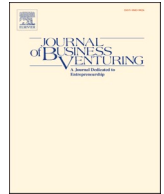




Contents lists available at ScienceDirect

Journal of Business Venturing

journal homepage: www.elsevier.com/locate/jbusvent

Bold, broad, rigorous, and...relevant? Designing entrepreneurship research for translation

Theodore L. Waldron^{a,*}, Jeffery S. McMullen^b, Scott L. Newbert^c, G. Tyge Payne^e, Jeffrey G. York^d

^a Stephenson Department of Entrepreneurship & Information Systems, E. J. Ourso College of Business, Louisiana State University, United States of America

^b Department of Management & Entrepreneurship, Kelley School of Business, Indiana University, United States of America

^c Department of Entrepreneurship & Innovation, Zicklin School of Business, Baruch College, United States of America

^d Strategy, Entrepreneurship, & Operations, Leeds School of Business, University of Colorado, Boulder, United States of America

^e School of Entrepreneurship, Spears School of Business, Oklahoma State University, United States of America

ARTICLE INFO

Keywords:

Entrepreneurship
Research
Relevance
Theory
Practice

ABSTRACT

Entrepreneurship research should strive for relevance—the potential to influence the thoughts, decisions, and actions of those who practice entrepreneurship. Despite the Journal of Business Venturing's (JBV) efforts to foster relevance, submissions often fail to demonstrate their potential for practical usefulness. Addressing this problem involves making practical relevance a guiding principle of research design that complements traditional academic standards for publishing in JBV. It also involves forgoing attempts to make research directly usable by practitioners, who rarely read scholarly journals like JBV, in favor of enhancing its suitability for translation in outlets intended for those audiences. We introduce three design criteria—importance, insight, and impact—to guide the creation of translatable research that honors traditional academic standards. Our discussion touches on what the design criteria mean, how they make research translatable, how scholars can satisfy them, what they look like in exemplar JBV publications, and why they matter now. Indeed, designing research with practical importance, insight, and impact in mind may yield academic contributions more suitable for dissemination beyond academic circles, positioning entrepreneurship research to achieve practical relevance.

Entrepreneurship scholarship should strive for relevance. Relevance means the potential for research to influence the thoughts, decisions, and actions of those engaged in the actual practice of entrepreneurship—entrepreneurs and their teams, investors, customers, and other stakeholders (Nicolai and Seidl, 2010). As applied social scientists, entrepreneurship scholars have an obligation to produce research characterized by demonstrable academic contributions (e.g., theoretical novelty, methodological sufficiency) and potential practical usefulness (Wry et al., 2023). To paraphrase (and slightly amend) a quote attributed to Kurt Lewin (1945), there is nothing more practical than good research. The alternative—conducting quality research for its own sake rather than to improve the lives of those we study—results in a shallow professional existence, one that is increasingly scrutinized by those who fund our work and marginalized by those who should utilize it (Klinsky, 2025). Avoiding this pitfall can be challenging because incentive structures in our

* Corresponding author.

E-mail addresses: twaldron@lsu.edu (T.L. Waldron), mcmullej@iu.edu (J.S. McMullen), Scott.Newbert@baruch.cuny.edu (S.L. Newbert), tyge.payne@okstate.edu (G.T. Payne), Jeffrey.York@colorado.edu (J.G. York).

<https://doi.org/10.1016/j.jbusvent.2025.106511>

Received 3 April 2025; Received in revised form 12 May 2025; Accepted 20 May 2025

Available online 30 May 2025

0883-9026/© 2025 Elsevier Inc. All rights are reserved, including those for text and data mining, AI training, and similar technologies.

profession have historically undervalued the broader implications of research, although times may be changing.

Like other scholarly journals, the *Journal of Business Venturing* (*JBV*) primarily evaluates research quality based on academic criteria, such as theoretical boldness, breadth of topical interest, and methodological rigor. But what about practical relevance? Our collective editorial experience suggests that many submissions continue to fall short in this regard despite *JBV*'s efforts to foster relevance (e.g., partnering with *Entrepreneur & Innovation Exchange* (*EIX*) to translate select publications for broader consumption). Deficiencies in relevance often manifest as the dreaded “so what” question, as contributions fail to matter beyond their intrinsic value in creating knowledge. Even authors who aim to have an impact beyond academia may struggle to answer this question if they lack experience in developing academic contributions that can be applied in entrepreneurial practice. Fortunately, like many ostensibly intractable problems, the relevance problem might have a simple solution.

Making practical relevance a guiding principle of research design that complements traditional academic standards may help scholars to optimize their work's practical usefulness. Designing research to be directly and immediately valuable for entrepreneurship practice, the focus of many prior calls for relevance (Kieser et al., 2015), amounts to tilting at windmills. Folks involved in entrepreneurial action rarely—if ever—seek out and read original academic research about their efforts, including that published in *JBV* and comparable scholarly journals. A more pragmatic approach entails designing research to optimize its suitability for translation in outlets that more directly reach these audiences. For example, *EIX*'s website annually draws more than 20 million visitors who seek open-access, research-based insights about entrepreneurship practice and education. Well-known periodicals, such as *Entrepreneur*, *Inc.*, and *Forbes*, likewise solicit such insights from scholars and share them with millions of readers. Although creating translatable knowledge does not guarantee widespread dissemination or relevance among practitioners, it increases the potential usefulness of entrepreneurship research beyond isolated academic circles.

We offer actionable prescriptions for how scholars can design research for translation, providing a realistic and achievable way to increase its potential relevance. Drawing insight from personal endeavors to disseminate our research for public consumption, we introduce three design criteria—*importance*, *insight*, and *impact*—for making research more translatable. We build on the premise that research should satisfy these criteria to become more readily suitable for translation. Table 1 foreshadows our elaboration of what these criteria mean, why they matter in making research translatable, what scholars can do to satisfy them, and what they look like in exemplar *JBV* publications. After building out these points, we provide some contextual reasons why making research translatable matters beyond our obligation as applied social scientists, underscoring the contemporary value of our prescriptions.

Some assumptions underpin our suggestions. Although our discussion primarily focuses on advising authors on how to increase the likelihood of their work being perceived as practically applicable and useful, the research design criteria also provide a useful checklist for editors and reviewers to assess this potential in *JBV* submissions. Additionally, we advocate making research's potential for practical relevance (as reflected in its translatability) a guiding principle of its design while assuming this focus complements rather

Table 1
Three criteria for designing translatable entrepreneurship research.

	Definition	Indicators	Exemplars
Importance	Important research attends to phenomena that are timely and consequential to contemporary entrepreneurship practice	- Specifies the real-world phenomena of interest in precise terms - Uses what extant scholarship does not explain about focal phenomena, but should, to motivate research questions - Pose research questions about aspects of focal phenomena that require theoretical attention and elaboration	- Genedy et al. (2024) - Lindquist and Vladasel (2025) - Meek et al., 2010 - Nason et al. (2024) - Nguyen et al. (2024) - Williams and Fathallah (2024) - Yoon et al. (2024)
Insight	Insightful research makes academic contributions that focus on improving scholarly understanding of/capacity to explain focal phenomena	- Shows evidence—examples, samples, data, etc.—that academic contributions apply to/accounts for/explains aspects of focal phenomena - Acknowledges contextual boundaries on the applicability of academic contributions - Ensures academic contributions balance empirical accuracy and conceptual generalizability - Avoids using extant theories to explain focal phenomena in ill-fitting ways or from creating new theories for every idiosyncratic empirical setting	- Bracht et al. (2024) - Chila and van den Oever (2025) - McMullen et al. (2021) - O'Neil et al. (2022)
Impact	Impactful research foreshadows its academic contributions' practical usefulness to participants in focal phenomena	- Offers actionable advice for practitioners based on academic contributions; makes practical implications sections meaningful - Features advice informed by/tailored to target practitioner audiences and outlets - Presents advice in non-academic language that can readily be translated for practitioner audiences	- Byrne and Giuliani (2025) - Cumming et al. (2024) - Waldron et al. (2022) - Yang et al. (2020)

than replaces traditional academic standards. Ensuring research exhibits academic boldness, breadth, and rigor may make it more suitable for publication in *JBV*. Simultaneously, ensuring that research exhibits practical importance, insight, and impact can make its academic contributions more suitable for translation in practitioner outlets and dissemination to broader audiences, thereby laying the groundwork for relevance.

1. Three criteria for designing translatable research

1.1. Research design criterion 1: Importance

Designing research to be translatable involves establishing its *importance*, which entails attending to timely and consequential phenomena in contemporary entrepreneurship practice. Our experience indicates that practitioner-oriented outlets consider importance a foundational requirement when selecting research to share with their audiences. We have found that simple design choices can help authors showcase their research's importance.

One design step involves defining the “real-world” phenomena that the research explores. Key considerations include articulating what the phenomena entail, providing examples of what they look like in reality, assessing how prevalent they are, and explaining why they matter to entrepreneurship practice. Research that showcases attentiveness to timely and consequential events should lend itself more naturally to translation than works that do not. For instance, [Nason et al. \(2024\)](#) examine how entrepreneurs make their firms visible to resource-granting stakeholders (e.g., community members) but not to resource-extracting stakeholders (e.g., authorities). Their paper, which explores such actions in Delft township of Cape Town, South Africa, is rich with images of “real-world” context. Other recent publications consider entrepreneurial responses to crisis ([Branzei et al., 2025](#)), such as how UK entrepreneurs pivot in response to global inflation ([Nguyen et al., 2024](#)).

Related design steps that scholars can take to establish their research's importance involve motivating and posing research questions about focal phenomena, which also vitally foreshadow academic contributions. Tackling a “gap” in the literature matters in scholarly inquiry ([Grant and Pollock, 2011](#)). However, the importance of addressing a gap necessitates demonstrating that doing so matters beyond the academy ([Fisher et al., 2021](#); [McMullen and Shepherd, 2006](#)). Important inquiries hold meaning for entrepreneurs, investors, policymakers, and other stakeholders of emerging organizations, markets, or industries, promising to create knowledge that may prescriptively guide behavior. We do not suggest that all entrepreneurship research should provide recipes or apply to specific practical problems; instead, it should strive to develop knowledge that can contribute to solving a class of such problems ([Van Aken, 2004](#)).

Connecting theoretically interesting and practically essential topics requires “studying problems that are not merely interesting but are relevant to society” ([Tihanyi, 2020](#): 329). Of course, appreciating the importance of various phenomena from the confines of the “ivory tower” is not always easy, because such appreciation results from building intimate familiarity with entrepreneurial actors, actions, and processes. Those interested in building such appreciation, a requisite for creating and evaluating translatable research, can look for opportunities inside (e.g., advancement/development initiatives, advisory boards, executive and graduate programs, outreach/service programs, etc.) and outside (e.g., coaching, consulting, etc.) academic settings ([Butler et al., 2015](#)). Not all knowledge created to improve decision-making and behavioral effectiveness in entrepreneurship qualifies as groundbreaking from a scholarly perspective. Yet, as [Pillutla and Thau \(2013\)](#) maintain, “as long as the problem is one that is unsolved and of interest, it is demanding theory that explains it” (p. 192).

Importance implies timeliness and consequence; research questions address phenomena deemed more important if they are salient and meaningful to entrepreneurs or other involved stakeholders. What qualifies as such may change over time; some research deemed to be unimportant now may turn out to be important later, and vice versa ([Learmonth et al., 2012](#)). Ultimately, the importance of entrepreneurship research depends on the possible present and future salience and consequentiality of focal phenomena.

As one example of important entrepreneurship research, [Yoon et al. \(2024\)](#) study how the physical effects of climate change motivate entrepreneurs to develop and protect intellectual property (IP) related to climate change adaptation. While these scholars identify a gap in the sustainable entrepreneurship literature (i.e., it overlooks IP-related concerns in entrepreneurial responses to climate change), they also motivate their work by citing news reports about how rising sea levels are devastating coastal communities worldwide. Rather than treating importance as an afterthought to their theory and methodology, [Yoon et al. \(2024\)](#) emphasize the role of a timely, consequential phenomenon in motivating their study and thus lay the groundwork for its academic contributions to be translatable.

We advise researchers to select problems for study that are important to those outside of academia and to formulate research questions based on the theoretical limitations of existing scholarship in addressing these problems. And the more people who find a problem meaningful, the more research on it may matter to someone other than a handful of academics. Such issues will likely resonate beyond the academy because “Every true tale... has the same underlying structure. ...Character has a problem. He struggles with a problem. Most of the piece is about the struggle, and then you get a resolution in the end in which the character overcomes the problem or is defeated by it” ([DeSilva, 2002](#) quoted in [Hart, 2011](#): 10). Thus, it stands to reason that research about common problems will likely find an audience beyond academia because practitioners can identify with the plight of the study's protagonist.

1.2. Research design criterion 2: Insight

Designing research to be translatable also involves offering *insights*, specifically academic contributions (e.g., new theories, findings) that aim to enhance the scholarly understanding of, or the capacity to explain, focal phenomena. In our experience, practitioner-

oriented outlets seek research that addresses timely, consequential phenomena (importance) and makes their underlying conceptual mechanics more comprehensible (insight). Effectively, demonstrating insight means making academic contributions that can be translated into practically usable advice.

One design step that scholars can take to establish their work's insightfulness is to show evidence that their academic contributions account for, apply to, and explain intended aspects of focal phenomena, not something adjacent or unrelated. Such evidence depends on the type of research project. For example, pure theoretical work that builds insights from profound observation or experience with phenomena often articulates the phenomenological grounding of ideas and offers empirical illustrations of key proposals. Advocates of phenomenon-driven theorizing, such as Fisher et al. (2021), Bamberger (2008), and Welter (2011), provide more detail about building theory that can explain the practical enactment of entrepreneurship, not solely its idealized depiction in past scholarship. Resulting insights may pertain more to the actors being studied and guide more effective behavior by them than would a theory without any phenomenological basis. The same premise applies to, and often is more readily observable in, empirical research due to its focus on relevant data.

Along with demonstrating applicability to focal phenomena, establishing research insight also requires acknowledging the contextual boundaries of its academic contributions' applicability, which are often rooted in features of the entrepreneurial phenomena under consideration (Waldron et al., 2025a). As Packard (2017: 541) notes, "only most of a sample, at best, will react in a particular way and never all of them, and that the magnitude of reactions often vary widely, demonstrates the heterogeneity of responses." Insightful research embraces, rather than shuns, reasonable limits on the generalizability of its academic contributions. Doing so involves specifying the "who" (e.g., what types of actors and ventures), "when" (e.g., what points in time; when in entrepreneurial processes), and "where" (e.g., what industries and regions) (Newbert et al., 2022). These "temporal and contextual factors set the boundaries of generalizability, and as such constitute the range of the theory" (Whetten, 1989: 492). Nothing can explain everything, after all.

Demonstrating insight entails presenting academic contributions in ways that simultaneously balance their explanatory accuracy and generalizability. To accomplish this, scholars must ensure their academic contributions neither overly focus on the empirical features of focal phenomena nor on conceptual representations untethered from these features. In qualitative research, for example, describing the literal behaviors of the actors involved may often be too idiosyncratic to matter much beyond the focal research setting. Conversely, theorizing constructs and relationships with little observable basis in these behaviors may lack sufficient capacity to explain them accurately in any research setting. Insightful research strikes a balance between empirical observations and theoretical explanations, yielding academic contributions that meet the criteria for publication in scholarly journals and translation in practitioner outlets.

To elaborate on this balance with a metaphor, authors should not mistake fascination with narrative or plot for theme. As opposed to a narrative, which recounts what the principal characters of a story have done, plot is "a series of events deliberately arranged so as to reveal their dramatic, thematic, and emotional significance" (Burroway, 1987, quoted in Hart, 2011: 10). Story, according to this view, is "narrative plus plot" (Hart, 2011:10). With this distinction in mind, we encourage a dualistic approach that recognizes papers tell a story on at least two levels simultaneously. The first is a story of facts concerned primarily with drama, while the second is a story of meaning concerned primarily with theme.

Facts alone often cannot tell a dramatic story to hold a reader's attention. Whether it is a humorous tale intended for children or a journalist's sensationalist account of a horrific tragedy, a narrative must infuse the facts with enough plot to become a story that can hold the intended audience's attention. Similarly, a case narrative with interesting results can provide dramatic insight without necessarily teaching the reader something that could be applicable to contexts beyond the current focus.

In contrast, theme transcends the factual plane, precisely because it taps into insight related to meaning. Without a theme, facts lack meaning, emotion, and inspiration. Consider, for instance, a fable, parable, or an allegory. Each conveys a moral to the story, offering insight that transcends simple fact by providing lessons that can be applied to other situations of a similar nature. Therefore, insightful research does not focus so myopically on the narrative contexts in which focal phenomena unfold that it loses sight of broader, theoretical constructs, proposals, and other relevant concepts that may apply to or help explain different phenomena. Indeed, "opportunities to...make shared theoretical contributions as opposed to contextually siloed phenomenological contributions" matter (Bakker and McMullen, 2023: 8). Just as in literature, plot in an academic paper communicatively serves the theme, but that theme flows from an intimate knowledge of the phenomenon grounded in deep understanding of context.

Editors and reviewers have become quite astute at noticing and calling out when data, results, or narratives are interesting but fall short of speaking to a theme capable of making a theoretical contribution. In fact, this lack of theoretical contribution has become one of the most common reasons for rejection (Campbell and Aguilera, 2022; Corley and Schinoff, 2017). As good as reviewers may be at detecting a lack of theoretical contribution, they seem to be just as bad, if not worse, at detecting when a paper has fallen short of generating practical insight because it has failed to engage an interesting phenomenon or context deeply enough. That is, reviewers are quick to reject a narrative without a theme, but slow to reject a theme that lacks an interesting phenomenological story. Put more bluntly, if the findings of your paper would make your mother reply, "Well, duh!", then it should not matter how much theoretical jargon is used to dress them up as insightful (DeNisi, 1994). Yet, perennial calls in adjacent fields, such as strategy, organizational behavior, or operations management, for research to be useful or practically relevant (Kieser et al., 2015; Toffel, 2016) are a testament to how easily scholars can confuse ostensible theoretical contributions with actual phenomenological insights of practical relevance.

What we advocate is not easy to deliver. Positioning academic contributions, those that aim to advance scholarly understanding of focal phenomena, within existing literature can be challenging. This challenge stems less from a lack of importance (e.g., the problem is real and faced by many) or insight (e.g., the theme offers a clear theoretical contribution) and more from a lack of scholarly conversation about the focal problem. In these circumstances, authors face pressure from editors and reviewers to draw on extant theories

to orient exploration of focal phenomena in ill-fitting ways, or to create new theories for every unique empirical setting. Resisting such pressure may yield more insightful academic contributions while creating tensions between knowledge creators and evaluators.

For example, [McMullen et al. \(2021\)](#), examined how digitization provides entrepreneurs direct access to consumers in cultural industries, such as online self-publishing, thereby promising the possibility of assuaging the plight of the starving artist—that is, paying one's dues into a trust-based social network that may never reciprocate. First, the authors sought to determine whether and how intermediaries use widespread acclaim when deciding to invest in self-published books. Then they flipped the script to examine whether entrepreneurs can encourage this intermediary investment through a digital serialization strategy—periodically publishing media content to capture and hold an audience's attention. After finding that acclaim was essential to both short- and long-term performance, the authors did not stop by describing how entrepreneurs captured the attention of customers and intermediaries. Instead, they sought to generate actionable knowledge by using their discovery model to examine the relative efficacy of various tactics. As a result, their discovery model provides insight into how entrepreneurs can leverage the fact that demand in the media sector often follows a power law distribution and exhibits positive feedback loops (i.e., customer attention initiates intermediary investment and stimulates further attention).

Despite their study's conceptual, methodological, and practical contributions, the path to publication was steep owing to the academia's tendency to privilege existing topics (that tend to be backward-looking in their positioning) over new phenomenological problems (that tend to be forward-looking). Addressing new phenomena can be difficult due to a lack of prior research on them in the scholarly conversation where one seeks to publish. This reality often prevents authors from embedding their research in extant literature as thoroughly as reviewers expect, encouraging reviewers to question the new topic's legitimacy. Reviewers often sense this embeddedness issue but lack the proper vernacular to diagnose or describe it adequately, mistakenly referring to it as a lack of theoretical contribution. Technically and typically, however, it has little to do with theory or contribution. More often, it reflects reviewer unfamiliarity or discomfort with the new literature that an author introduces to entrepreneurship research. Savvy authors aware of this obstacle can lean on their work's importance and insight, as advised in this article, to overcome this challenge.

Studying new phenomena can generate considerable insight, and the reward of publishing such research can be high for authors, establishing a beachhead for a conversation that could not gain traction before. However, the price of attempting to start a scholarly conversation in a literature stream where a particular phenomenon has yet to be examined or remains grossly underrepresented can be daunting. The key to success can sometimes depend on finding an editor who understands the distinction between embeddedness challenges, which can plague attempts to frame and position a paper about a new phenomenon, versus the often imprecisely used accusation of a "lack of theoretical contribution." Given our call for authors to address important practical problems, often involving new phenomena, we knowingly ask authors to assume additional risk by emphasizing or even prioritizing their academic contributions' translatability for practitioners.

1.3. Research design criterion 3: Impact

Designing research to be translatable involves demonstrating *impact*; namely, foreshadowing its academic contributions' practical applicability and usefulness to actual participants in focal phenomena (e.g., entrepreneurs; investors). Research's impact naturally follows from and enriches efforts to highlight its importance and insight. Yet scholars often overlook this step, which provides the most direct evidence of research's suitability for translation. Our experience suggests that practitioner-oriented outlets indeed prioritize research that provides some indication of how their efforts to improve scholarly understanding of, or capacity to explain focal phenomena, matter to non-academic readership.

The most prominent thing scholars can do to establish their research's impact is to offer actionable advice for practitioners. Of course, specifying how academic contributions advance focal scholarly conversations remains indispensable. Yet, articulating broader, practical usefulness often requires great care when reporting how research contributions meaningfully inform actual entrepreneurial thoughts, decisions, and actions. Figuring out such practical implications requires foresight, taking different perspectives, and considering details uncommon in academic business research, which typically glosses over such considerations. [Hideg et al. \(2020: 1681\)](#) observe that "the writing of practical implications is often seen as a final one-paragraph hurdle rather than as an essential component of a manuscript." However, to the extent that we care about having broad impact beyond academic circles, an outcome mediated by our research's translatability, we must show how it informs more effective practices by entrepreneurial actors ([Butler et al., 2015](#)).

Maximizing research impact means presenting actionable advice in ways that can be translated in practitioner outlets and that eventually facilitate this outcome—something we have attempted to do (and sometimes accomplished) ourselves. Practical implications sections should outline the beginnings of advice, based on the research's academic contribution, that authors might want to feature in practitioner-oriented articles. These sections should provide some indication of the research's usability beyond academia without overelaborating it, given that entrepreneurship practitioners tend to overlook scholarly journals. Deciding on target outlets, audiences, and topics for translation, however tentatively, can inform what advice to feature. When presenting advice, using non-academic language and providing practical examples can help immensely. In our experience, putting a bit more thought and effort into practical implications reduces the future work needed to translate "raw" academic contributions for non-academic audiences.

[Waldron et al. \(2022\)](#) exemplify this approach. Using in-depth, qualitative analysis, the authors examine the fight for control of a craft whiskey distillery to theorize why entrepreneur-investor rivalry can occur, how it can unfold, and how it can conclude. Specifically, the authors theorized a process model to explain how the seemingly noble, mutual decision for entrepreneurs and investors to divide control evenly into complementary roles create conditions for rivalry to arise, worsen, and conclude as poorly as it did in the focal case.

The authors extensively discussed their theory's implications for practice, providing detailed, actionable advice based on their proposals. Examples include warning entrepreneurs and investors that designing relationships based on parity may not create the lasting paradise they intend (power hierarchies can help) and alerting them that formalizing relationship parity in legal documents does not guarantee such arrangements' persistence (enforcement matters). Other examples include alerting both parties that avoiding conflict means prioritizing collective goals over individual pursuits and remembering that conflict can harm innocent bystanders.

Elaborating actionable advice for entrepreneurship practice positioned the authors to publish complementary academic contributions and practical prescriptions for entrepreneurs, investors, and stakeholders affected by their relationships. Such works now appear in both scholarly outlets (e.g., *Academy of Management Perspectives*; Waldron et al., 2025c) and those with greater exposure to these actors (e.g., *EIX*; Waldron et al., 2023, 2025b). As initial evidence of impact, the *EIX* articles have been read more than 50,000 times since 2023.

2. Contextual reasons for making research translatable

Circling back to our core point, entrepreneurship research should be relevant, as facilitated by its suitability for broader translation. We strongly believe that entrepreneurship scholars have an obligation to try to position their work for relevance, regardless of whether they achieve it. Failing to honor this obligation means doing research for its own sake and thus walking the road toward irrelevance, a destination that few among us want to reach. And, to put our money where our minds, hearts, and mouths are, we have begun practicing what we preach—admittedly sometimes more successfully than others. The point, and our hope, is that scholars will try to do the same as they prepare submissions for *JBV* while remaining true to the journal's commitment to scholarly boldness, broadness, and rigor.

If the obligation to enhance entrepreneurship research's relevance, borne of working in an applied social science, proves insufficient to motivate such efforts, perhaps the profound changes happening around us will. Ongoing shifts in the higher-education landscape could eventually impact entrepreneurship, other academic business disciplines, and their respective journals. The U.S., for example, is experiencing severe challenges in terms of rising tuition costs, crushing student debt, and declining university enrollments (Klinsky, 2025), not to mention disconcerting student post-graduation performance (Molinsky and Pisman, 2019), evaporating government support (Nietzel, 2025), strengthening artificial intelligence (Weil, 2024), and more. Forces like these have reinvigorated questions about the value of academic research to business education and practice, with corresponding expectations that such scholarship should matter within and beyond academia (Haenlein and Jack, 2025; Stremersch et al., 2021). Uncertainty abounds regarding when *JBV* and other scholarly entrepreneurship journals will face such questions from non-academics and how such scrutiny will impact their activities.

Additionally, universities bankrolling those involved in *JBV*'s review processes have begun to support the creation, publication, and translation of relevant entrepreneurship research. As Tihanyi (2020: 330) argued, “to recognize and reward important research, the way research productivity is evaluated...will need to change.” Based on an informal survey of *JBV*'s editorial team, the tide seems to be moving in this direction. Many editors reported that their home institutions incorporate—and, in some cases, require—indicators of research translation into tenure and promotion decisions. Examples include publishing practitioner-oriented articles, cases, and reports; providing consulting services for businesses; delivering research presentations to non-academic organizations; informing government policy; receiving media coverage; and otherwise disseminating research-based advice to mass audiences. These experiences suggest an increasing appetite at the university level for attempts to make research relevant, namely by creating knowledge suitable for dissemination beyond the ivory tower.

3. Conclusion

This article offered design criteria to guide prospective *JBV* contributors in making their research more conducive to widespread dissemination, namely through practitioner-oriented outlets. Enacting this advice, or the spirit of it, consistently and systematically may help to position *JBV* to honor its disciplinary obligation to feature relevant scholarship, to mitigate unwanted scrutiny of the journal's value outside academic circles, and to benefit from changing expectations at its home institutions. These accomplishments hinge not only on the extent to which authors attend to their research's eventual dissemination when designing it, but also on how editors and reviewers will consider, value, and look to enhance submissions' potential for relevance. Speaking to ourselves as much as others, this means evaluating the suitability of academic contributions for eventual translation.

The idea of making research relevant in organizational studies certainly is not new. However, changing circumstances certainly seem to bring some urgency and salience to an existing obligation for applied social scientists. The immortal sentiments of Bob Dylan may capture the opportunity at hand. Dylan once sang, “Come on, try a little, nothing is forever, there's got to be something better than in the middle.” Wait, never mind, that was his kid's song (*One Headlight* by Jakob Dylan of the Wallflowers). It was Bob who intimated “the times...are a-changin,” so we must “start swimmin’ or “sink like a stone.” What choice will we make?

CRedit authorship contribution statement

Theodore L. Waldron: Writing – review & editing, Writing – original draft, Supervision, Project administration, Conceptualization. **Jeffery S. McMullen:** Writing – review & editing, Writing – original draft, Conceptualization. **Scott L. Newbert:** Writing – review & editing, Writing – original draft, Conceptualization. **G. Tyge Payne:** Writing – review & editing, Writing – original draft. **Jeffrey G. York:** Writing – review & editing, Writing – original draft.

Acknowledgement

We owe a debt of gratitude to our editor, Phil Kim, and anonymous reviewers. Their support and advice were instrumental in crafting this article.

References

- Bakker, R.M., McMullen, J.S., 2023. Inclusive entrepreneurship: a call for a shared theoretical conversation about unconventional entrepreneurs. *J. Bus. Ventur.* 38, 106268.
- Bamberger, P., 2008. From the editors: beyond contextualization: using context theories to narrow the micro-macro gap in management research. *Acad. Manag. J.* 51 (5), 839–846.
- Bracht, F., Mahieu, J., Vanhaverbeke, S., 2024. The signaling value of legal form in entrepreneurial debt financing. *J. Bus. Ventur.* 39, 106380.
- Branzei, O., McMullen, J.S., Newbert, S.L., Schwens, C., 2025. Journal of business venturing 2024 year in review: the year of exercising entrepreneurial agency in response to crises. *J. Bus. Ventur.* 40 (4), 106485. <https://doi.org/10.1016/j.jbusvent.2025.106485>.
- Burroway, J., 1987. *Writing Fiction: A Guide to Narrative Craft*. Scott, Foresman and Co., Glenview, IL and London.
- Butler, N., Delaney, H., Spoelstra, S., 2015. Problematising ‘relevance’ in the business school: the case of leadership studies. *Br. J. Manag.* 26, 731–744.
- Byrne, J., Giuliani, A.P., 2025. The rise and fall of the girlboss: gender, social expectations and entrepreneurial hype. *J. Bus. Ventur.* 40 (4), 106486.
- Campbell, J.T., Aguilera, R.V., 2022. Why I rejected your paper: common pitfalls in writing theory papers and how to avoid them. *Acad. Manag. Rev.* 47 (4), 521–527.
- Chila, V., van den Oever, K., 2025. Time to say goodbye? The role of SBIR funding, VC rounds, and initial alliance for director exit in new ventures. *J. Bus. Ventur.* 40 (3), 106482.
- Corley, K.G., Schinoff, B.S., 2017. Who, me? An inductive study of novice experts in the context of how editors come to understand theoretical contribution. *Acad. Manag. Perspect.* 31 (1), 4–27.
- Cumming, D., Meoli, M., Rossi, A., Vismara, S., 2024. ESG and crowdfunding platforms. *J. Bus. Ventur.* 39 (1), 106362.
- DeNisi, A.S., 1994. Is relevant research irrelevant? On evaluating the contribution of research to management practice. *J. Manag. Issues* 6 (2), 145–159.
- DeSilva, B., 2002. Endings. *Nieman Rep.* 56 (1), 47.
- Fisher, G., Mayer, K., Morris, S., 2021. From the editors—phenomenon-based theorizing. *Acad. Manag. Rev.* 46 (4), 631–639.
- Genedy, M., Hellerstedt, K., Naldi, L., Wiklund, J., 2024. Growing pains in scale-ups: how scaling affects new venture employee burnout and job satisfaction. *J. Bus. Ventur.* 39 (2), 106367.
- Grant, A.M., Pollock, T.G., 2011. From the editors: publishing in AMJ – part 3: setting the hook. *Acad. Manag. J.* 54 (5), 873–879.
- Haenlein, M., Jack, A., 2025. Measuring the long-term impact of business school research on academia, teaching, society and decision makers. *Int. J. Res. Mark.* 42 (1), 1–12.
- Hart, J., 2011. *Storycraft: The Complete Guide to Writing Narrative Nonfiction*. University of Chicago Press, Chicago, IL.
- Hideg, I., DeCelles, K.A., Tihanyi, L., 2020. From the editors – publishing practical and responsible research in AMJ. *Acad. Manag. J.* 63 (6), 1681–1686.
- Kieser, A., Nicolai, A., Seidl, D., 2015. The practical relevance of management research: turning the debate on relevance into a rigorous scientific research program. *Acad. Manag. Ann.* 9 (1), 143–233.
- Klinsky, S., 2025. Soaring tuition is making college unaffordable. We can fix this: there’s a model that can save billions in college loans and debt. We need to scale it, U.S. News. https://www.usnews.com/opinion/articles/2025-01-06/college-tuition-and-student-debt-are-out-of-control-there-is-a-way-to-fix-it?utm_source=chatgpt.com.
- Learmonth, M., Lockett, A., Dowd, K., 2012. Promoting scholarship that matters: the uselessness of useful research and the usefulness of useless research. *Br. J. Manag.* 23 (1), 35–44.
- Levin, K., 1945. The research center for group dynamics at Massachusetts Institute of Technology. *Sociometry* 8, 126–135.
- Lindquist, M.J., Vlasel, T., 2025. Are entrepreneurs more upwardly mobile? *J. Bus. Ventur.* 40 (4), 106498.
- McMullen, J.S., Shepherd, D.A., 2006. Encouraging consensus-challenging research in universities. *J. Manag. Stud.* 43 (8), 1643–1669.
- McMullen, J.S., Ding, A.W., Li, S., 2021. From cultural entrepreneurship to economic entrepreneurship in cultural industries: the role of digital serialization. *J. Bus. Ventur.* 36 (6), 106157.
- Meek, W.R., Pacheco, D.F., York, J.G., 2010. The impact of social norms on entrepreneurial action: evidence from the environmental entrepreneur context. *J. Bus. Ventur.* 25 (5), 493–509.
- Molinsky, A., Pisman, S., 2019. The biggest hurdles recent graduates face entering the workforce, *Harv. Bus. Rev.*. <https://hbr.org/2019/04/the-biggest-hurdles-recent-graduates-face-entering-the-workforce> 9.
- Nason, R., Vedula, S., Bothello, J., Bacq, S., Charman, A., 2024. Sight unseen: the visibility paradox of entrepreneurship in an informal economy. *J. Bus. Ventur.* 39 (2), 106364.
- Newbert, S.L., Kher, R., Yang, S., 2022. Now that’s interesting and important! Moving beyond averages to increase the inferential value of empirical findings in entrepreneurship research. *J. Bus. Ventur.* 37 (2), 106135.
- Nguyen, B., Tran, H., Stephan, U., Van, H.N., Anh, P.T.H., 2024. I can’t get it out of my mind” - why, how, and when crisis rumination leads entrepreneurs to act and pivot during crises. *J. Bus. Ventur.* 39 (4), 106395.
- Nicolai, A., Seidl, D., 2010. That’s relevant! Different forms of practical relevance in management science. *Organ. Stud.* 31 (9–10), 1257–1285.
- Nietzel, M.T., 2025. The number of college applicants shows healthy increase for next fall, *Forbes*. <https://www.forbes.com/sites/michaelnietzel/2025/03/14/the-number-of-college-applicants-shows-healthy-increase-for-next-fall>.
- O’Neil, I., Ucbasaran, D., York, J.G., 2022. The evolution of founder identity as an authenticity work process. *J. Bus. Ventur.* 37 (1), 106301.
- Packard, M.D., 2017. Where did interpretivism go in the theory of entrepreneurship? *J. Bus. Ventur.* 32 (5), 536–549.
- Pillutla, M.M., Thau, S., 2013. Organizational sciences’ obsession with “that’s interesting!”: consequences and an alternative. *Organ. Psychol. Rev.* 3 (2), 187–194.
- Stremersch, S., Winer, R.S., Camacho, N., 2021. Faculty research incentives and business school health: a new perspective from and for marketing. *J. Mark.* 85 (5), 1–21.
- Tihanyi, L., 2020. From “that’s interesting” to “that’s important”. *Acad. Manag. J.* 63 (2), 329–331.
- Toffel, M.W., 2016. Enhancing the practical relevance of research. *Prod. Oper. Manag.* 25 (9), 1493–1505.
- Van Aken, J.E., 2004. Management research based on the paradigm of the design sciences: the quest for field-tested and grounded technological rules. *J. Manag. Stud.* 41 (2).
- Waldron, T.L., McMullen, J.S., Petrenko, O.V., Trudell, L.T., Aronson, O., 2022. Entrepreneur-investor rivalry over new venture control: the battle for Balcones Distilling. *J. Bus. Ventur.* 37 (4), 106225.
- Waldron, T.L., McMullen, J.S., Petrenko, O.V., Trudell, L.T., Aronson, O.A., 2023. How a distillery founder and his investor went off the rails. *Entrepreneur & Innovation Exchange*. <https://doi.org/10.32617/875-666312f2ed6ee> (January 27).
- Waldron, T.L., Markman, G.D., McMullen, J.S., Gianiodis, P., 2025a. Time to face the music: a commentary on Lee, Withers, Busenbark, and Zajac’s “how music theory can inform competitive dynamics”. *Acad. Manag. Rev.* <https://doi.org/10.5465/amr.2024.0606>.
- Waldron, T.L., McMullen, J.S., Payne, G.T., Petrenko, O.V., Wetherbe, J., 2025b. Cash is not always king in founder/investor relationships. *Entrepreneur & Innovation Exchange*. <https://doi.org/10.32617/1212-67cf0bd649c67> (March 10).
- Waldron, T.L., McMullen, J.S., Payne, G.T., Petrenko, O.V., Wetherbe, J., 2025c. More than money: the complex dynamics of capital and power in entrepreneur-investor relationships. *Acad. Manag. Perspect.* 39 (2), 274–289. <https://doi.org/10.5465/amp.2023.0223>.

- Weil, D., 2024. 7 questions college leaders should ask about AI, Inside Higher Ed. <https://www.insidehighered.com/author/david-weil> (February 1).
- Welter, F., 2011. Contextualizing entrepreneurship: conceptual challenges and ways forward. *Entrep. Theory Pract.* 35 (1), 165–184.
- Whetten, D.A., 1989. What constitutes a theoretical contribution? *Acad. Manag. Rev.* 14 (4), 490–495. <https://doi.org/10.2307/258554>.
- Williams, T.A., Fathallah, R., 2024. Adapting a collective will and a way during a civil war: the persistence of an entrepreneurial ecosystem as an architecture of hope. *J. Bus. Ventur.* 39 (2), 106369.
- Wry, T., York, J., Waldron, T., 2023. The purpose and potential of entrepreneurship research. *Strateg. Organ.* 21, 429–446.
- Yang, S., Kher, R., Newbert, S.L., 2020. What signals matter for social startups? It depends: the influence of gender role congruity on social impact accelerator selection decisions. *J. Bus. Ventur.* 35 (2), 105932.
- Yoon, H., Tashman, P., Benischke, M.H., Doh, J., Kim, N., 2024. Climate impact, institutional context, and national climate change adaptation IP protection rates. *J. Bus. Ventur.* 39, 106359.