



## Essay

# Navigating the promises and perils of researching emerging phenomena in strategy and organizations

Strategic Organization

1–14

© The Author(s) 2022

Article reuse guidelines:

[sagepub.com/journals-permissions](https://sagepub.com/journals-permissions)

DOI: 10.1177/14761270221122394

[journals.sagepub.com/home/soq](https://journals.sagepub.com/home/soq)

**Siobhan O'Mahony**   
Boston University, USA

**Susan Cohen**   
University of Georgia, USA

## Abstract

Editors and reviewers often issue clarion calls for interesting research with novel theoretical contributions. In response to these calls, scholars often gravitate toward emerging phenomena—novel contexts lacking scholarly community or hot contexts with growing interest. However, simply examining novel and hot phenomena is insufficient to carve an “interesting” theoretical contribution. The promise of studying emerging phenomenon may be seductive, but doing so can also introduce under examined perils. We argue that novel and hot phenomena have distinct promises and perils that are under appreciated—with significant consequences for scholarly careers. Novel phenomena can provide first mover advantages to scholars and generate much interest but may constitute a lonely, risky journey if an appropriate theoretical community does not emerge. Hot topics attract significant attention, but can also be marked by conceptual confusion, fragmenting the accumulation of knowledge as scholars struggle to differentiate their work within a rapidly growing field. Yet, what is considered novel or hot is dynamic. Scholarly interest in novel phenomena can wax, ignite fascination, and become hot or wane with skeptical, uncertain acceptance, influencing both promises and perils. We contribute strategies to help strategy and organization scholars mitigate the perils and amplify the promises of theorizing from novel and hot phenomena.

## Keywords

field research, inductive, novel phenomena, research design, research methods, theorizing

“Only those who go where few have gone, can see what few have seen”

—attributed to Buddha

“The future is here—it’s just not evenly distributed”

—William Gibson

---

## Corresponding author:

Siobhan O'Mahony, Questrom School of Business, Boston University, 595 Commonwealth Avenue, Boston, MA 02215-1300, USA.

Email: [somahony@bu.edu](mailto:somahony@bu.edu)

Editors have long extolled the benefits of generating interesting theoretical research as part of a scholarly research agenda (Davis, 1971; Elsbach and Kramer, 2015). When asked, editors express a taste for research that is innovative or counterintuitive, with less formulaic theory testing (Bartunek et al., 2006: 9). Some scholars attempt to answer these calls by conducting research on “emerging” phenomena (Bamberger and Pratt, 2010; Ployhart and Bartunek, 2019; Von Krogh et al., 2012). By “emerging,” we mean phenomena that are novel or hot—but these are different states, with unique promises and perils. Novel phenomena unfold in new research settings lacking accepted definitions, shared language, and an established audience or community to receive or evaluate research.

If phenomena attract significant attention from the public or from a large scholarly community they eventually become “hot” and new forums mobilize, often accompanied by competition and a rush for primacy. For example, recent novel phenomena might include air taxis (Zuzul and Tripsas, 2020), smart cities (Zuzul and Edmondson, 2016), drones (Bremner and Eisenhardt, 2022) or networked activists (Massa and O’Mahony, 2021); while recent hot topics include business models (Teece, 2010; Zott and Amit, 2008), lean startup methodology (Contigiani and Levinthal, 2019; Leatherbee and Katila, 2020), and machine learning and algorithmic controls (Kellogg et al., 2020; Leavitt et al., 2021).

While the prize for producing interesting theoretical contributions is clear, the path for crafting theory from emerging phenomena is less so. Rarely do editors suggest how scholars actually produce theoretically interesting research from emerging phenomena (Sutton and Staw, 1995). Scholars have analyzed the research approach appropriate based on the maturity of extant theory (Edmondson and McManus, 2007), but few have done so based on the maturity of the phenomena. Much like cycles of management fashion or discourse (Abrahamson, 1996; Barley and Kunda, 1992), the interests of reviewers and editors also wax and wane. Thus, the strategies scholars draw upon to generate an “interesting” theoretical contribution when phenomena are novel may not be the same as when phenomena are hot. *What are the perils and promises of researching emerging phenomena?* Drawing from our collective experiences, we trace how to amplify the promises while mitigating the perils.

## The promise and perils of studying novel phenomena

Novel phenomena are intriguing, as they offer scholars uncharted territory and much room for exploration. Novel phenomena that “fall outside the scope of available theories” can create conditions ripe for innovation (Von Krogh et al., 2012: 277). Novel phenomena tempt scholars with the possibility of making new discoveries and being first to introduce those discoveries to the field. First mover advantages can enhance opportunities for recognition and help carve a distinct scholarly identity. Scholars’ own interests or social networks may draw them to novel phenomena, observing from afar or from a closet (Sutton, 1997), until assured the phenomena merits study. Novel phenomena offer opportunities to develop new theories or frameworks that can inform practice, advance policy debates, inform grand challenges or reshape markets or society (Ferraro et al., 2015; Pontikes and Rindova, 2020). For example, scholars studying crowdsourcing were called upon to inform policymakers in the early days of the phenomena. By studying novel phenomena, scholars can identify new constructs that align theory with practice and refresh the toolkits available to address complex or intractable societal problems. Scholars studying novel phenomena do not typically have to worry about their research being exciting, but may have more difficulty crafting theoretical contributions that are understood and accepted by established scholarly communities. Novel phenomena often introduce new types of data or methods and may require constructs that lack commonly accepted definitions. As shown in Table 1, these conditions can create underappreciated perils that generate risk for the study of novel phenomena, especially for junior scholars.

**Table 1.** Comparing the promises and perils of emerging phenomena.

	Promises	Perils	Mitigating strategies <sup>a</sup>
<b>Novel Phenomena</b>  New phenomena lacking accepted definitions, shared language, and an established scholarly community to receive and/or evaluate the research.	<ul style="list-style-type: none"> <li>• First mover advantages to those obtaining early access</li> <li>• Exciting research sustains interest</li> <li>• Opportunity to generate unique data</li> <li>• Potential to identify novel constructs that align theory with current practice</li> <li>• High potential for findings that stimulate “interesting” grounded theories</li> <li>• High potential impact if a scholarly community emerges</li> <li>• Potential to inform current practice or policy debates</li> </ul>	<i>Irrelevant or Illegitimate research context</i> <ul style="list-style-type: none"> <li>• Risk of phenomena becoming extinct</li> <li>• Context may be viewed as irrelevant or illegitimate</li> <li>• Explaining and justifying research context can compete with theorizing</li> </ul> <i>Burden of novelty</i> <ul style="list-style-type: none"> <li>• Data quality may be difficult to ascertain</li> <li>• Causal identification challenging, instruments untested</li> <li>• Lack of scholarly community to offer feedback</li> </ul> <i>Conceptual voids</i> <ul style="list-style-type: none"> <li>• Evaluative audience may generalize from popular press or limited personal experience</li> <li>• Generalizability may be unclear</li> </ul>	<ul style="list-style-type: none"> <li>• Leverage comparisons and levels of analyses</li> <li>• Locate a puzzle</li> <li>• Integrate novel ideas with the familiar</li> <li>• Phase novelty</li> <li>• Borrow existing scholarly communities</li> </ul>
<b>Hot Phenomena</b>  An area of academic research marked by new entrants and forums, triggered by or reinforced by public or media attention, often accompanied by competition and a rush for primacy.	<ul style="list-style-type: none"> <li>• A growing audience is enthusiastic and receptive</li> <li>• Research setting is accepted as interesting, reducing questions of relevance</li> <li>• Increased accessibility and plurality of data</li> <li>• A wide variety of methods proliferate which fosters creativity</li> <li>• Fast followers can benefit from first movers</li> <li>• With growth, new conceptual distinctions and sub-areas develop and specialize</li> </ul>	<i>The lure of the hot</i> <ul style="list-style-type: none"> <li>• Risk of overhype and over entry</li> <li>• Scholars may rush to exploit new data sources without appreciating underlying differences</li> </ul> <i>Competition and the Race for Primacy</i> <ul style="list-style-type: none"> <li>• Parallel, competing trajectories may narrow opportunities for distinction</li> <li>• Opportunities for grounded theory diminish</li> </ul> <i>Fragmenting the accumulation of new knowledge</i> <ul style="list-style-type: none"> <li>• Varying definitions of constructs surface theoretical or empirical inconsistencies</li> <li>• Uncoordinated growth can create overlapping or competing communities</li> <li>• Contestation over appropriate methods</li> </ul>	<ul style="list-style-type: none"> <li>• Supersize data collection</li> <li>• Contribute pieces to a puzzle</li> <li>• Develop dedicated communities and publishing outlets</li> <li>• Foster disclosure of research in progress</li> <li>• Manage the race for primacy</li> </ul>

<sup>a</sup>Mitigating strategies address multiple perils.

### *Peril 1: irrelevant or illegitimate research context*

Initially, novel phenomena may be viewed as irrelevant, if not illegitimate. External audiences might struggle to appreciate the value of novel phenomena or generalize from personal anecdotal experiences, creating a peril for scholars. Some novel phenomena fail to gain significant traction and dissipate into the ether. For example, initial buzz over the “Second Life” virtual reality world died quickly without gaining much scholarly attention. People could not ascertain if it was a virtual world or a game nor assess its import for society<sup>1</sup> or for organization science. Nonfungible tokens (NFTs), air taxis, drones and smart cities may face a similar peril. Exogenous events can interrupt novel phenomena from emerging such as when the corona virus pandemic shuttered coworking and maker spaces. Changes in economic conditions can do the same, affecting scholars’ ability to continue on-going research. Over time, scholars may tire of a phenomenon. While Wikipedia initially garnered significant scholarly attention, scholars now question what can be learned from further exploration, despite the phenomena’s persistence. Thus, the threat of irrelevance should give scholars pause prior to studying novel phenomena.

To convince others of the legitimacy of novel phenomena for theory, scholars may need to dedicate extra real estate in their manuscripts to explain the importance of the phenomenon for the field (Golden-Biddle and Locke, 2006). For example, before scholars could publish theory explaining how entrepreneurial accelerators mitigated bounded rationality (Cohen et al., 2019a), they spent years establishing accelerators as a context relevant to organization scholars (Cohen, 2013). The challenge is how to describe novel phenomenon without bogging down a manuscript. Wanda Orlikowski reflected,

What was lost is a clarity of purpose. . . It feels like the paper has a split identity, a condition brought on by the review process. Perhaps it is what was needed at the time. Perhaps it was obligatory then given the state of the field. . . (Golden-Biddle and Locke, 2006: 109)

Scholars looking to theorize from novel phenomena face a double-edged sword. Too little explanation leaves audiences wanting; too much description crowds room for theorizing

### *Peril 2: burden of novelty*

Novel phenomena offer blue oceans, with many potential streams to pursue to create impact. But blue oceans can overwhelm scholars as they navigate multiple, interdependent research design decisions without a conceptual map. Lack of prior research often results in less field-level knowledge and fewer opportunities for vicarious learning. Without prior work to build upon, scholars need to innovate at every stage of research, introducing perils throughout. For example, the sampling frame may be unclear, the data quality unknown, and instruments untested. Further, it may be difficult to find informed scholars that can offer feedback or advice on proposed research designs.

### *Peril 3: conceptual voids*

When studying novel phenomena, it may not be immediately clear which research community to engage—there may be a conceptual void. Emerging phenomena may defy traditional definitions or cross units of analyses, offering many options as to the appropriate theoretical lens. This can be daunting as selecting a theoretical lens equates to selecting the audience for the research. To illustrate, scholars studying crowdfunding could draw from theories on individual motivation; online communities; or investors’ evaluations of potential investments. As emerging phenomena are often

multivocal, scholars may need to experiment with several “theoretical hats” to determine the best fit. Even after a theoretical hat is chosen, convincing others of the fit is not a one-time activity, but rather an on-going process requiring time-consuming iteration through writing, revising and presenting the research.

## Mitigating the perils of studying novel phenomena

Scholars have experimented with mitigating the perils of studying novel phenomena to create and publish theoretically interesting research. We articulate four strategies that may help mitigate multiple perils: 1) *Leverage comparisons and levels of analyses*; 2) *Locate a puzzle*; 3) *Integrate novel ideas with the familiar*; 4) *Phase novelty*, and 5) *Borrow existing scholarly communities*.

### *Leverage comparisons and levels of analyses*

By definition, novel phenomena lack long histories, legitimized data sources, and validated constructs. One way scholars can mitigate these challenges is through comparative case study methods (Eisenhardt, 1989, 2021; Eisenhardt and Graebner, 2007). Multiple case studies that combine replication with variation can help increase the robustness of findings and combat anecdotal knowledge or assumptions that develop as awareness of the phenomena builds. Bechky and O'Mahony (2015) advocate for varied settings that allow for comparison across features of theoretical interest rather than increasing the sample size. An additional consideration is determining where populations experience the phenomena prior to its becoming widely distributed—as suggested by our opening quotes. For example, Christin (2018) studied web journalists and editors in France and the United States and compared their responses to the intense quantification of online journalism. Surprising variation across both context and organizational hierarchy revealed how cultural differences persisted despite technological convergence. Developing cross-context comparative designs depends on strong networks and project management skills but can help mitigate the burden of novelty when the phenomena are not yet broadly distributed.

Novel phenomena often unfold in complex social systems where it is easy to lose focus. For example, researchers initially found hackathons to be chaotic and lacking structure, but these conditions allowed scholars to compare parallel innovation processes, enriching opportunities for building theory on accelerated innovation (Lifshitz-Assaf et al., 2021). Research designs that trace a large number of “small n” incidences nested within cases can help scholars avoid becoming overwhelmed. Nested designs can increase opportunities for comparison despite lack of longitudinal data. For example, in their study of Innocentive, Jeppesen and Lakhani (2010) focused their analysis at the problem level—explaining the conditions that predicted problem resolution. Massa and O'Mahony's (2021) study of networked activists initially examined all aspects of self-organization—but this proved untenable. By narrowing their examination to 70 raids, nested by phase, they took advantage of replication and comparison to pinpoint differences in control. This grounded approach enabled a more precise theoretical explanation of how different modes of control changed over time. If nested designs are baked into the initial research design, they can provide flexibility in theorizing and publishing later in the process, attenuating the perils associated with studying novel settings.

### *Locate a puzzle*

To mitigate against the peril of navigating a conceptual void, scholars may want to be circumspect as to how they introduce novel contexts, positioning them as a means to answer theoretical puzzles

rather than as a primary motivation. To locate a puzzle that will be theoretically interesting, scholars must know two things well: the phenomena and the literature. Engaging deeply with a phenomena's context before embarking on a research design can help scholars assess its relevance for organization theory. Deep absorption in secondary research such as press articles, white papers, books, blogs or online forums can foster appreciation for the contours of a novel phenomenon. Preliminary field visits with direct observation or pilot interviews with industry pioneers can deepen assessments of the aspects of a novel phenomenon likely to inform theory. To make the most of these visits, we encourage scholars to locate a puzzle that intersects intriguing aspects of the phenomena with gaps or inconsistencies in theory. Scholars need a framework to synthesize where gaps in the literature exist. In what ways does the phenomena contradict extant theory? What is most surprising or counter intuitive?

Only through constant comparison between the phenomena and the literature did Grodal and O'Mahony (2017) locate a puzzle that drove their research question—despite massive financial investment and collaboration in the field of nanotechnology how did the grand challenge of molecular manufacturing become displaced? Locating a puzzle requires precision in drawing contrast between novel phenomena and extant theory. For example, ASQ reviewers initially struggled to understand how crowdsourcing differed from other forms of collaboration. To address this challenge, without compromising room for theorizing, Lifshitz-Assaf (2018) explicitly compared dimensions of crowdsourcing with traditional forms of collaboration. This helped convince reviewers of how the novel context related to more well studied settings. The paper later received the ASQ dissertation award.

Deeply engaging theory to identify debates, limitations or paradoxes can be challenging for junior scholars embedded in the phenomena and uncertain of which theoretical conversations to join. Early feedback from senior scholars can help mitigate the peril of irrelevance and the peril of conceptual voids by inspiring research design revisions while adaptations are still possible. For example, when Siobhan was collecting data on how open source programmers managed challenges stemming from the commercialization of their code, Mark Granovetter, advised that she study corporations engaged in open source as well. This was not initially considered in the research design, but was excellent advice as it enabled capture of opposing perspectives, which enriched the theory generated. Often, misalignment between data and theory can be spotted early by experienced outsiders who lack emotional attachment to a phenomenon. Rather than draft full papers for each iteration, quick prototyping trials via abstracts, tables or figures can make emergent theorizing more accessible to feedback and accelerate iteration (Langley and Ravasi, 2019).

Carving deep theoretical linkages and counterintuitive insight rather than pursue empirically driven questions can increase opportunities for original contributions and insulate against being scooped. Research rooted in theory is timeless. Identifying contemporary analogous contexts or conditions that share underlying features can enhance the relevancy of the theory generated. For example, while Massa and O'Mahony's (2021) research on networked activism was under review, the attack on the US capital unfolded using similar organizing methods which helped establish the relevance of networked activism in other settings.

### *Integrate novel ideas with the familiar*

Once a puzzle is located, balancing familiarity with novelty is core to mitigating the perils of novelty and gaining acceptance. There is always a tension between building upon prior theories and differentiating from those theories—this is endemic to publishing in academia (Locke and Golden-Biddle, 1997). One solution is to aim for optimal levels of distinctiveness (Brewer, 1991), a modest level of originality that combines or extends familiar ideas with distinct innovative ideas. “Baking



differentiation in” to the research design can help generate compelling theoretical contributions as distinctiveness may be more difficult to achieve in the reviewing and revising stages, after the editorial and review team is selected. The aim is to generate enough novelty to be interesting but not so much that new knowledge cannot be absorbed by those unfamiliar with it. What is less clear is how scholars strike this balance.

To assimilate novel phenomena with extant theory, scholars can use analogies, narratives or frames that compare the novel to the familiar (Bingham and Kahl, 2013). By refracting novel phenomena through the lens of mature theory, scholars can pinpoint where misalignment exists to create optimal levels of distinctiveness. For example, Mazmanian et al. (2013) unpacked how the established construct of *autonomy* was challenged in the context of wireless email. Karunakaran et al. (2022) studied how the established construct of *accountability* was reconfigured in the context of social media.

At the same time, scholars need to be wary of invoking constructs or theory that is familiar but too distant from the novel context. To explain novel phenomena to scholarly audiences, scholars often need to induct or name new definitions or constructs. Yet, naming new constructs can accidentally land scholars in theoretical minefields. For example, people collaborating in hackathons or flashmobs may seem like “temporary teams.” Yet, since they are quickly assembled and disassembled, these groups may not meet the traditional definition of a “team.” Thus, the teams literature may not be the appropriate fit. Constructing workarounds, for example, using terms like “participant” rather than team member, can circumvent conceptual confusion from adjacent audiences and maintain data integrity.

Another strategy is to unearth traditional constructs in infrequent usage. For example, to explain how open source programmers gained authority over their work rather than over people, Dahlander and O'Mahony (2011) redefined the concept of “lateral authority” (with thanks to Mary Parker Follett). Alternatively, scholars can modify established constructs, with novel twists. In contrast to Abbott's notion of professionals' boundary *protection* work, Lifshitz-Assaf (2018) identified how professionals engaged in “boundary *dismantling* work.” Finally, scholars can introduce new terms to the literature, inspired by the field. To explain how contract workers acquired jobs without the requisite experience—O'Mahony and Bechky (2006) identified the native construct of “stretch-work”—opportunities that leveraged existing skills and extended them to new areas.

What is challenging is resisting the temptation to overclaim novelty. For example, O'Mahony and Ferraro (2007) were initially excited by the discovery of a cryptographic key ring on an open source project, but, made a strategic choice. Rather than extol the novelty of cryptographic data, they converted it into a standard social network format and drew upon established network analysis and regression methods to explain patterns of leadership and governance over time. The goal of integrating the novel and the familiar is to appeal to existing scholarly communities with familiar theory, constructs or methods while igniting scholars' curiosity by highlighting how novel phenomena challenge or defy extant theory—without overplaying what is novel.

### *Phase the introduction of novelty*

To mitigate the perils of novelty, scholars can design research programs with multiple studies that phase paths to publication. Early studies create common understanding while later studies layer larger theoretical contributions. For example, some scholars publish a 'stepping-stone' paper in a peer reviewed, lower tier journal to build a conceptual foundation that will enhance top tier journals' ability to appreciate the phenomena. Stepping-stone pieces that lay conceptual groundwork in advance of significant theoretical advances need to be carefully scoped so as not to cannibalize the contribution. For example, prior to publishing their award-winning *Administrative Science*

*Quarterly* paper that showed how firm networks can be a locus of innovation (Powell et al., 1996), Powell (1990) published a theory piece in *Research in Organizational Behavior* that articulated how network forms differentiated from market and hierarchical forms. This piece provided a conceptual platform upon which a stream of path breaking empirical work followed. Stepping-stone pieces can be impactful in their own right—Powell's (1990) paper has received more citations than the 1996 ASQ that followed. More recently, an early exploratory quantitative paper on crowdfunding (Mollick, 2014) laid the groundwork for theoretically driven work on gender and expert evaluation (e.g. Greenberg and Mollick, 2017; Mollick and Nanda, 2015). Cohen (2013) laid the groundwork for future theoretical work on accelerators (e.g. Cohen et al., 2019a). Once the relevance of the phenomena is accepted, more complicated questions of theoretical import can be more easily tackled.

### ***Borrow existing scholarly communities***

Communities help diffuse critical noncanonical knowledge and enhance problem solving. But, with novel phenomena, a scholarly community may not yet exist. For example, a team of scholars studying hackathons initially engaged in “forum hopping” to find an audience. Over 3 years, the team explored a variety of scholarly forums devoted to experimentation, innovation, open innovation, 3D printing and temporality until they created a professional development workshop at the Academy of Management. While community building is an enticing way to craft shared understandings, junior scholars, in particular, need to moderate this effort so as not to detract from research-oriented pursuits. Leveraging existing community infrastructure may be key when phenomena are at the earliest stages of emergence and not yet hot.

### **The promise and perils of studying hot phenomena**

The transition from novel to hot can be uneven and does not announce itself. Novel phenomena become hot when a plethora of scholars recognize the relevance of the phenomena, accompanied by competition and a rush for primacy. An influx of scholarly entrants, often from diverse domains, adds their own theoretical perspectives and methodological preferences to the mix. This growth may be triggered by or reinforced by public or media attention. Increased accessibility and plurality of data can expedite this transition—as evidenced by research on patents, crowdfunding, open source software and other forms of platform based research. With more types of data, a wider variety of qualitative and quantitative methods proliferate, fostering creativity and a shift from theory building to theory testing. Fast followers benefit from first movers and can leverage stepping-stone papers, accelerating time to publication. More forums and outlets appear as subareas develop, enhancing opportunities for developing ideas, finding coauthors and gaining feedback. Expectations of research rigor and quality escalate as scholars become knowledgeable enough to critically evaluate scholarly research. This landscape differs from that of novel phenomena and thus the perils differ in ways both subtle and dramatic.

### ***Peril 1: the lure of the hot***

Merton (1979) norms of disinterestedness and skepticism, can be compromised by those motivated to pursue hyped phenomena. An infusion of resources may further attract fast followers who “goal graft” exciting elements of the hot phenomena on to existing research agendas (Grodal and O'Mahony, 2017). Latecomers may not fully appreciate nuances of the phenomena, the quality of



the data available nor how the field is emerging. Without proper theorizing, research on hot phenomena risks being associated only with the phenomena. If the phenomenon falls out of fashion, the research receives less impact. The lure of the hot can lead scholars to rest their theoretical motivation on identifying an empirical gap or relying on scholars' calls for more research. For example, algorithmic control has attracted much interest from a variety of scholars, but reviewers take a hardline on assessing what is theoretically novel with respect to extant theory. Simply showing that an existing theory could apply to an emerging context is insufficient to generate interesting theory.

### *Peril 2: competition and the race for primacy*

Diffused parallel trajectories to study the same hot phenomena can accelerate the race for primacy. The transition from novel to hot can be subtle, complicating scholars' assessments of the career implications of studying hot phenomena. The rush of new entrants leaves scholars unable to clearly identify where both empirical and theoretical gaps exist as research moving through the publication process may not be visible. Over time, opportunities for theory building subside and expectations of methodological rigor rise as theory testing becomes paramount. Overall, hype and competition create another type of peril: that scholarly investment studying the phenomena may not bear fruit.

### *Peril 3: fragmenting the accumulation of new knowledge*

Scholars from different perspectives may approach hot phenomena in varied ways, generating simultaneous, diverse explorations which can fragment the accumulation of new knowledge (e.g., Murray and O'Mahony, 2007). Interest in hot phenomena can shift the substantive boundaries (what it is about) as well as the social boundaries (who is involved). For example, as accelerators grew in number, so too did the organizations that called themselves accelerators—producing variation in quality and definition. At one point, the industry pioneer, Y Combinator, decided that it no longer wanted to be called an accelerator, though it did not change its operations. At the same time, organizations that did not meet the criterion latched on to the moniker.

A similar pattern unfolded when established technology firms launched their own “sponsored,” “hybrid,” or “gated” open source projects, grafting on to the popularity of community based open source software (Shah, 2006; West and O'Mahony, 2008). The peril scholars face is that they might assume they are studying similar phenomena without realizing underlying differences. Variation can inspire interesting research—but only if clear definitions align with the dynamism of the phenomena. Without mechanisms to enable the disclosure of parallel streams of research, the field can progress quickly but in an uneven way—with scholars focused on the most salient aspects of hot phenomena while critical aspects remain under explored.

## **Mitigating the perils of studying hot phenomena**

With hot phenomena, the battle is no longer over whether the phenomenon is relevant enough to contribute to organizational theory but how to collaborate with or differentiate from a burgeoning crowd. Thus, we identify a few strategies that may help mitigate multiple perils: 1) *Supersize data collection*; 2) *Contribute pieces to a puzzle*; 3) *Develop dedicated communities and publishing outlets*; 4) *Foster disclosure of research in progress*; 5) *Manage the race for primacy*.

### *Supersize data collection*

Expect interesting, counterintuitive theoretical contributions to be met with skepticism. The lure of the hot does little to help. One mitigation strategy is to “supersize” the empirical evidence collected to generate interesting theory. For example, when Hila Lifshitz-Assaf was studying crowdsourcing, she anticipated skeptical audiences and thus over collected data longitudinally at three levels of analyses: the professional level, the project level, and the organization level. Supersizing the data collected helps ensure the potential for empirical and theoretical contributions, especially when there is uncertainty as to the path the research may take during analysis, theorizing, revision and review process.

### *Contribute pieces to a puzzle*

Regardless of the enthusiasm for the context, it is critical to maintain informed skepticism to overcome the lure of the hot and contribute pieces to a theoretical puzzle. Simply showing that an existing theory could apply to hot phenomena is insufficient. The onus is on scholars to precisely identify aspects of the phenomena that challenge extant theories to prevent a protracted battle over: “what is the theoretical contribution?” One strategy is to systematically compare mature theories to hot phenomena to identify where violations in expectations occur. For example, in their comparison with prior technological advances, Kellogg et al. (2020) identified four affordances of algorithmic control: comprehensive, instantaneous, interactive and opaque that differed from extant modes of technical control. Showing specifically how algorithmic control differed from existing theory provided new theoretical purchase built upon accepted foundations. Through constant comparison, scholars can identify what transcends the particulars of a research setting and hone on those aspects with the most potential for grounded theoretical insight. To contribute pieces to a puzzle, the theoretical contribution must be the primary hook and not contingent on the phenomena.

### *Develop dedicated communities and publishing outlets*

Community is equally important in the hot phase but the focus shifts from legitimating theoretical relevance to orchestrating a growing, enthusiastic audience. When phenomena are hot, new sub-communities specializing in different aspects of the phenomena emerge. As the field enlarges, scholars supplement panels or workshops tacked on to established venues with dedicated conferences to focus the evolving dialogue and nurture relationships among scholars. Enough of the perils of novelty have dissipated to justify this investment. Dedicated forums allow scholars to present work in progress in vivid ways and obtain feedback—which can help scholars cultivate differentiation and mitigate the peril of knowledge fragmentation. For example, scholars and practitioners convened a small conference at Emory University to build a shared understanding of accelerators which ignited new collaborations and edited volumes (e.g., Roberts and Lall, 2019). A single gathering can spark new coauthor relationships and inspire special issues that celebrate a phenomena’s theoretical potential. Dedicated publishing outlets create a welcome mat for new ideas and accelerate theory building. For example, when open source software became hot, senior scholars like Georg von Krogh and Eric von Hippel issued a call for a special issue in *Research Policy* signaling the types of research questions desired. This helped mitigate the peril of an illegitimate research context by framing open source software as interesting to a range of scholars in policy, economics, and sociology.

### *Foster the disclosure of research in progress*

When phenomena are hot, the challenge is how to ensure an original contribution, given the likelihood of simultaneous exploration and the production of “paper twins” (Bikard, 2020) in the race for primacy. One mitigation strategy is to foster the disclosure of research in progress to help reinforce common definitions; shape the direction of knowledge generation as well as build community. For example, Karim Lakhani, then a doctoral student at MIT, designed a website where scholars studying open source software could upload their working papers. This fostered a community of scholars who quickly learned of the different (or same!) directions others were pursuing. Platforms like SSRN and NBER can also provide this function. When dedicated communities form, scholars develop a better sense of colleagues’ research trajectories and can adapt their research programs to differentiate from others and ensure original contributions. Scholars may veer toward keeping projects close to the vest, but ideas often benefit more from feedback and iteration when exposed to the light.

### *Manage the race for primacy*

When scholars learn of competing research programs, they can either accelerate their own path to publication or collaborate. When parallel author teams, Murray and Fehder and Cohen and Hochberg, discovered that they were all writing about accelerator design, they decided to collaborate to create collective impact (Cohen et al., 2019b). Combining forces can help scholars rise above the hype, especially when authors share temporal urgency. After all, papers written by teams receive twice as many citations as those written by individuals (Ahmadpoor and Jones, 2019). Yet, knowing when to compete and when to collaborate is unclear. Collaboration decisions may be easier to make once the data are collected and authors can identify the unique contributions each party brings to the manuscript. Crafting collaborations on at an early stage, when the direction of the project is less clear, may be more challenging. Introducing a collaborator embedded in the context when it was novel can apprise teams of developments in the field and navigate the competition. Alternatively, if scholars pursue solo authorship, often prized in tenure review, they may want to leverage available data; simplify their contribution to ease the path to publication or showcase a unique perspective—particularly if “stepping-stone” pieces are already or nearly published. More likely, a full research program will balance new and existing collaborations with solo work.

## **Conclusion**

Calls for interesting research never abate (Bartunek et al., 2006; Davis, 1971), but how to produce interesting research is less clear. One popular path is to build theory from emerging phenomena. We have argued that novel and hot phenomena face distinct promises and perils that call for different mitigation strategies. When phenomena are so novel that there is little agreement on its relevance and only a few entrants, what is important is to legitimate and justify the relevance of the phenomena. Once this is accomplished, there is greater need to differentiate and build upon others’ work. This is analogous to competitors collaborating to create legitimacy for a new market and, later, differentiating from each other (Navis and Glynn, 2010). Our framework suggests a similar pattern for scholarship, but different strategies are called for to develop the market for new ideas and differentiate within that market. We offer a modest initial sampling of strategies to help scholars amplify the promise of contributing interesting research from emerging phenomena while mitigating the perils. With the growth of interest in emerging phenomena like bitcoin, blockchain and NFTs, we hope this modest start stimulates further ideas.


## Acknowledgements

The authors thank Rebecca Karp for her input on early-stage ideas and Christine Beckman and Hila Lifshitz-Assaf for their sharp comments, questions and additions. We also thank the panelists and participants of the Academy of Management Professional Development Workshop on Building Novel Theory from Hot Topics: Strategies for Theorizing from Emerging Phenomena. The contents of this publication are solely the responsibility of the authors.

## Funding

The author(s) disclosed receipt of the following financial support for the research, authorship, and/or publication of this article: This work was in part funded by the Ewing Marion Kauffman Foundation and the University of Virginia, Darden Business School Batten Institute.

## ORCID iDs

Siobhan O'Mahony  <https://orcid.org/0000-0001-8289-0374>

Susan Cohen  <https://orcid.org/0000-0002-6867-2204>

## Note

1. <http://news.bbc.co.uk/2/hi/8367957.stm>

## References

- Abrahamson E (1996) Management fashion. *The Academy of Management Review* 21(1): 254.
- Ahmadpoor M and Jones BF (2019) Decoding team and individual impact in science and invention. *Proceedings of the National Academy of Sciences* 116(28): 13885–13890.
- Bamberger PA and Pratt MG (2010) Moving forward by looking back: Reclaiming unconventional research contexts and samples in organizational scholarship. *Academy of Management Journal* 53(4): 665–671.
- Barley SR and Kunda G (1992) Design and devotion: Surges of rational and normative ideologies of control in managerial discourse. *Administrative Science Quarterly* 37(3): 363–399.
- Bartunek JM, Rynes SL and Ireland RD (2006) What makes management research interesting, and why does it matter? *Academy of Management Journal* 49(1): 9–15.
- Bechky BA and O'Mahony S (2015) Leveraging comparative field data for theory generation. In: Elsbach KD and Kramer R (eds) *Handbook of Qualitative Organizational Research*. New York: Routledge, pp. 200–208.
- Bikard M (2020) Idea twins: Simultaneous discoveries as a research tool. *Strategic Management Journal* 41(8): 1528–1543.
- Bingham CB and Kahl SJ (2013) The process of schema emergence: Assimilation, deconstruction, unitization and the plurality of analogies. *Academy of Management Journal* 56(1): 14–34.
- Bremner RP and Eisenhardt KM (2022) Organizing form, experimentation, and performance: Innovation in the nascent civilian drone industry. *Organization Science* 33: 1251–1699.
- Brewer MB (1991) The social self: On being the same and different at the same time. *Personality and Social Psychology Bulletin* 17(5): 475–482.
- Christin A (2018) Counting clicks: Quantification and variation in web journalism in the United States and France. *American Journal of Sociology* 123(5): 1382–1415.
- Cohen SL (2013) What do accelerators do? Insights from incubators and angels. *Innovations: Technology, Governance, Globalization* 8(3–4): 19–25.
- Cohen SL, Bingham CB and Hallen BL (2019a) The role of accelerator designs in mitigating bounded rationality in new ventures. *Administrative Science Quarterly* 64(1): 810–854.
- Cohen SL, Fehder DC, Hochberg YV, et al. (2019b) The design of startup accelerators. *Research Policy* 48(7): 1781–1797.

- Contigiani A and Levinthal DA (2019) Situating the construct of lean start-up: Adjacent conversations and possible future directions. *Industrial and Corporate Change* 28(3): 551–564.
- Dahlander L and O'Mahony S (2011) Progressing to the center: Coordinating project work. *Organization Science* 22(4): 961–979.
- Davis MS (1971) That's interesting! Towards a phenomenology of sociology and a sociology of phenomenology. *Philosophy of the Social Sciences* 1(2): 309–344.
- Edmondson AC and McManus S (2007) Methodological fit in management field research. *The Academy of Management Review* 32(4): 1155–1179.
- Eisenhardt KM (1989) Building theories from case study research. *The Academy of Management Review* 14(4): 532–550.
- Eisenhardt KM (2021) What is the Eisenhardt Method, really? *Strategic Organization* 19(1): 147–160.
- Eisenhardt KM and Graebner ME (2007) Theory building from cases: Opportunities and challenges. *The Academy of Management Journal* 50(1): 25–32.
- Elsbach KD and Kramer RM (2015) In search of innovative pathways and methods in qualitative research. In: Elsbach KD and Kramer R (eds) *Handbook of Qualitative Organizational Research*. New York: Routledge, pp. 3–8.
- Ferraro F, Etzion D and Gehman J (2015) Tackling grand challenges pragmatically: Robust action revisited. *Organization Studies* 36(3): 363–390.
- Golden-Biddle K and Locke K (2006) *Composing Qualitative Research*. Thousand Oaks, CA: SAGE.
- Greenberg J and Mollick E (2017) Activist choice homophily and the crowdfunding of female founders. *Administrative Science Quarterly* 62(2): 341–374.
- Grodal S and O'Mahony S (2017) How does a grand challenge become displaced? Explaining the duality of field mobilization. *Academy of Management Journal* 60(5): 1801–1827.
- Jeppesen LB and Lakhani KR (2010) Marginality and problem-solving effectiveness in broadcast search. *Organization Science* 21(5): 1016–1033.
- Karunakaran A, Orlikowski WJ and Scott SV (2022) Crowd-based accountability: Examining how social media commentary reconfigures organizational accountability. *Organization Science* 33(1): 170–193.
- Kellogg KC, Valentine MA and Christin A (2020) Algorithms at work: The new contested terrain of control. *Academy of Management Annals* 14(1): 366–410.
- Langley A and Ravasi D (2019) Visual artefacts as tools for analysis and theorizing. In: Zilber TB, Amis JM and Mair J (eds) *Research in the Sociology of Organizations*. Bingley: Emerald Publishing, pp. 173–199.
- Leatherbee M and Katila R (2020) The lean startup method: Early-stage teams and hypothesis-based probing of business ideas. *Strategic Entrepreneurship Journal* 14(4): 570–593.
- Leavitt K, Schabram K, Hariharan P, et al. (2021) Ghost in the machine: On organizational theory in the age of machine learning. *Academy of Management Review* 46(4): 750–777.
- Lifshitz-Assaf H (2018) Dismantling knowledge boundaries at NASA: The critical role of professional identity in open innovation. *Administrative Science Quarterly* 63(4): 746–782.
- Lifshitz-Assaf H, Lebovitz S and Zalmanson L (2021) Minimal and adaptive coordination: How hackathons' projects accelerate innovation without killing it. *Academy of Management Journal* 64(3): 684–715.
- Locke K and Golden-Biddle K (1997) Constructing opportunities for contribution: Structuring intertextual coherence and “problematizing” in organizational studies. *Academy of Management Journal* 40(5): 1023–1062.
- Massa FG and O'Mahony S (2021) Order from chaos: How networked activists self-organize by creating a participation architecture. *Administrative Science Quarterly* 66(4): 1037–1083.
- Mazmanian M, Orlikowski WJ and Yates J (2013) The autonomy paradox: The implications of mobile email devices for knowledge professionals. *Organization Science* 24(5): 1337–1357.
- Merton RK (1979) *The Sociology of Science: Theoretical and Empirical Investigations* (ed NW Storer). Chicago, IL: University of Chicago Press.
- Mollick E (2014) The dynamics of crowdfunding: An exploratory study. *Journal of Business Venturing* 29(1): 1–16.
- Mollick E and Nanda R (2015) Wisdom or madness? Comparing crowds with expert evaluation in funding the arts. *Management Science* 62(6): 1533–1553.

- Murray F and O'Mahony S (2007) Exploring the foundations of cumulative innovation: Implications for organization science. *Organization Science* 18(6): 1006–1021.
- Navis C and Glynn MA (2010) How new market categories emerge: Temporal dynamics of legitimacy, identity, and entrepreneurship in satellite radio, 1990–2005. *Administrative Science Quarterly* 55(3): 439–471.
- O'Mahony S and Bechky BA (2006) Stretchwork: Managing the career progression paradox in external labor markets. *Academy of Management Journal* 49(5): 918–941.
- O'Mahony S and Ferraro F (2007) The emergence of governance in an open source community. *Academy of Management Journal* 50(5): 1079–1106.
- Ployhart RE and Bartunek JM (2019) Editors' comments: There is nothing so theoretical as good practice—A call for phenomenal theory. *Academy of Management Review* 44(3): 493–497.
- Pontikes EG and Rindova VP (2020) Shaping markets through temporal, constructive, and interactive agency. *Strategy Science* 5(3): 149–159.
- Powell WV (1990) Neither market nor hierarchy: Network forms of organization. *Research in Organizational Behavior* 12: 295–336.
- Powell WW, Koput KW and Smith-Doerr L (1996) Interorganizational collaboration and the locus of innovation: Networks of learning in biotechnology. *Administrative Science Quarterly* 41: 116–145.
- Roberts PW and Lall SA (2019) Entrepreneurs, ecosystems, and accelerators. In: Roberts PW and Lall SA (eds) *Observing Acceleration: Uncovering the Effects of Accelerators on Impact-Oriented Entrepreneurs*. Berlin: Springer, pp. 13–29.
- Shah SK (2006) Motivation, governance, and the viability of hybrid forms in open source software development. *Management Science* 52: 1000–1014.
- Sutton RI (1997) Crossroads—The virtues of closet qualitative research. *Organization Science* 8(1): 97–106.
- Sutton RI and Staw BM (1995) What theory is not. *Administrative Science Quarterly* 40(3): 371–384.
- Teece DJ (2010) Business models, business strategy and innovation. *Long Range Planning* 43(2–3): 172–194.
- Von Krogh G, Rossi-Lamastra C and Haefliger S (2012) Phenomenon-based research in management and organisation science: When is it rigorous and does it matter? *Long Range Planning* 45(4): 277–298.
- West J and O'Mahony S (2008) The role of participation architecture in growing sponsored open source communities. *Industry and Innovation* 15(2): 145–168.
- Zott C and Amit R (2008) The fit between product market strategy and business model: Implications for firm performance. *Strategic Management Journal* 29(1): 1–26.
- Zuzul T and Edmondson AC (2016) The advocacy trap: When legitimacy building inhibits organizational learning. *Academy of Management Discoveries* 3(3): 302–321.
- Zuzul T and Tripsas M (2020) Start-up inertia versus flexibility: The role of founder identity in a nascent industry. *Administrative Science Quarterly* 65(2): 395–433.

## Author biographies

Siobhan O'Mahony is the Feld Family Professor in Innovation and Entrepreneurship at Boston University and an Associate Editor at *Administrative Science Quarterly*. She has spent two decades studying emerging phenomena but still can be naïve as to the perils. Her research frequently examines how creative or technical experts produce collaborative outcomes without the support of traditional organizations and how collectives self-govern amid the tensions of growth. She has a PhD in Management Science and Engineering from Stanford University, and an MPA and BA from Cornell University.

Susan Cohen is an Assistant Professor in the Department of Management at the University of Georgia's Terry College of Business. Cohen's research explores innovation, organizational learning, and strategy formation in novel research contexts, primarily seed accelerator programs. She has wrestled with the perils of studying novel topics for many years. She earned her PhD in Strategy and Entrepreneurship from the University of North Carolina at Chapel Hill, BA in Mathematics from the University of Michigan and MBA from the Kellogg School of Management.