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ELEMENTA
Science of the Anthropocene

REVIEW

The emergence of convergence

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Science is increasingly a collaborative pursuit. Although the modern scientific enterprise owes much to individuals working at the core of their field, humanity is increasingly confronted by highly complex problems that require the integration of a variety of disciplinary and methodological expertise. In 2016, the U.S. National Science Foundation launched an initiative prioritizing support for *convergence research* as a means of "solving vexing research problems, in particular, complex problems focusing on societal needs." We discuss our understanding of the objectives of convergence research and describe in detail the conditions and processes likely to generate successful convergence research. We use our recent experience as participants in a convergence workshop series focused on resilience in the Arctic to highlight key points. The emergence of resilience science over the past 50 years is presented as a successful contemporary example of the emergence of convergence. We close by describing some of the challenges to the development of convergence research, such as timescales and discounting the future, appropriate metrics of success, allocation issues, and funding agency requirements.

Keywords: Convergence research, Transdisciplinary, Resilience, Wicked problems, Complex social-ecological systems

Introduction

Science is increasingly a collaborative pursuit (Gibbons et al., 1994). Although the modern scientific enterprise owes much to individuals working within the core of their discipline, humanity is increasingly confronted by highly complex social-ecological systems problems. These problems are unlikely to be solved with siloed disciplinary and context-specific approaches and will instead require the integration of multiple disciplinary and methodological expertise. In recognition of this, advances in integrative approaches have sought to more effectively and substantively bring together the methodological expertise of a variety of disciplines, moving from disciplinary to multi-, inter-, and trans-disciplinary approaches (Lang et al., 2012). In 2016, the U.S. National Science Foundation (NSF) launched an initiative prioritizing support for *convergence research* as a means of "solving vexing research problems, in particular complex problems focusing on societal needs" (https://www.nsf.gov/od/oia/convergence/index.jsp). Convergence research builds upon transdisciplinary approaches to integrating knowledge across multiple disciplines to address wicked and compelling social problems (e.g., climate change, pandemics) by explicitly aspiring to develop new science that may emerge from and may comprise more holistic knowledge than the sum of individual disciplines (Arnold and Bowman, 2021).

The authors were involved in a series of Navigating the New Arctic (NNA) convergence workshops (NSF solicitation 20-514) focused on understanding resilience and change in the Arctic. Most convergence research to date has focused on integrating across the health sciences, nanotechnology, biotechnology, and information technology (https://www.nsf.gov/od/oia/convergence/exemplars.jsp). More recently, however, there has been a growing focus on cross-cutting earth science problems, including navigating the changing social, environmental, and climatological conditions of the Arctic (Wilson, 2019). The rapid rate of global change and the degree to which regional and even local problems directly impact global processes have forced the development of new scientific approaches capable of addressing problems that have scaled up to affect all of Earth.

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Problems facing the Arctic, such as increased weather variability, loss of sea ice, melting of permafrost, changing animal distributions and migration patterns, and rapid social change, exemplify the complexity, speed, and scale of challenges emerging at the nexus of rapid climate, land use, and societal change (Rantanen et al., 2022). Because of the strong potential to create and foster innovation, convergence research may provide opportunities to confront and navigate Arctic change and other complex socio-ecological challenges facing humankind.

We briefly describe the NNA convergence workshop series that inspired this article, and then answer the following questions: (1) What is convergence research? (2) When is a convergence process the appropriate approach? (3) What should a convergence process look like? (4) Are there examples of successful convergence research? (5) What kinds of challenges are there in the execution of a convergence approach? We use examples from our recent experience in the NNA convergence workshop series to highlight our discussion throughout the article.

Navigating the New Arctic workshop series

The overarching focus of a series of 3 U.S. NSF-funded convergence workshops was to address the urgent need to better predict and manage rapid changes in the Arctic, with the objective of reducing inequitable and undesirable outcomes for people and nature. In particular, Indigenous traditional knowledge is often left out or isolated in the Arctic narrative, risking critical understanding and pathways toward equitable and desirable outcomes in the New Arctic. We used a complex systems scientific framework because it allows insight into general system dynamics while transcending the particular details. Such an approach requires an in-depth appraisal of diverse system components and a convergence of data and knowledge from disparate fields, requiring expert knowledge from many areas and disciplines.

The workshops convened participants with relevant, disparate expertise to converge around 6 thematic goals and commenced with listening sessions led by Indigenous and other Arctic stakeholders with purposeful pauses for discourse, reflection, and trust building. The 6 themes were (1) resilience of local Arctic variables to global-scale drivers; (2) Indigenous Traditional Knowledge: incorporating Indigenous Peoples' experiential and observational knowledge into a complex Arctic system framework; (3) biogeochemical cycling: a synthetic approach to understanding change in the Far North; (4) identifying spatial regimes in the Arctic to detect past and approaching thresholds with statistical and remote sensing tools; (5) the Arctic and agroecosystem mid-latitude connection: complex spatial and temporal feedbacks; and (6) novel, convergent tools for understanding adaptive capacity and resilience in an Arctic in transition.

These workshops provided a venue where alternative viewpoints and traditional and disciplinary understanding could be voiced to enhance our knowledge of change in the New Arctic. We explored alternative scenarios resulting from the loss of Arctic resilience that have occurred or are underway, sources of adaptive capacity and pathways for transformation to desired states, and methods to enhance the resilience of desirable states. Our results were communicated to Indigenous and other stakeholders in the Arctic.

Convergence research

Convergence research was developed over the past several decades by U.S. federal agencies and international partners as a response to the rapidity and irreversibility of change in science and technology (Roco et al., 2013). More recently, it has evolved to include a deliberate focus on "supporting societal values and needs" (Roco et al., 2013). Convergence research is meant to go beyond transdisciplinary research, or the integration of multiple viewpoints, expertise, and disciplines. Convergence research works across disciplinary boundaries to deeply integrate multiple perspectives, expertise, knowledge, methods, tools, and analytical approaches into synthetic, high-level frameworks in order to solve complex intellectual questions confronting humanity (National Research Council, 2014; National Science Foundation, 2016). These frameworks represent a "converged" or shared vision of a problem that can facilitate the generation of new science and address problems vexing society. In other words, the goal is not the reduction of multiple views into one constrained, shared space but to stimulate the emergence of something larger than the sum of the parts through the process of bringing together and integrating bodies of specialized knowledge—to generate emergent knowledge and innovation greater than those of siloed disciplines (National Research Council, 2014; Eyre et al., 2021). We argue that the hardest to achieve but most desirable measure of success for a convergence process is the emergence of novel science or scientific approaches which represent a collection of new ways of framing key dynamics and behavior and methodologies to cope with this expansion of perspective and framing.

However, a convergence process is also one that is fundamentally riddled with uncertainty. For starters, there is no prescribed formula for building this integrative, collaborative process, and the people, approaches, and methods that are integrated may vary widely for different complex social-ecological challenges (Angeler et al., 2020a). Despite the well-intentioned desire to organize convergence research around a specific and predefined problem, bringing multiple disciplines into one room means that the definition of the problem itself is nonstationary and will inevitably change due to the broadening of perspectives that occurs as a result of collectively working on a shared problem, as well as with the passage of time or from additional participants. Convergence research enhances reflexivity, a process for introspection that is inherent in the construction of meaning by individuals (Bourdieu, 2003). Reflexivity in a conservation application can be thought of as a "continual and intentional interrogation of how one's role as a scientist influences the scientific process by looking inward to understand our own values, purposes, and influences, outward to understand relationships with others and understandings of others, backward to understand lessons from the past, and forward to understand future impacts" (Beck et al., 2021). Convergence processes, though not formulaic, cannot succeed without meaningful engagement and collaboration with disciplines and stakeholder groups often far outside our own, which requires self-awareness, transparency, open-mindedness, and respect about values and biases (Lélé and Norgaard, 2005). Although these factors, together with preparedness, opportunity, and desire, play an important role in the convergence process, outcomes in terms of novel solutions and innovative approaches can be accidental and serendipitous (Gaughan, 2010).

In our Arctic experience, we charged ourselves with the task of exploring potential pathways to equitable and desirable outcomes for people and nature in light of climate change-driven disruptions. The nature of the problem as understood by ecologists was quite different from the respective understandings of Indigenous Elders, engineers, social scientists, local communities, or human health experts. The challenge of such an expansion of the problem space is that it can also expand the scope and range of possible solutions in a way that can make the problem less tractable despite more realistically reflecting the complexity of socio-ecological problems.

Solutions from single disciplines can therefore be attractive but can also reflect spurious certitude and run the risk of unanticipated and undesired outcomes that are often experienced as surprise. This can happen when we treat a complex system with many interacting components and nonlinear behavior as if it only has predictable, linear dynamics and then are surprised when system response to a manipulation or disturbance is outside our expectations. Engineering solutions, for example, have frequently been suggested for complex challenges. To address coral reef bleaching, Australia is currently considering cool water injections to near-surface reefs to keep the corals from dissociating (Baird et al., 2020). Similarly, state and federal agencies in the American Midwest are currently herbiciding and plowing a fifth-order stream, the Platte River, to maintain open sandbar habitats (Birge et al., 2019; Allen, personal observation). These coercive and reductionist approaches expend considerable resources while failing to address the underlying cause and are often undermined by unexpected processes and outcomes because they do not account for unintended consequences or interactions with other system components (Angeler et al., 2020b).

It is clear that there are few singular solutions, in part because there are few singular problems. The most vexing problems facing humanity today occur in complex socio-ecological systems, where many independent entities, processes, and structures interact across multiple spatial and temporal scales. These problems are further aggravated by current disciplinary models that do not account for such complexity and prioritize linear growth. We argue that single-discipline solutions for problems that actually require a convergence approach will ultimately fail once humanity's will and capital to coerce the managed social-ecological systems ends (Angeler et al., 2020b) or when a surprise occurs (e.g., sudden regime shifts like coral reefs irreversibly changing to an algae-dominated system). Single-discipline solutions may also fail to capture the moral and ethical dimensions of a "problem" and "solution"—namely, who benefits or is potentially harmed. Political, spatial, racial, gender, historical, and other injustices resulting from the power structures of natural resource management are a crucial component of understanding and meeting complex socio-ecological challenges (Chan et al., 2007; Chambers et al., 2021; Massarella et al., 2021).

Approaching these complex problems with simple solutions is at best incomplete and at worst leads to unintended consequences (Holling and Meffe, 1996). It is also critical to recognize that wicked problems rarely have solutions that can halt and revert systems back to their "original" state. Instead, it is often more appropriate to say that there is a suite of probable system trajectories that are individually shaped by human interactions with the changing environment. Thus, realistic solutions proactively and adaptively navigate wicked problems while developing scenarios for creating the preparedness to react if surprises occur (Herrmann et al., 2021). Different management actions or societal choices shape the dynamics of social-ecological systems and once an approach is implemented, it automatically precludes other approaches (Alrøe and Noe, 2016). The uncertainty in these dynamics is high and fundamentally irreducible. Although we recognize that scientists, society, and policymakers often prefer clearly defined problems with clear metrics for success, convergence problems require broad approaches to problem definition and metrics of success and solutions. There are a range of perceptional, institutional, funding, and other challenges which leave little room for creative and intuitive thinking and that need to be addressed to make convergence processes effective and create the space for novel science to emerge.

When is convergence the right approach?

The most essential precondition for a convergence approach is sufficient problem complexity. Some problems are small or focused enough for a disciplinary solution or process. For example, during the current COVID-19 global pandemic, solving shortages in medical supplies or calculating death rates may be complicated, but are not necessarily complex because the problems are known and can be unambiguously quantified. Modeling the spread of the disease, however, is complex because political, geographic, epidemiological, medical, behavioral, and psychological domains interact in unexpected and unpredictable ways.

Problems that require a convergence approach are those that involve nonlinearity, unpredictability, and irreducible uncertainty in system behavior and dynamics, all of which are heightened when there is tight coupling between multiple complex systems (Sundstrom et al., 2023). Climate change is an obvious example, as the impacts of climate change intersect with every social and ecological system, spanning all levels of organization. Food security is another important example. Modern food systems are increasingly reliant on industrialized agriculture, which has dramatically increased the efficiency of food production but has also had increasingly costly impacts on ecological and social systems. The failure of the Texas power grid in February 2021 demonstrates that a singular focus on efficiency can fail spectacularly when a system is confronted by an unplanned-for disturbance; in this case, a colder-than-expected winter storm. Efficiency is often the goal of engineering approaches in which systems are expected to behave in a predictable and consistent manner and disturbances are expected to fall within a known range of variance. Systems where social, ecological, and economic elements are strongly interconnected and codependent are ripe for convergence approaches, and this includes designing a more resilient power grid or agricultural systems.

Nonstationary systems (where not only system components but also the whole system dynamically changes) will also generally benefit from a convergence approach. Too often, problems are assessed as if the system is static and at equilibrium. For example, ecosystem restoration often presumes that a degraded system can be returned to its previous undegraded state with sufficient time and inputs. This fails to account for the fact that complex systems have thresholds or tipping points that, when crossed, may not allow the system to return to its previous state (Duarte et al., 2009). For example, cloud forests are highly vulnerable to transitioning to a non-forest condition with logging because the removal of trees changes rainfall patterns—the trees create their own precipitation microclimate. Reduced rainfall means the trees cannot regenerate and forests instead transition to grassland (Hildebrandt and Eltahir, 2006, 2008). Increased variability in underlying system conditions means increased uncertainty regarding system behavior and dynamics and the increased risk of crossing potentially irreversible and catastrophic thresholds (Scheffer, 2009). Although all complex systems—be they ecological, social, or other—are dynamic and variable over time, they do not all change at similar rates. Convergence research may be particularly valuable as an approach to confronting problems in systems experiencing rapid change and therefore high uncertainty because convergence processes can promote innovative views about the broad ramifications of change and facilitate the assessment of an issue from a variety of perspectives.

What should a convergence process look like?

Angeler et al. (2020a) describe 2 interdependent scientific approaches drawn from Szent-Györgyi's (1972) discussion of Apollonian and Dionysian perspectives. We argue that iterative cycling between focused (Apollonian) and transcendent (Dionysian) processes represents the optimal approach to convergence. Focused inquiries center on the "of what, to what, and for whom" of science (Angeler et al., 2020a). It is a more traditional and familiar approach, as it is a "goal-oriented, logical and structured questioning process about what we know that we do not know to fill existing knowledge gaps" (Angeler et al., 2020a). Transcendent inquiry, on the other hand, prioritizes intuition and creativity in moving toward discoveries that can be unexpected or even accidental. Szent-Gyorgyi (1972) argued that the Dionysian knows "only the direction in which he wants to go out into the unknown; he has no idea what he is going to find there and how he is going to find it." Lest this sound like an altogether fruitless way to conduct scientific inquiry, virtually all of the major technological discoveries that have transformed modern civilization in the early 21st century emerged from the unstructured, transcendent pursuit of knowledge (Dijkgraaf, 2017). In many cases, these discoveries had no apparent value or application beyond the satisfaction of basic scientific curiosity until many decades later, when they eventually proved to be instrumental in technological development and innovation (e.g., Einstein's general theory of relativity leading to GPS navigation or quantum mechanics leading to quantum supremacy in computing). Dionysian inquiry is a critical and necessary complement to more traditional, focused science and has proven its worth repeatedly when viewed through the lens of a sufficient passage of time (Flexner, 2017).

Science often involves iteration between phases like these. Scholars discuss cycling between theory building and theory testing (e.g., Colquitt and Zapata-Phelan, 2007), generating and testing hypotheses (e.g., Hartwick and Barki, 1994), and avoiding false and missed discoveries (Type I and Type II errors; Holling and Allen, 2002). These cycles map well on to the cycling we propose here. Focused science excels at solving discrete and tangible "problems" like testing existing theory and hypotheses and avoiding false discoveries. Transcendent inquiry, however, is where "vision, intuition, soul, and artistry" happen (Parker and Hackett, 2012) and where innovation, novelty, and new science are more likely to emerge. As these latter outcomes represent the ultimate goals of a convergence process, explicitly incorporating transcendent inquiry into a convergence process becomes critical. Too often, science must prove its worth before it is even conducted—there must be a known outcome, which requires the ability to clearly define the problem, the path to a solution, and the scope of the potential solution. This focused approach is an important part of the scientific process, but overemphasizing it necessarily constrains what is possible. It reduces the chances of serendipity, unexpected accidental insight, and unconstrained inquiry because it defines the potential answers when we often do not even know how to define the problem. This is particularly true in the case of wicked social-ecological problems, where what is viewed as the "problem" can change in fundamental ways from one stakeholder to another or throughout the scientific process. However, endless unconstrained inquiry that is never directed toward a particular goal is also unlikely to produce meaningful outcomes, at least in a timely manner. We argue that the development of new science and successful convergence processes requires intentional movement between focused inquiry and transcendent activities meant to facilitate insight and novelty, which can help recalibrate targeted focused inquiry and consequently stimulate further lines of transcendent inquiry.

Phases in a convergence process

In general, we argue against starting convergence research in a focused phase. Beginning with a tightly scoped phase of hypothesis testing does not naturally allow the space for the problem definition to expand, as the specificity at the heart of a focused phase is fundamental for providing a concrete contribution but is also the source of its greatest limitation. Thus, although a focused phase is more likely to be productive in the traditional sense, in that clearly defined analytical goals will be met and will result in traditional products (such as manuscripts and new proposal applications), these activities are unlikely to provoke a transformative understanding of a novel problem. Starting with a transcendent phase avoids the pitfalls of making assumptions about the scope or scale of the "problem," facilitates the integration of an interdisciplinary group of stakeholders, and allows the participants to position the problem within a broader understanding, thereby creating more opportunities for reinterpretation and contextualization with a more expansive perspective than is possible in a focused phase. During an initial transcendent phase, details such as the sequence in which the voices in the room are heard and the time allocated to each of them could define the trajectory of the endeavor. For example, starting with underrepresented voices can lead to very different perspectives and highlight new or at least not well-trodden paths. This process should generate more questions than answers, as allowing the problem to be defined from diverse perspectives offers the chance to broaden the discussion and evaluate how focused research can best inform and be informed by a more expansive view that transcends disciplinary approaches, contexts, problems, teams, and paradigms. Incorporating the learning achieved in one phase into the other should manifest in a broadening that can create the opportunity for genuine novelty to develop.

Iterative movement between the 2 phases is then necessary, because the result of these first 2 phases should be the unveiling of multiple lines of inquiry: the original inclusion of diverse viewpoints allows for a broad problem space for the initial round of testing hypotheses which, in turn, exposes the potential lack of methods and frameworks that can be brought to bear on the nature of the wicked problem. Fixed alternations between the phases may be inappropriately deterministic, as the generation of innovation and novelty obviously does not follow a recipe. However, the space for transcendent discovery can be deliberately created. This includes collaborative conditions that foster open-minded inquiry, a safe space for exploring novel theories, and a broadening of perspective that facilitates epiphanies. The resilience science discussion below gives examples of how Holling and his collaborators achieved this, but our own experiences in transdisciplinary collaborations as well as from the literature suggest that building high levels of trust through positive, open, fun, and respectful interactions guided by supportive and strong leadership are essential to create space for risky and creative intellectual propositions (Parker and Hackett, 2012; Specht and Crowston, 2022). Movement between the phases can be purposeful, but transcendent discovery cannot be forced. Eventually, exploration of these phases, which should deeply inform each other, should also expose the outer limits of the problem, such as where the coupling between new system elements under consideration is sufficiently weak that participants can draw a boundary.

The distinction between focused and transcendent phases of science is not particular to convergence research. What is particular to convergence is that each new transcendent phase should see a broadening of the problem/solution space (**Figure 1**). Thus, every movement from focused to transcendent should meaningfully

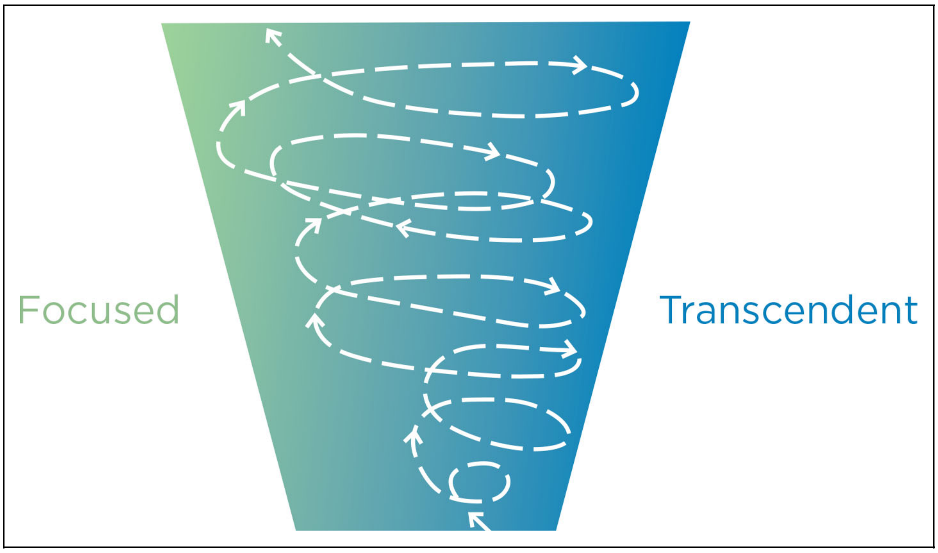

Figure 1. A convergence process moves between phases of focused and transcendent inquiry. The process should be continually expanding to accommodate shared learning, the scales under consideration, diverse perspectives, and an expanded view of the problem and potential solutions. The outcomes include both personal and intellectual transformation, new methods, models and tools, and new science.

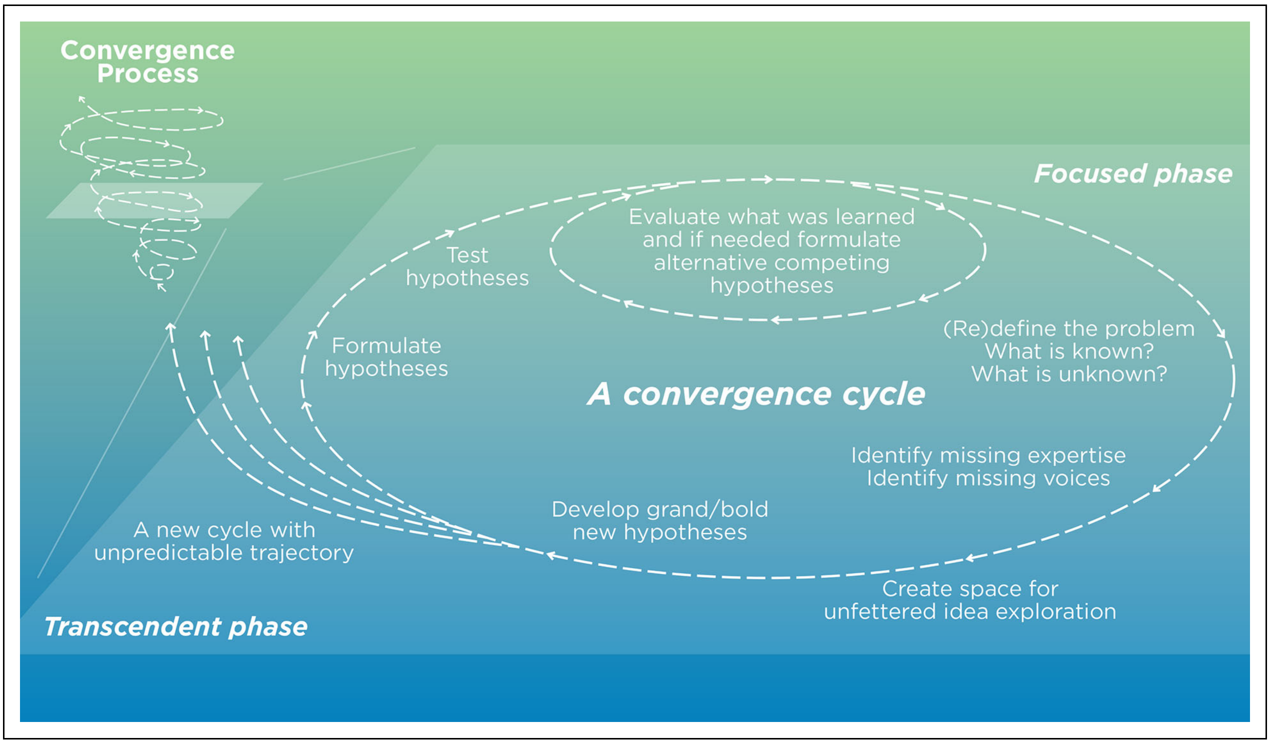

Figure 2. A slice of one cycle of a convergence process, as participants move through a focused phase (light green upper half of diagram) that begins with formulating hypotheses, and then down into a transcendent phase (blue bottom half of diagram). Where the convergence process goes next is context dependent, therefore uncertain. Participants could choose to stay in the transcendent phase or move into one or more parallel focused phases.

integrate a broader and richer collection of perspectives and, as a consequence, every step from transcendent to focused will result in a greater probability of innovation. It is likely that a transcendent phase can spur parallel focused inquiries, especially for wicked problems that have many interacting coupled social-ecological components. What is critical is that there is a formal mechanism for repeatedly bringing participants back together to share their learning and leverage it across the entire community of stakeholders relevant to the wicked problem. This requires a long-term perspective, which we will discuss in more detail later.

Convergence research is therefore a science that seeks to deliberately increase the scope of the perspectives and approaches that are integrated in a meaningful way into the research process, especially during the transcendent phase. Thus, research that begins with a wicked problem requires iterative steps that both broaden the scoping of what is understood to be part of the problem, and increasingly deep and focused hypothesis testing that from the beginning represents a plurality of views, concepts, and a recognition of the strong coupling across systems impacted by the problem. The other half of the process requires the transformational integration of an ever-increasing set of perspectives to consider the findings of those focused efforts, to contextualize them within a broader understanding of how the world works, and, from that, to develop new and transformational research questions for new focused inquiries that may require different teams and methods (**Figure 2**).

The emergence of resilience science

Undertaking scientific problem-solving via a convergence approach can be high risk, and require significant investments of time, the transformation of institutions, and limited methodological clarity relative to traditional science. The emergence of the concept of ecological resilience, and its subsequent maturation into new science that has penetrated a variety of different knowledge domains, stands as a useful contemporary example of a successful process of convergence. Holling's seminal paper (1973) on ecological resilience has been cited more than 20,000 times. It should be noted, however, that the emergence of resilience science as a new and integrative field to address complex problems emerged organically, without any higher-level coordination, support or intentionality, which stands in contrast to the convergence-specific NSF funding programs currently available in the United States.

The development of resilience science required the hybridization of ecological, social, and economic theories and perspectives (among others), but also the development of novel approaches, methods, and ideas. These novel ideas and novel combinations of existing theory and their applications to newly recognized phenomena gave rise to an emergent body of theory including concepts such as ecological resilience, adaptive management, adaptive governance, panarchy, adaptive cycles, regime shifts, and others (Walters, 1986; Gunderson and Holling, 2002; Allen and Garmestani, 2015; Gunderson et al., 2021) that are more than the sum of the parts. Resilience science is now a comprehensive suite of independent but tightly

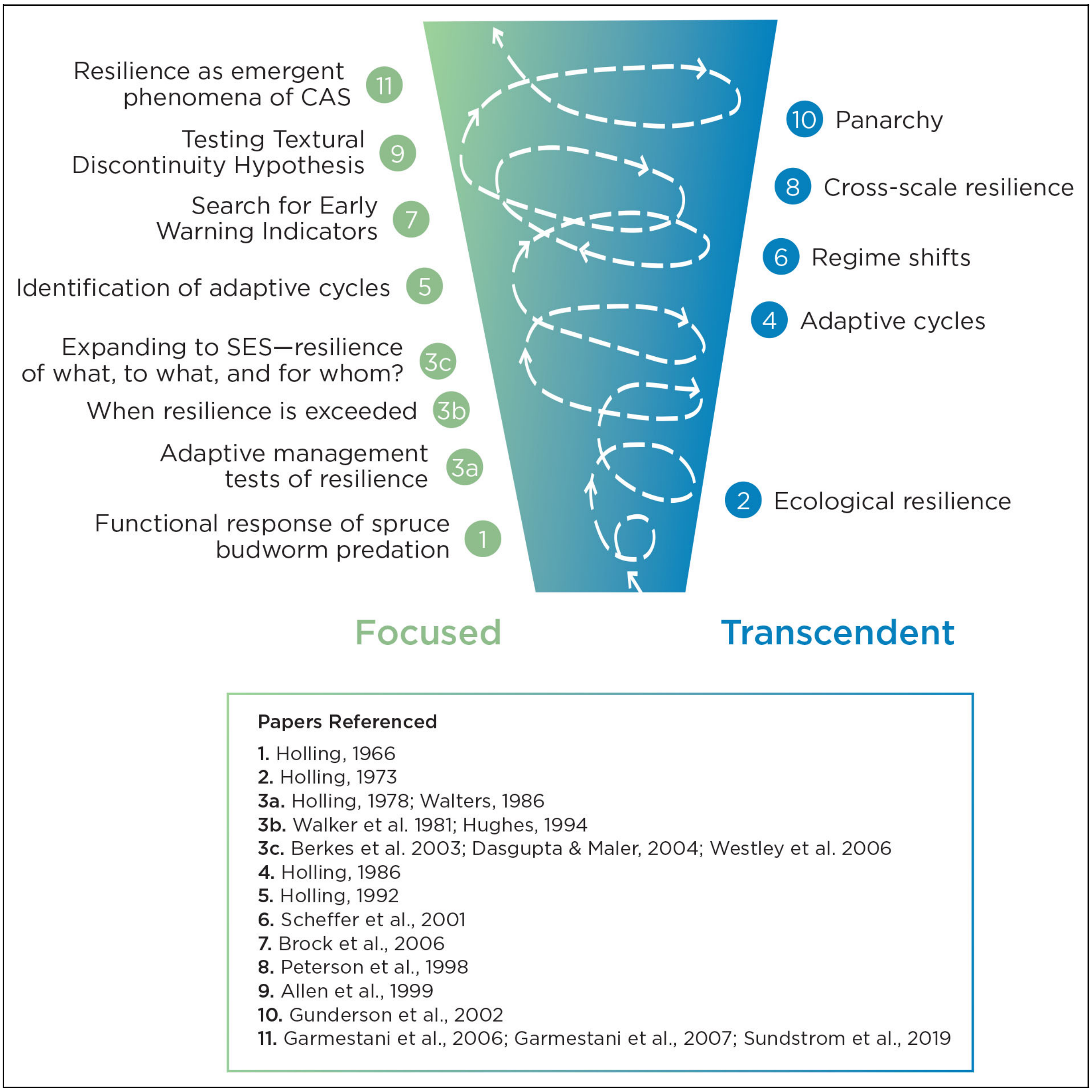

Figure 3. The emergence of resilience science via a convergence process, 1966–2019, with movement between focused and transcendent scientific inquiry. The origins of resilience science begin with a focused research question on spruce budworm predation (number 1, bottom left of tornado in green). Numbered movement up the tornado signifies the approximate chronological order of major developments in resilience science and an example of a significant publication associated with that development (see list of Papers Referenced). Transcendent items represent wholly new scientific propositions, while focused items represent major testable hypotheses. The development and maturation of resilience science represents a clear broadening of the space of inquiry from insect predation dynamics in British Columbia, Canada (Item 1) to social-ecological systems (SES) (Item 3c), and most recently to other types of complex adaptive systems (CAS; Item 11).

coupled concepts transforming how we conceptualize, study, analyze, and manage complex adaptive systems, which include virtually all systems of interest to humans (**Figure 3**).

Resilience science has its origins in attempts to explain simple phenomenon observed in nature (cycling in spruce budworm outbreaks). As such, it was originally a focused science pursuit. Holling (1966) observed cycling patterns in spruce budworm outbreaks and used those observations as the catalyzing seed for a transformative theory on ecological stability and resilience that has deeply influenced how we understand the dynamics and behavior of complex systems (Holling, 1973). This took time, however. Holling's group of core collaborators grew over the decades and deliberately expanded to include scientists from a diversity of fields and even of opposing viewpoints (e.g., a Santa Fe Institute workshop in 2004 that included opponents of a particular hypothesis; Allen and Holling, 2008). As the theory matured and critical mass built among academics and others discussing and applying the nascent theory, the theory became more inherently—and explicitly—complex and has thus become more suitable to address a broad range of societal challenges, such as food security and the management of social-ecological systems.

The growth of resilience as a new science has been well-documented (Parker and Hackett, 2012) and epitomizes a successful convergence approach. The success was due to a diligent and purposeful "flip" of space and time approaches from focused to transcendent by Holling and his collaborators, and a purposeful scaling up of the approach to more complex problems (problems of complex adaptive systems, rather than reductionist problems often pursued in "traditional" science), as well as a balanced focus on both false and missed discoveries, the careful integration of multiple disciplines as needed and as a foil when not necessarily needed, and an exuberant exploration of intellectual space. In the 1990s, Holling and his collaborators created the space for the development of transcendent science with annual retreats, where the focus was on the unguided and exuberant exploration of wild ideas (personal communication by an attendee; also see Parker and Hackett, 2012; Holling and Sundstrom, 2015). It is critical to note that the development of resilience science occurred over more than 50 years. Holling's initial research on spruce budworm outbreaks took place in the 1960s and 1970s (Holling, 1966; Holling and Buckingham, 1976). His first "transcendent" article was published in 1973 (Holling, 1973) and was largely ignored by the scientific community for approximately 20 years. It is now recognized as the genesis of an entire field of science, but one that was initially developed in fits and spurts. In addition to initial skepticism toward and reticent acceptance of Holling's ideas, one reason for the time lags, however, was logistical; tests for the presence of alternative regimes as a result of the loss of ecological resilience required long-term research and monitoring programs. It took decades to observe and explain a pattern of regime shifts (see Hughes, 1994; Estes and Duggins, 1995 for early examples).

The development of resilience science was not planned and did not receive any federal or agency funding in its establishment, and thus provides many lessons for funding agencies trying to deliberately replicate this process for the advancement of science. As a successful example of an emergent convergence process, resilience science has helped address problems that were not, at the time, considered. This includes the identification of alternative dynamic states, development of early indicators of regime shifts, identification of spatial regimes, pathways for transformative change, management paradigms for social-ecological systems that are nonstationary and at non-equilibrium, and the development of related sciences such as disaster planning. As an emergent science, resilience has application to systems dynamics at multiple scales, and is applicable to a wide range of complex systems. We think it is possible to replicate this success in ways that will benefit humankind as it tackles increasingly complex wicked problems. However, the lessons learned from both the history of resilience science and our own NNA convergence workshops are that it will take an intentional commitment to a process that we are only beginning to understand.

Challenges to the development of convergence research

Timescales and discounting the future

Time is a relevant issue for convergence processes in 3 distinct ways: the timescales relevant to individual participants; the timescales of the key processes at play in a complex social-ecological problem; and the implications of these differing timescales on how key actors discount the future (how we value goods in the future compared to how we value them now). Exposure to multiple varied perspectives in a convergence process is critical, but it is important to recognize that spatiotemporal scales beyond the immediate interests of the stakeholders' present may be highly pertinent. There may be concrete and narrow management actions that can be rapidly taken in response to one cycle of transcendent and focused phases, but the very nature of wicked social-ecological problems that represent the intersection of multiple coupled complex adaptive systems and subsystems means the long-term view will be critically important. Generating meaningful solutions will take time, and driving societal change even longer. There is no avoiding the fact that many complex problems are inescapably connected to human values and societal norms. Institutional, social, technological, and economic change take time and a commitment to the future. Funding cycles tend not to reflect this reality, nor do they cope well with the reality that discovery and learning across multiple interacting systems takes time. Furthermore, in light of our ability to discount the future, it can be difficult to even generate a shared understanding of the future cost of current choices, regardless of the political will required to plan for the future. Climate change is a highly relevant example.

Climate science developed in a knowledge silo, which slowed the development of a shared understanding of climate change and, by extension, society's response to the climate crisis. Anthropogenic global warming and its main cause, reliance upon fossil fuels for energy, became established as scientific facts in climatology and atmospheric sciences in the 1960s and 1970s. As is often the case in science, the initial message of climatologists did not resonate in other academic communities. An early model integrating economic and climate data helped to confirm contemporary climate change and its relationship with human activity as a scientific fact, but concluded that the costs of the drastic mitigation actions recommended by climatologists were too high in relation to their benefits (Nordhaus, 1994). The economic component of Nordhaus's (1994) model used a discount rate that, when comparing the welfare of the present generation against the welfare of future generations, concluded it was too expensive to take mitigation actions (Weitzman, 2001; Hoel and Sterner, 2007). Economic analyses traditionally deal with the study of short- and medium-term phenomena where a decade is often considered the distant future, whereas some climate change impacts won't manifest for several decades, others for hundreds and others yet for possibly thousands of years. The timescale at which different disciplines and stakeholder groups operate and how they define and ultimately value the future may vary greatly and needs to be explicitly acknowledged. Indigenous peoples in the NNA convergence workshops stated a desire for *six generations* as the relevant timescale to understand change in the Arctic. Given the rapid rate of change in environmental variables in the Arctic, including unprecedented changes in permafrost temperature and land and sea ice extent, an accelerated convergence process may be crucial and will need to incorporate the immediacy of climate change impacts in the Arctic in addition to the multigenerational perspective offered by the Indigenous participants.

Metrics of success

The typical scientific metrics of success, such as grant money awarded, articles published and cited, and PhD students graduated, are not particularly relevant when measuring the success of a convergence research project in the short term (Nowotny et al., 2003). It is important to first define a successful outcome, and then devise metrics for quantifying the degree of success. Alternative metrics such as a potential creativity indicator (Soler, 2007) are needed, but they may be harder to quantify as they will necessarily be less tangible.

We propose that one key successful outcome from a convergence approach is that all or most participants leave the table transformed as a result of (inadvertently or by design) reflexive engagement. What this means will be different for each individual, but it applies to all participants whether they are a researcher, a stakeholder, a government representative, or other. For example, one early career researcher walked away from the NNA workshops with a profoundly different view of how her science fits into the larger picture. As a permafrost ecologist, she typically framed permafrost thaw as capable of changing the global climate through permafrost-carbon feedbacks but struggled to see how her work was relevant to the lives of Arctic residents. However, after hearing stories about the changing nutritional content of moose from Elders during the Tanana Chiefs conference, she realized that cascading interactions between permafrost thaw, hydrology, and vegetation are directly impacting food security and cultural values associated with food identity for Indigenous Arctic residents who are dependent on moose for nutrition. This broader view of her science was an important transformation, and one that will ultimately facilitate broader thinking. However, under typical metrics of success, this outcome is invisible because it won't lead to a publication during the time frame of the convergence workshop funding cycle. Similarly, a research economist participating in the workshop noted that personal transformation would be fundamental in any modeling approach that aspired to describe the local economy and its ability to withstand environmental shocks. The decision of some Alaskan tribes to live far away from roads and highways seems counterintuitive through an economic lens—economic theory, while useful to conceptualize the trade-offs individuals and societies face, cannot be used in isolation. On the other hand, the recognition that Indigenous communities have lived in a harsh and changing environment for millennium and also, as described by the Elders, recently endured deep socioeconomic (non-environmental) shocks, puts the issue of climate change in both a broader and more local perspective. Transformation for the economist meant a deeper understanding of individual and group agency and an ongoing reflection on its implications for climate adaptation in the Arctic.

A possible medium-term metric of a successful convergence process is how broadly the findings can be applied. Convergence should lead to core generalizable principles and a broad framework that can be applied across systems. Because this is the development of theory rather than the generation of case studies or completion of deliverables, this will necessarily be a slower process, as was described with regard to the development of resilience science. As such, tabulating the success of convergence research needs to focus on process, not merely the development of an endpoint, such as "solutions." The concept of *a* solution is antithetical to the complexity of the type of problems that require a convergence approach. Rather than searching for *the* solution—the archetype of a perfect outcome—participants in convergence research should be exploring multiple possible trajectories that emphasize how trade-offs associated with different choices and expressed values lead to a landscape of outcomes and different navigational paths to get to them. The desirability of one vision or end point should be understood in the context of for whom, and for what duration.

The metrics used to evaluate the success of convergence research share an important core with traditional science, in that they both need to contribute to the conversation. In traditional science, researchers engage in dialogue with other researchers via the publication of their work or at meetings and conferences. The building of new knowledge from previous work is the backbone of scientific strength. The conversation in convergence research needs to be broadened to include communication styles that are not solely the province of scientists, such as TED talks, white papers, nonprofits, and community projects and products should be expanded from peer-reviewed science to include other forms of communication, like blogs or books that effectively synthesize and communicate system understanding, such as Rachel Carson's *Silent Spring* (1962) or Marjorie Stoneman Douglas's *The Everglades: River of Grass* (1947). Employing a broader set of metrics to evaluate the success of convergence research will be needed in order to fully appreciate the power of the convergence approach. That said, given enough time a successful convergence process will generate products more traditionally recognized as markers of success; the explosion of resilience science in the literature is one example.

Allocation problem

Research is an unpredictable enterprise and discovery is often the result of chance. This understanding has led funding agencies like the NSF to move away from end-point research calls where explicit solutions are demanded, to funding schemes that create environments that increase the odds of novel discovery. Convergence is one such approach that builds on the idea that tackling wicked problems requires the creation of a multistakeholder interdisciplinary environment. Although they do not center convergence research, the considerable success achieved through the integrative approaches used by the John Wesley Center for Analysis and Synthesis (United States Geographical Survey, 2023), the National Socio-Environmental Synthesis Center (SESYNC; University of Maryland, 2023), and the National Center for Ecological Analysis and Synthesis (NCEAS; UC Santa Barbara, 2023) demonstrates the clear benefits of integrating thinking to generate solutions. SESYNC and the Powell Center have advanced computational capabilities that facilitate the use of existing but underused data by researchers from multiple disciplines. In the experience of some of the authors, they provide a conducive environment for undertaking transcendent research in that the organizations aim to bring together researchers from a variety of contexts and disciplinary backgrounds, but their focus on data is necessarily retrospective and their willingness to fund deeply novel work is limited. Thus, although new insights and perspectives are being developed from projects supported by these organizations, convergence research shifts its focus prospectively to new scholarly efforts.

The underlying challenge for funders, who must decide to whom, and for what problem research money should be allocated is ubiquitous. Priority in scientific research has an important moral dimension, as prioritization implies subordination (see Medawar, 1969). The high opportunity cost of public funding means agencies must justify their research priorities to oversight parties who may be ill-equipped to understand research processes. The reduction in dollars spent on basic research in the United States in recent decades reflects this struggle (Dijkgraaf, 2017). Not only that, to the extent that convergence research tackles multidimensional complex problems, the process of back-and-forth movement between transcendent and focused research may be expected to be particularly long, thus making it difficult for funding agencies to justify their allocation of resources given the time frames upon which success can be reasonably expected.

Furthermore, it is also reasonable to expect that most convergence processes will fail to achieve the goal of novel solutions, let alone of a new science, because the generation of novelty can be encouraged, coaxed, and facilitated, but not guaranteed. For example, research on scientific and intellectual social movements shows that emotions are central to their success; a case study on the Resilience Alliance, the network that incubated and developed resilience science, made clear that the high rates of trust and strong, positive emotional feelings between participants were central to their success and were cultivated over many years (Parker and Hackett, 2012). High rates of failure will put funding agencies in an even more difficult position, such that one logical solution will be to fund established scholars with a proven track record because they will ostensibly be more likely to succeed. There is also considerable risk for academics themselves, where career development in a highly competitive academic system lends itself to risk averseness. The review process may disadvantage junior scholars, even though it is arguably junior scholars who will be more likely to think outside the box and ask and test risky propositions. These paradoxes highlight some of the challenges funding agencies face when trying to expand funding opportunities beyond standard focused-science approaches.

Requirements from funding agencies

There are important opportunities for science-funding institutions to facilitate and, hopefully, accelerate a convergence process. The NSF already has funding in place for both a scoping phase akin to a transcendent phase and a focused phase via their Phase I and Phase II Convergence Accelerator program (NSF solicitation 20-565). There is the risk of a disconnect, however, between the language used to acquire funding and what actually emerges as the "problem" after a convergence process. Solicitations that require that the problem already be defined and that the "appropriate" mix of expertise be a priori delineated risk constraining the convergence effort so profoundly as to curtail the transcendent phases central to convergence research. Furthermore, fair and objective reviewing of convergence grant proposals is currently still challenging (Eyre et al., 2021). These issues point to a critical limitation with grant processes in general. They require that researchers have already identified the boundaries of the problem, which forces both the problem identification and the possible solutions into a small space, making innovation, contagion, and serendipity less likely to emerge. Szent-Györgyi (1972) confessed that writing his Dionysian (i.e., transcendent)-based grant applications was "agony," as "defining the unknown or writing down the subconscious is a contradiction in absurdum."

Ideally, a convergence process begins with a problem definition scoping-phase, rather than requiring such detail a priori, and also allows for the possibility that one transcendent scoping phase is unlikely to include all critical voices in the room on the first cycle. The problem definition is also unlikely to remain static or linear—a successful convergence process will almost certainly be nonlinear, as the process of discovery and novelty is often intuitive and serendipitous. In our experience in the NNA workshops, Indigenous people defined the problem in different and often unexpected ways than did disciplinary experts, to the point that climate change, despite the consequences it has had for Arctic Indigenous people in all areas of their lives, was a secondary consideration in light of many issues that more fundamentally challenge their ability to live as they desire. They defined the problem of the New Arctic in terms of the following nonexhaustive list of issues: health risks; access to jobs; transportation challenges; biological impacts on prey, including parasitism, shifting animal population ranges, and decreased fat stores; increasing severity of fires; collapsing riverbanks; reduced food security and food identity; increased access to undesired resource extraction; and land use and land cover change, among others. The stories they shared spoke of missing the climate conditions they used to experience (i.e., more ice, longer winters) but were more focused on techniques for adapting. There was an emphasis on maintaining identity by retaining beneficial pieces and adapting to change that was summed in the following phrase

Elders repeated: "When the times change, we change." The issues they emphasized were in stark contrast to other themes discussed at the workshops by non-Indigenous participants, such as permafrost contributions to climate change; the role of interactions between sea ice, albedo, and oceanic chemistry on climate change; methods to model resilience of multi-scaled spatial regimes; agricultural implications of Arctic–Mid-latitude connections; and the need for locally relevant actionable items.

It should be readily apparent from this example how different the problem identification would have been had only engineers, biogeochemists, and permafrost ecologists been present. To the greatest extent possible, funding for convergence processes should allow for problem identification to occur *after* funding has been granted, and for desired products and outcomes to be flexible and moving targets as a reflection of the learning and transformation that should occur in a convergence process. Taken together, this supports current views that traditional models based on governmental funding may be insufficient and that other, potentially unconventional approaches, including a mix of institutional seed funding, philanthropy, joint ventures, angel investment, and so on, may be necessary to bolster convergence research in the long term (Eyre et al., 2021).

Conclusion

The three transcendent-style workshops undertaken in the New Arctic convergence workshops each represented a broadening of the problem definition and the voices and disciplines represented in the room. The focus was on hearing from highly different perspectives and creating a shared understanding of the problem definition. We argue that convergence research will benefit from the purposeful movement between focused and transcendent science, and that these processes will take time with success measured by an expanded set of non-traditional and traditional metrics.

It is almost irrelevant whether or not convergence research processes actually succeed in meeting NSF's goal of "solving vexing research problems, in particular complex problems focusing on societal needs," because we are in an era of rapid and dynamic change, and the implication of the verb "to solve" is that a problem is fixed sufficiently in time and space as to permit a "solution" (Scown et al., 2023). What is not irrelevant is the importance of the philosophy and perspective that underpins a convergence approach. In particular, broadening perspectives is crucial and takes time, commitment, and willingness to allow organic learning, collaboration, and idea generation. A convergence approach is also invaluable for creating opportunities for diverse researchers, practitioners, and citizens to come together repeatedly to build trust, a shared language, and a broader perspective on both problems and solutions. Supporting and creating space for focused and transcendent approaches to science will yield unexpected insights and learning, and maybe, given time, these will coalesce into the emergence of new science. Many of the underlying problems humanity is now facing are like tangled balls of yarn—pull one end and countless other strings are moved. For these reasons and more, convergence approaches, which intertwine multiple disciplines and a plurality of perspectives, are critical, however difficult to implement they may be.

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The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this article.

Author contributions

All authors contributed to developing the concepts, writing the article, and designing the figures.

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