Commentary

OPEN ACCESS

THE EMBO JOURNAL

Creativity as an antidote to research becoming too predictable

Markus Baer[1], Anja Groth[2], Anders H Lund[3] & Katrine Sonne-Hansen[3,*]

Most living things do not like extreme heat. Case in point—in 2021, French winemakers recorded the smallest harvest since 1957 due to rising temperatures. Unlike the grapes that give birth to dry whites and luscious reds, some organisms flourish in extremely hot environments, however. In the late 1960s, Thomas Brock, a microbiologist from Cleveland, and his undergraduate student Hudson Freeze conducted research in Yellowstone National Park. What drew their interest was that some organisms seem to thrive in the hot springs sprinkled throughout the park. From a sample of pink bacteria collected from Mushroom Spring, Brock and his student isolated a prokaryotic organism thriving at 70°C, which they named *Thermus aquaticus*—after the Greek word for "hot" and the Latin for "water." The ability of an enzyme (DNA polymerase) from *Thermus aquaticus* to tolerate high temperatures would later spur the invention of the *polymerase chain reaction* or PCR, which won biochemist Kary Mullis a share of the 1993 Nobel Prize in Chemistry and revolutionized biomedicine.

When it was published in the *Journal of Bacteriology*, the work by Brock and Freeze went largely undetected. It generated a few citations but did not manage to attract the attention of the wider community of biologists (Bhattacharya & Packalen, 2020). Of course, this is not uncommon for novel findings—their true value may remain unknown for a while, even if the work later spurs new ideas and scientific breakthroughs. Precisely, because it constitutes a venture into the unknown, pursuing novel ideas requires a special set of circumstances. Without the National Science Foundation's financial support and without Brock being able to spend a decade exploring the hot springs of Yellowstone National Park, satisfying his curiosity about things that thrive in extreme heat (but undoubtedly offending his nose in the process—those thermal pools can be quite pungent), the world likely would have had to wait longer for the advent of PCR.

Our core argument is that the conditions that allow and encourage scientists to engage in the relentless, creative exploration of the unknown are becoming harder and harder to find. There are several reasons for this. For one, finding new ideas appears increasingly difficult. Data from the United States, for instance, suggest that research productivity (defined as ratio of the output of ideas to the inputs used to make them) in a number of fields, including medical research, is declining over time. To offset the difficulty in finding new ideas, the United States would have to double its research effort every 13 years (Bloom *et al*, 2020).

One of the consequences of this increase in research activity is that the number of papers published each year has increased over time (Chu & Evans, 2021). This growth has some undesirable side effects. Scientists focus their attention on work that is already well-cited rather than on new ideas or on ideas on the fringes of the scientific mainstream (Chu & Evans, 2021). Sifting through a deluge of ideas—published in an actual Mount Kilimanjaro of papers (Van Noorden *et al*, 2014)—to find a nugget of wisdom is hard. This leads to a calcification of the intellectual structure of a field, slowing down progress over time.

Funding agencies further exacerbate this trend. There is a tendency to minimize risk—it has become the norm that grant proposals have to already provide substantial amounts of data supporting the proposed theories/hypotheses (incidentally, something that Thomas Brock would not have been able to do)—and to reward work on topics that are more established. As recently as the 1990s, however, research that explored more current ideas was not at a disadvantage when it came to funding (Packalen & Bhattacharya, 2020). Going back to these "old ways" of maintaining a balance between funding work that builds on more established ideas and work that builds on more recent advances may be something that the biomedical sciences could aspire to. Small steps are being undertaken. For instance, some foundations in Denmark are now providing opportunities for (modest) funding of applicants whose ideas would likely get shunned by the traditional funding schemes.

As obtaining external funding is the lifeblood for many research programs, investigators are responding to these pressures by "playing it safe," pursuing ideas that, from the outset, are likely to be publishable to ensure a constant stream of papers. Long gone are the days that biologists could explore the hot springs of Yellowstone National Park without knowing what all that exploring would amount to (other than a nice tan). A journey into the exploration of the unknown has been replaced with a ticket on the Shinkansen "bullet train"—"destination: known" and always on time.

Contemporary academic training practices have not been able to fight back these

1 Washington University, St. Louis, MO, USA
2 NNF Center for Protein Research (CPR), University of Copenhagen, Copenhagen, Denmark
3 Biotech Research and Innovation Centre, University of Copenhagen, Copenhagen, Denmark
*Corresponding author. E-mail: katrine.sonne@bric.ku.dk
DOI 10.15252/embj.2022112835 | The EMBO Journal (2023) 42: e112835 | Published online 25 January 2023

developments. Quite the contrary—one of the consequences of the "pressure to produce" is that budding researchers are often recruited onto preexisting projects with already defined milestones and deliverables, all the while having to develop a range of other skills. Naturally, this leaves little room and time for engaging in more exploratory aspects of the scientific process. The result is that we are turning the next generation of scientists into excellent experimentalists and "research managers," rather than into bold scientific thinkers.

We are at a point at which a systematic focus on training and injecting *creativity* into the research process in the life sciences is imperative. When hearing the word "creativity," many people think of the tortured artist, toiling away in isolation in a village in the south of France (but who would not want a sip of a French Cabernet Sauvignon at the end of a hard day's work—before it runs out). As enticing as this image of radiant colors and crystalline light might be, it is by no means the sole context in which creativity can flourish. *Creativity* is defined as the generation of ideas that are new and have potential value by addressing a problem or capitalizing on an opportunity. There is no mention of *artistic* endeavors in this definition! In fact, creativity is fundamental to the human condition and, as such, can be found anywhere, anytime, given the right circumstances.

Creativity may be most pressingly needed during the early stages of the knowledge production process—when we have to make what physicist Richard Feynman has called "educated guesses" as to how the world may work. This is the opaquer part of the scientific process; the part that benefits from the use of intuition and of a language that is permissible of it—what Itai Yanai and Martin Lercher refer to as *night science language* (Yanai & Lercher, 2020; check out also their podcast series entitled, "Night Science"). While the part of the process that deals with testing existing ideas is highly visible and more easily describable, the guessing, theory-generating part often gets far less systematic attention. Yet, it is the part of the scientific process that is becoming ever more important. We are not so much in need of more data, but of educated guesses (i.e., a theory) about what to look for in the first place. This call for ideas is echoed by Paul Nurse, quoting the famous

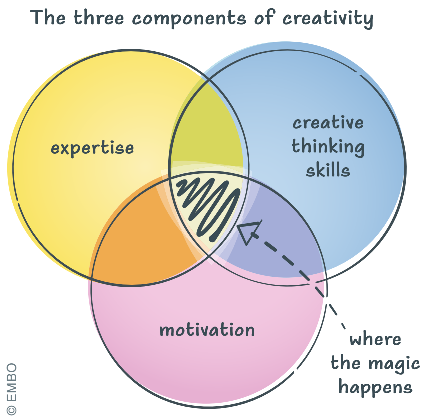

Figure 1. Three components of creativity.

words of the late biologist Sydney Brenner, "we are drowning in a sea of data and starving for knowledge" (Nurse, 2021).

To understand the value of creativity for making educated guesses, it is helpful to dissect it into its components. According to Teresa Amabile, one of the pioneers of the study of creativity, there are three components to creativity—domain expertise, intrinsic motivation, and creativity-relevant skills (Amabile, 1996). To put it simply: creativity flourishes when people have the *wit* (knowledge of the domain), the *will* (intrinsic motivation), and the necessary *creative tools* to tackle interesting and challenging problems (Fig 1).

Domain expertise refers to a high level of domain-specific knowledge acquired though experience. Without expertise, it is impossible to know where on the scientific frontier to look for new and interesting problems. However, there is a downside to becoming an expert. The more we know about a domain and the longer we have studied it, the more we lose flexibility in seeing new problems and devising novel solutions to them (Dane, 2010). Edward Tufte, for instance, describes how experts are likely to glance past unexpected findings in their datasets, whereas outsiders are more likely to pay attention to these surprises, as they see the world through what he calls "vacation eyes" (Tufte, 2020). While the loss of flexibility may not be of immediate concern to budding scientists, the benefits of learning ever more about the very same domain start to evaporate rather quickly over one's career. Luckily, there is an antidote—investing in becoming well-versed in new and different domains, that is, developing knowledge *breadth* rather than (further) *depth*. Research suggests that there are immediate benefits from knowledge breadth for creativity—even scientists just at the beginning of their academic journey should benefit from developing expertise in additional domains (Mannucci & Yong, 2018).

How can we accomplish this? One strategy is to allow and encourage early-stage scientists to immerse themselves in *analogous problems domains* and the solutions they may inspire. An example might serve to illustrate this principle: Some years ago, the already mentioned Shinkansen "bullet train" needed redesign. The train's speed created a sonic boom that was heard for hundreds of meters when exiting tunnels. So, a group of engineers was charged with making the train quieter. One of the lead engineers, Eiji Nakatsu, was a bird watcher. He realized that birds diving into water to catch pray face the same challenge as the train trying to cut through air while going through a tunnel. The new design of the train's front was based on the shape of the Kingfisher's beak—a bird diving at high speed from one medium (air) into another (water) with barely a splash. To emulate Eiji Nakatsu, it will be necessary to allow scientists to not only spend time studying topics other than the ones they are actively investigating, but also to allow them to join research collaborations with scientists from other domains and even disciplines investigating analogous problems.

Creativity requires a certain type of motivation—*intrinsic* motivation. People are intrinsically motivated to the extent that they derive pleasure from the work itself and from the opportunity to acquire new skills. *Extrinsic* motivation is just the opposite—it is the drive that comes from incentives, such as financial compensation and recognition. The reason why intrinsic motivation is so important to the creative process is that it provides perseverance—in the face of setbacks, obstacles, and naysayers. Intrinsic motivation is largely a function of the nature of the work—how challenging it is and how much autonomy it affords. Whenever we have the freedom to explore new lines of inquiry, to satisfy our curiosity, and, perhaps most importantly, to make mistakes, intrinsic motivation ensues. However, the knowledge production process that has become dominant in the life sciences is antithetical to budding scientists experiencing autonomy. Predefined (externally funded) research projects that are too rigidly managed (be it by funders or by principal investigators) with their milestones and deliverables offer little room to exercise autonomy.

If we want research to flourish, it will be imperative for us to take responsibility and rethink the knowledge production process in our laboratories to allow for the occasional detours, setbacks, and dead ends. Case in point—Richard Feynman, who won the Nobel Prize in Physics for his contributions to quantum electrodynamics, developed his ideas based on an observation that many would consider a major intellectual detour—a cafeteria worker throwing a plate into the air. Feynman observed that the "Cornell" logo on the plate was going around much faster than the plate's wobble. Armed with this observation and allowed the freedom to explore the dynamics of the motion of the plate, he developed the basis for the Feynman diagrams (Feynman *et al*, 1985). Naturally, this more autonomous and playful approach may decrease the efficiency of the knowledge production process. However, efficiency is not the primary criterion by which to evaluate research quality. The novelty and utility of our ideas should be the primary criteria. Research leaders may thus want to embrace the values of autonomy and novelty more courageously and embolden early-stage researchers to do just the same. Similarly, academic institutions need to take a good, hard look at themselves, increasing the "breathing space and time" for scientist to engage in exploration of new ideas and research avenues.

The final component contributing to our creativity is a set of creative skills that allow people to take greater advantage of their drive and of what they know. One skill that is imperative here is the ability to tolerate uncertainty. The uncertainty of not knowing, of taking guesses in the absence of a firmly established knowledge base, and of trying out things without knowing exactly what the outcome will be. The systems biologist Uri Alon refers to this as staying in the "research cloud," highlighting the value of transitioning from the "known" to the "unknown" (i.e., the research cloud) and temporarily residing in this state of uncertainty despite the discomfort and frustration that are bound to arise (Alon, 2013). The notion of the "research cloud" may seem to conflict with the prevailing scientific culture, in which there is little room for speculations or intuitions. To combat this, we need to reimagine the ways in which research leaders interact with their teams, so as to encourage budding scientists to become more comfortable stepping outside the scientific path they learned as students. Case in point—critical thinking is highly priced in the training of university students. However, the inquisitive and evaluative processes that critical analysis relies upon can be antithetical to the generative processes required for creativity—it is difficult to develop new insights while at the same time having to defend them from others' critical examination. This calls for supervisors and mentors to create what we call "creative oases"—spaces in which critical analysis is dispensed with and risk-taking and speculation are encouraged.

Another lesson from creativity research is that it is impacted heavily by the work environment in which people operate. Creative teams thrive in high-trust environments, and whenever their members practice a "*yes and*" rather than a "*no because*" approach that encourages young researchers to engage and contribute toward new solutions to long-standing scientific questions. Thus, it is crucial that administrative and research leaders are engaged in building a supportive and inclusive culture. To illustrate—at four biomedical research centers at the University of Copenhagen, we held in autumn 2021 a four-session workshop for research group leaders on how to nurture a culture that fosters creativity. The sessions focused on how to guide teams through the different phases of the creative process, and introduced tools for divergent and convergent thinking. The underlying principle was that creativity thrives when leaders build an environment that allows the team to capitalize on the collective knowledge of individual members—an environment built on the principles of diversity of thought, autonomy, and a high degree of psychological safety (e.g., deferring judgment in order to promote idea sharing and interpersonal risk-taking).

In conclusion, we believe that research organizations should not dwell too much on the structural barriers to creativity (funding agencies and politicians need to dismantle these barriers) but rather take action to encourage more "theory-guessing" and nurture the ability for budding scientist to find delight in staying in the "research cloud"—at least for some time. Also, research communities and academic institutions should take greater responsibility for embracing a truly team-based approach to creativity (rather than the "lone genius" model), in which scientists are granted the freedom to take the occasional intellectual detour or flight of fancy—without repercussions or fear of failure. These efforts will be needed if we are to make lasting changes to the way in which we engage the scientific process and venture into the unknown in the pursuit of transformational research.

References

Alon U (2013) Why science demands a leap into the unknown. *TED* (June 12th, 2013). https://www.ted.com/talks/uri_alon_why_science_demands_a_leap_into_the_unknown

Amabile TM (1996) *Creativity in context: update to the "social psychology of creativity"*. Boulder, CO: Westview Press

Bhattacharya J, Packalen M (2020) Stagnation and scientific incentives. National Bureau of Economic Research (working paper 26752). https://doi.org/10.3386/w26752

Bloom N, Jones CI, Van Reenen J, Webb M (2020) Are ideas getting harder to find? *Am Econ Rev* 110: 1104–1144

Chu JSG, Evans JA (2021) Slowed canonical progress in large fields of science. *Proc Natl Acad Sci U S A* 118: e2021636118

Dane E (2010) Reconsidering the trade-off between expertise and flexibility: a cognitive entrenchment perspective. *Acad Manage Rev* 35: 579–603

Feynman RP, Leighton R, Hutchings E (1985) *Surely you're joking, Mr. Feynman!: adventures of a curious character*. New York, NY: W. W. Norton & Company, Inc.

Mannucci PV, Yong K (2018) The differential impact of knowledge depth and knowledge breadth on creativity over individual careers. *Acad Manage J* 61: 1741–1763

Nurse P (2021) Biology must generate ideas as well as data. *Nature* 597: 305

Packalen M, Bhattacharya J (2020) NIH funding and the pursuit of edge science. *Proc Natl Acad Sci U S A* 117: 12011–12016

Tufte ER (2020) *Seeing with fresh eyes: meaning, space, data, truth*. Cheshire, CT: Graphic Press

Van Noorden R, Maher B, Nuzzo R (2014) The top 100 papers. *Nature* 514: 550–553

Yanai I, Lercher M (2020) The two languages of science. *Genome Biol* 21: 147

