunities for contribution—but they do not constitute contribution independently. Novelty for the sake of novelty impedes the discipline's ability to build cumulative knowledge. Truly novel research often requires greater theorizing effort, methodological care, and/or evaluative patience. Hence, we have positioned novelty alongside rigor and relevance as part of a triadic framework that surfaces the tensions among them and provides concrete strategies for their management. We believe that properly calibrated novel insights are the "diamonds" that reviewers and editors "cut" and polish to expand our collective understanding (Saunders, 2005), and the framework presented here is intended to guide that process.

Throughout this editorial, our goal has been to clarify the definition of novelty in IS research; delineate the tensions between novelty, rigor, and relevance; and offer practical tools—including paradigm-specific heuristics—to support a more calibrated, conceptually grounded, and developmentally supportive approach for authors, reviewers, and editors. Authors should not have to choose between contributing new ideas and surviving the review process, nor should reviewers and editors default to skepticism when faced with unfamiliar findings or unconventional approaches. Instead, we must foster a rigorous but generous evaluative culture that supports conceptual experimentation, rewards well-founded originality, and helps novel ideas take root.

References

Benbasat, I., & Zmud, R. W. (1999). Empirical research in information systems: The practice of relevance. *MIS Quarterly, 23*(1), 3-16. https://doi.org/10.2307/249403

Colquitt, J. A., & George, G. (2011). Publishing in AMJ—Part 1: Topic Choice. *Academy of Management Journal, 54*(3), 432-435. https://doi.org/10.5465/amj.2011.61965960

Colquitt, J. A., & Zapata-Phelan, C. P. (2007). Trends in theory building and theory testing: A five-decade study of the Academy of Management Journal. *Academy of Management Journal, 50*(6), 1281-1303. https://doi.org/10.5465/amj.2007.28165855

Cooper, A. F., Lu, Y., Forde, J., & De Sa, C. M. (2021). Hyperparameter optimization is deceiving us, and how to stop it. In *Proceedings of the 35th Conference on Neural Information Processing Systems*.

Crossan, M. M., & Apaydin, M. (2010). A multi-dimensional framework of organizational innovation: A systematic review of the literature. *Journal of Management Studies, 47*(6), 1154-1191. https://doi.org/10.1111/j.1467-6486.2009.00880.x

Davis, M. S. (1971). That's interesting! Towards a phenomenology of sociology and a sociology of phenomenology. *Philosophy of the Social Sciences, 1*(2), 309-344. https://doi.org/10.1177/004839317100100211

Gopal, A., Chen, P.-y., Oh, W., Xu, S. X., & Sarker, S. (2024). On crafting effective theoretical contributions for empirical papers in economics of information systems: Some editorial reflections. *Information Systems Research, 35*(3), 917-935. https://doi.org/10.1287/isre.2024.editorial.v35.n3

Gregor, S., & Hevner, A. R. (2013). Positioning and presenting design science research for maximum impact. *MIS Quarterly, 37*(2), 337-355. https://doi.org/10.25300/MISQ/2013/37.2.01

Gupta, A. (2017). Editorial thoughts: What and how ISR publishes. *Information Systems Research 28*(1), 1-4. https://doi.org/10.1287/isre.2017.0691

Hevner, A. R., March, S. T., Park, J., & Ram, S. (2004). Design science in information systems research. *MIS Quarterly, 28*(1), 75-105. https://doi.org/10.2307/25148625

Hyland, K. (2003). Self-citation and self-reference: Credibility and promotion in academic publication. *Journal of the American Society for Information Science and Technology, 54*(3), 251-259. https://doi.org/10.1002/asi.10204

Klein, H. K., & Myers, M. D. (1999). A set of principles for conducting and evaluating interpretive field studies in information systems. *MIS Quarterly, 23*(1), 67-93. https://doi.org/10.2307/249410

Leonardi, P. M. (2011). When flexible routines meet flexible technologies: Affordance, constraint, and the imbrication of human and material agencies. *MIS Quarterly, 35*(1), 147-167. https://doi.org/10.2307/23043493

Lin, M., Lucas, H. C., Jr., & Shmueli, G. (2013). Research commentary—Too big to fail: Large samples and the p-value problem. *Information Systems Research, 24*(4), 906-917. https://doi.org/10.1287/isre.2013.0480

Locke, K., & Golden-Biddle, K. (1997). Constructing opportunities for contribution: Structuring intertextual coherence and "problematizing" in organizational studies. *Academy of Management Journal, 40*(5), 1023-1062. https://doi.org/10.2307/256926

McGrath, J. E. (1981). Dilemmatics: The study of research choices and dilemmas. *American Behavioral Scientist, 25*(2), 179-210. https://doi.org/10.1177/000276428102500205

Mertens, W., & Recker, J. (2020). New guidelines for null hypothesis significance testing in hypothetico-deductive IS research. *Journal of the Association for Information Systems, 21*(4), 1072-1102. https://doi.org/10.17705/1jais.00629

Rai, A. (2017). Editor's comments: The first few pages. *MIS Quarterly, 42*(2), iii-vi.

Rosemann, M., & Vessey, I. (2008). Toward improving the relevance of information systems research to practice: The role of applicability checks. *MIS Quarterly, 31*(1), 1-22. https://doi.org/10.2307/25148826

Saunders, C. (2005). Editorial: Looking for diamond cutters. *MIS Quarterly, 29*(1), iii-viii.

Spell, C. S. (2001). Management fashions: Where do they come from, and are they old wine in new bottles? *Journal of Management Inquiry, 10*(4), 358-373. https://doi.org/10.1177/1056492601104009

Sudhir, K. (2016). The exploration-exploitation tradeoff and efficiency in knowledge production. *Marketing Science, 35*(1), 1-9. https://doi.org/10.1287/mksc.2015.0974

Tihanyi, L. (2020). From "that's interesting" to "that's important. *Academy of Management Journal, 63*(2), 329-331. https://doi.org/10.5465/amj.2020.4002

Tsang, E. W. K. (2022). That's interesting! A flawed article has influenced generations of management researchers. *Journal of Management Inquiry, 31*(2), 150-164. https://doi.org/10.1177/10564926211048708

Van de Ven, A. H. (2007). *Engaged scholarship: A guide for organizational and social research*. Oxford University Press.

Van Maanen, J., Sørensen, J. B., & Mitchell, T. R. (2007). The interplay between theory and method. *Academy of Management Review, 32*(4), 1145-1154. https://doi.org/10.5465/amr.2007.26586080

Venkatesh, V., Brown, S.A., and Bala, H. "Bridging the Qualitative-Quantitative Divide: Guidelines for Mixed-Method Research," MIS Quarterly, 2013. Vol. 37, No. 1, pp. 21-54. 10.25300/MISQ/2013/37.1.02

Walsham, G. (1995). Interpretive case studies in IS research: Nature and method. *European Journal of Information Systems, 4*, 74-81. https://doi.org/10.1057/ejis.1995.9