‘First you see, then you know’: Becoming more creative in academic work

Across disciplines and projects, there can be pressure for researchers to provide novel insights. But this can be easier said than done. **Patrick Dunleavy** *offers some helpful strategies for innovative and creative thinking. Look beyond your discipline and through forms of science and scholarly communication that are more accessible. And make sure to keep a record (perhaps as a blog?) so you don’t lose these ideas.*

Karl Popper memorably said: ‘There is no such thing as a logical method of having new ideas or a logical reconstruction of this process’. That remains true, and so any advice here can only hope to prompt or stimulate your own thinking, in a somewhat tangential way. And of course, I have no special access to a hoard of secrets about being more creative or innovative in research— still less of a generic kind that always work. My musings and advice below are phrased in a definite-sounding way. But they stand a long way away from, say, the claims to offer advice validated by psychological research in Daniel Levitin’s impressive and useful, *The Organized Mind*.

Instead my hope here is that by collating together some thoughts below in a systematic way, by providing an organized menu of ideas, perhaps readers may be triggered to pick out and try what might work for them. As in a restaurant, don’t eat the whole menu. Instead, treat everything here as a set of provocations, or suggestions. Some of these ideas may (just) help (some) people to formulate their own ideas of how to go forward in their discipline area, and in a way relevant for their particular research.

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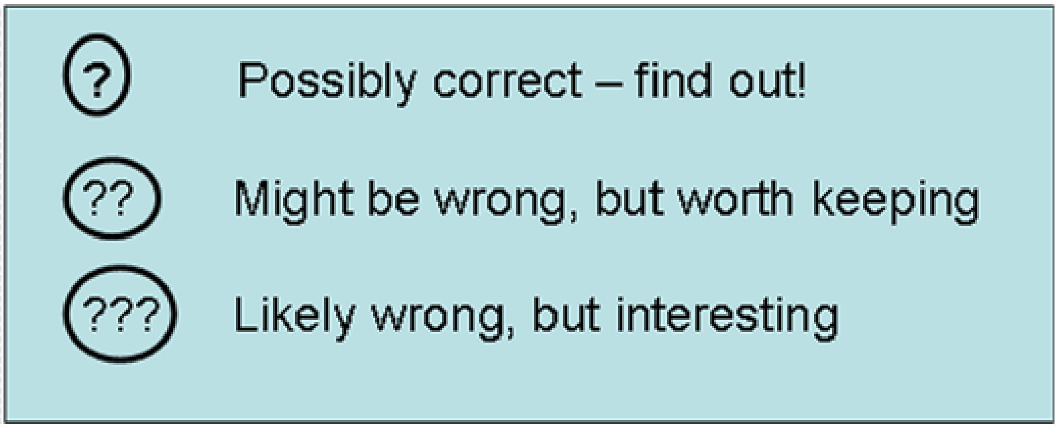
1. Take the risk of trying to think innovatively

'You cannot jump the gorge in two leaps', Chinese farmers reputedly say. So when any venture sounds too risky, our incentive is to avoid the effort and just make the long trek around via a distant bridge. In academia one of the ways of doing this is by over-extending literature searches, and taking at face value the 'narcissism of small differences' by which multiple authors seek to differentiate their work. In fact new connections aren't lying out there plentifully in the literature. They can't often be surfaced by endless searches or massive literature reviews (although a 'systematic review' can sometimes generate unexpected insights). The implication for researchers is, as Arthur Schopenhauer urged: 'Don't read, think!'

Of course, your thinking also needs to be well-informed, matured and perhaps even saturated within your professiona context. But on its own, knowledge of a field without the stimulus of an effort to reconstruct or redirect attention, to vary an assumption, to 're-see' a line of analysis, will always just produce more of the same. So it may be useful to deliberately think in 'blue skies' (even 'ignorant') mode, some of the time. Some of what you get will not work. But, as Linus Pauling said: 'The best way to get a good idea is to get a lot of ideas'.

It's also far worse to miss one good idea for fear of writing down ten poor or half-baked ones, a diffidence problem that seems to acutely afflict many intellectuals. As William Emmerson remarked: 'In every work of genius we recognize our own rejected thoughts; they come back to us with a certain alienated majesty'.

Edward de Bono wrote a whole (rather dreadful) book about the difficulties people have in thinking about what is only 'possibly correct, possibly wrong'. One device that can help here is a way of immediately recording the status of your ideas, so that they automatically go into your records with a kind of health warning attached. Here's a set of 'reinforced' question marks that I use to indicate the conditionality of an idea.



Bear in mind too that nothing is so evanescent as your own good ideas, so quickly past and lost. ‘Chance gives rise to thoughts and chance removes them: no art can keep or acquire them’, wrote Blaise Pascal. ‘A thought has escaped me. I wanted to write it down. I write instead that it has escaped me’.

2. Look harder

Be alert for anomalies and paradoxes, things that are accepted but perhaps should not be. Scientific progress often involves an ability to notice small (often minutely) anomalous events or outcomes. A first stage in seeing a new idea is often to problematize things that we prosaically explain in unreflective ways, seeing past ‘common sense’ accounts of how things work. ‘Common sense is not concerned with the relations of things to one another..’said the theological Bernard Lonergan. ‘[It] has no theoretical inclinations. It remains completely in the familiar world of things for us’. Many scientific developments rest on not accepting tiny mismatches or inconsistencies, but recognizing what makes them interesting. ‘Familiar things happen, and mankind does not bother about them’, said A. N. Whitehead.’ It requires a very unusual mind to undertake the analysis of the obvious’.

A willingness to realistically but critically probe or challenge orthodoxies may also be important, given that even science has strong tendencies to suppressing or diminishing ‘puzzles’ that do not fit within a prevailing paradigm of explanation. Again being critical can be an uncomfortable stance to adopt, especially in STEM science fields where most critiques are ill founded and many crankish, and where journal reviewers are exceptionally averse to critiques of established work. Yet ‘a dead thing can go with the stream’, said G. K. Chesterton,’ but only a living thing can go against it’.

Look for unstated premisses, contestable ‘primitives’, or assumptions where the consequences of varying them has not been explicitly considered. ‘The difficulty lies, not in the new ideas, but in escaping from the old ones, which ramify, for those brought up as most of us have been, into every corner of our minds’, said John Maynard Keynes. Trace out the consequences of varying assumptions. In many cases, the implications of tendentious or ‘unrealistic’ assumptions or limitations of methods may not actually be that great. Initial effects that look large or consequential may actually damp down, or make little difference to model outcomes over time. But in other cases the consequences of changing assumptions or mis-measuring may spiral out and grow over many iterations.

‘Keep on the lookout for novel ideas that others have used successfully’, said Thomas Edison. ‘Your idea has to be original only in its adaptation to the problem you’re working on’. As I’ve argued in another blog, finding a good idea in another field and then working out how to transpose it to your own and make it do useful work can often be a promising strategy.

3. Look widely

Of course, academics and scientists must look a lot at journals. But remember also that anything you read in journals is 'light from a distant star'—it tells you only where the field was three years ago in STEM disciplines, perhaps four+ years in the social sciences. In addition, a far wider set of journals are relevant now than in the past, as digital accessing spreads the distribution of citable and original work. Record how you search, so that you can replicate complex Boolean search queries later, or adapt a successful search process for new applications.

So now you also need to scan academic blogs (especially multi-author blogs), use academic Twitter, xArchiv, and other modern forms of digital scholarship. Look also at conference papers, Google Scholar, Scopus and search options regularly. Go to conferences, especially at PhD and ECR stages. You're investing 3 or 4 years in a PhD project, or 2–3 years post-doc, so you'd better know where your field is going.

Don't be a social media hermit. At a minimum join Google Scholar Citations(GSC), Research Gate and Academia.edu. Once there, follow all the people close to your field and those you admire, so as to get instant alerts of their new stuff. GSC's 'My Updates' feature is also time-saving in 'pushing' potentially relevant work to you, based on your publications and citations. Once you have some publications, it's a good idea to also follow people who cite your work—they are most likely to have good things you can copy, re-use or re-purpose.

Look beyond your discipline at other STEM or social sciences or humanities subjects, especially in blogs and forms of science and scholarly communication that are more accessible. See if you can cross-pollinate, and take their good ideas for a walk in your garden. Look outside academia too—at multi-author blogs, newspapers, magazines, cultural trends. Contacts with clients, practitioners and consultants can all generate innovative perspectives. In fact such 'naive users' of academic and scientific work often generate highly original questions, because they are less inhibited by professionally learned ('can't be done') barriers.

4. Keep your practice under review

Watch for self-imposed mental limits – the little, extra constraints we often impose without realizing it. Grow ideas more. Jot everything down (to cope with the Pascal problem). Record your ideas and first impressions immediately, and in a form that you can definitely find again—maybe a full-text searchable notes archive on PC, or perhaps specialized software, rather than easily lost sheets of paper or hard to search notebooks. But there is also some evidence that writing, sketching and otherwise doodling notes of ideas may work better than computer-based systems.

However you do it, don't keep ideas circling at the back of your mind, anxious all the while that they will elude you—that way you'll keep re-reviewing the same six or seven ideas, but get no more. Instead write them down, try and expound them coherently. See what you think. Get your ideas down, however roughly expressed they may be, and only try to get them organized later on. Protect new ideas from criticism for a time. Never review their viability one by one—that way you may discard too much and miss connections. Instead try to surface a whole field of ideas, and then critique them only at the same time as you look for connections or for some underlying 'gold' content.

Develop your hunches, and constructively use your non-academic emotional or professional commitments. Developed a bit they can be creative ('push a bit further') drivers, so long as your powers of realistic critique are not too dulled. Use analogies, metaphors, images, little prototypes to drive intuitive explanation—even in very technical areas. Persistence is also important, because insights take time to develop. Hannah Arendt put it well: 'Every thought is an after thought. By repeating in imagination, we de-sense what has been given to the senses. And only in this immaterial form can our thinking faculty now begin to concern itself with these data… First you see, then you know'.

5. Be a constructive self-critic

Always sift and critique your ideas as a separate stage of your thinking, after you've let them grow a bit, and created and recorded a whole field of potentially relevant ideas. Never 'brainstorm' or generate ideas and then immediately critique them. Tackle sorting bad ideas from good with a whole field in view. Immerse in a topic, get a lot of ideas and don’t dismiss any until you have a full context.

But then target inconsistencies rigorously. Tighten up or systematize your ideas, make them fit together—or see if you can ‘pivot’ them in a new direction. ‘To create consists precisely in not making useless combinations. Invention is discernment, choice…’, said Henri Poincaré. (He went on to claim, quite unrealistically for most of us: ‘The sterile combinations do not even present themselves to the mind of the inventor’). Focus on a problem, never on a ‘gap’ in knowledge. Puzzles are opportunities, gaps are just a void, that may exist for good reasons. Don’t ask questions so much as evaluate and critique substantive propositions.

Run several ideas in competition. Start a file for each and just add stuff when it occurs to you for a month, say. After that time, look in each file and see which looks fuller or more promising. If you have idea A and your available alternative is nothing, you will always tend to over-identify with A, sticking with it when you maybe should move on. If you run idea A in competition with idea B, you’ll normally make better choices, because you can pick among alternatives. In a separate post I cover a systematic approach to evaluation derived from start-ups, that might prove useful.

Then, when you get to the point of explaining new ideas to other people, work hard on the presentation. Don’t let smal glitches mask genuine insights. Try to make the details of your account reflect and implement the bigger, driving ideas at a small scale.

6. Expect innovation to be an ‘up and down’ process

There is a dialectic inherent in all learning—to see things differently you need to go through a phase of unlearning what you thought that you already knew, and embrace the uncomfortable fact that you don’t know what is going on. As André Gide observed: ‘One does not set out in search of new lands without being willing to be alone on an empty sea’ A period of what Eddy Izzard called ‘humiliation’ is an essential transition stage in re-understanding. You have to tear the ‘creativity muscle’ a little to make it stronger. This can be psychologically (and physically) costly, and demoralizing at times. As Alan Alda memorably put it: ‘Originality is unexplored territory. You get there by carrying a canoe: you can’t take a taxi’.

But people are also most creative when they are not too worried. ‘Conditions for creativity are to be puzzled; to concentrate; to accept conflict and tension; to be born everyday; to feel a sense of self’, said Erich Fromm. And Carl Jung, went further, arguing: “The creation of something new is not accomplished by the intellect but by the play instinct acting from inner necessity. The creative mind plays with the objects it loves.”

You have to be psychologically secure to innovate, so it is important to keep your ‘risk’ exposure comfortable for you. Be somewhat over-optimistic and over-ambitious, but also get some good insurance in case ‘breakthroughs’ or promising new insights don’t happen. Try to identify and build in ‘solid work’ fallback options, ‘exit ramps’ and ‘second best’ outcomes—don’t let your whole PhD or postdoc project depend on innovation being achieved.

Over time, the effort to think more creatively will hopefully pay dividends. As Martin Heidigger argued: ‘The anxiety of those who are [intellectually] daring cannot be opposed to joy or even to the comfortable enjoyment of tranquilized bustle. It stands—outside all such opposition—in secret alliance with the cheerfulness and gentleness of creative longing’.

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If you’d like to think some more about these ideas, my book: Patrick Dunleavy, ‘Authoring a PhD’ (Palgrave Macmillan, 2003) has a relevant chapter on creatively ‘envisioning’ projects, like a PhD or a post-doc research bid.

Note: This article gives the views of the authors, and not the position of the Impact of Social Science blog, nor of the

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About the Author

Patrick Dunleavy *is Professor of Political Science at the LSE and is Chair of the LSE Public Policy Group. He is well known for his book* Authoring a PhD: How to plan, draft, write and finish a doctoral dissertation or thesis *(Palgrave Macmillan, 2003).*

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