Negotiating the essential tension: working within, across, and outside research traditions

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Script for

Taber, K. S. (2020). Negotiating the essential tension: working within, across, and outside research traditions (Keynote talk). Paper presented at the *Inclusive Ways of Knowing: Diverging from Tradition: Kaleidoscope Conference 2020*, On-line.

The essential tension?

The essential tension that is referred to in the title of this talk is that tension between the imperative to work within an established and recognised tradition, and the imperative to innovate. It is suggested that finding a comfort point in relation to that tension is a key feature of being a research student, or of being an academic. Tradition may instinctively be associated with safety, and innovation with risk, and there is some truth to that. Indeed there have been times and places in human history when (and where) to eschew tradition was to at least invite ridicule, probably to invite exclusion, and perhaps to invite death - maybe even grisly death.

One example would be Giordano Bruno who held such ridiculous ideas, considered so beyond the pale - that is outside the boundaries of conventional thinking - that he was burnt at the stake for them. To be fair to the authorities, his 'thinking outside the box' was pretty extreme. One of his suggestions was that the stars could actually be other suns that may have their own planets, and possibly even support life.

Galileo is often seen as the poster-boy of scientific martyrs, although arguably he could have avoided trouble had he been more politically astute. That is, it was not so much his ideas that got him into trouble as inability to take sensible advice about how to frame the presentation of ideas in ways that were not a direct provocation to authority. That is worth baring in mind if your supervisor has reservations about how your examiners may respond to your conclusions.

In any case, Galileo was only *shown* the instruments of torture, rather than having them used on him, something akin to a modern-day miscreant accepting a police caution, and he ended up in house arrest - a bit like working from home during a pandemic.

Hypatia of Alexandria, however, was killed, and is sometimes known as a martyr for philosophy - although in modern terms she could also be considered an astronomer and a mathematician. She was killed because people did not like what she is supposed to have said. It seems a moot point whether the 'Christian' mob who murdered her did so more from political rather than religious motives. This is a well known image of her although we might wonder if this the appearance of a typical Egyptian of the fourth Century? Or is this just how a European artist imagines how a great thinker must have appeared. We readily impose our expectations, based on what is familiar, on the world. Our implicit biases tend to lead to use identifying with our heroes and heroines - and assuming they were just like us. So members of the Pre-Raphaelite Brotherhood brought a particular aesthetic to their representation of mythical and historical women - so, for them, Hypatia obviously looks like this.

But failure to innovate may also bring risks, indeed existential risks. We might, a little tongue in cheek, think of the dinosaurs, or at least the ones that did not innovate by becoming birds. Or we might think of highly successful companies such as Kodak - a world leader in producing photographic film, that decided not to shift its base into digital photography, even though its research department was developing the technology. One of my part-time jobs when young was working in Boots the Chemists on the department that sold vinyl records and celluloid photographic film. No one thought to ask where all the plastic ended up in those days.

We cannot assume that those things we grow up accustomed to will last. I remember going to work with my father during school holidays when I was at primary school. I would travel into London with him in his luxury company vehicle. He worked as a kind of mechanic, but in what now seems a rather specialised niche. He worked for a company that supplied adding machines to businesses in London. His job was to visit offices and factories to service and mend the machines. Any business in those days needed both a pool of typists to produce letters, and adding machines to keep track of accounts. 'Those days' being the so-called 'swinging sixties' - although I was much too young to be able to confirm if they were indeed swinging.

Later, as a teenager in the 1970s, I even had a part-time job which required using such machines to add up long lists of numbers - although at least by the 1970s the machines were powered by mains electricity and no longer needed to be operated by pulling a handle for each entry. Looking back now, such technology seems alien to the modern world - indeed the adding machine had much more in common with its earlier ancestors, Babbage's calculating engines, than the electronic computer that is now so ubiquitous. As Ada Lovelace showed, Babbages' machines could in principle be programmed to follow complex algorithms, but it was never going to be possible to make mobile phones with this kind of technology. I doubt there is anyone employed anywhere in the world today to mend mechanical 'adding machines'.

Does any of this apply to the academy?

Perhaps not quite in the same way, but then we might ask if there are any professional philosophers today who claim to be logical positivists? Despite being perhaps the most influential school of thought in the first half of the twentieth century, later philosophers have moved beyond them - something that Wittgenstein, who was so influential to the logical positivists, would surely have approved of - perhaps seeing that school of philosophy as another metaphorical ladder that once climbed should be kicked away to continue with one's ascent towards knowledge.

We might ask how many current research psychologists consider themselves unreformed Freudians? Or Gestalt psychologists for that matter? Or even pure behaviourists? So, I wonder if in 30 years time anyone will claim to be adopting the ideas of Foucault or Bourdieu as the basis for their theoretical framework?

Kuhn and the essential tension

I have taken the expression 'the essential tension' from Thomas Kuhn, which seems pertinent to my theme given that Kuhn was a physicist cum historian cum philosopher who was considered to have helped initiate a programme of research in sociology. Kuhn is best known for changing the word *paradigm* from a relatively obscure technical term into a ubiquitous phrase in academia - and so he might seem to be largely responsible for torturing generations of graduate students who are now expected to set out their paradigmatic position for the benefit of supervisors and examiners.

Kuhn certainly did not think he was creating a new term, just employing one that was already in use. Ironically, Kuhn's ideas developed from his work on the history of science, and especially the history of physics - where research students seldom have to engage with issues of the philosophical aspects of research paradigms, as they are inducted into a well-established research tradition where such questions have already been settled.

Kuhn's notion of an essential tension concerned the relative imperatives of convergent and divergent thinking, of tradition and innovation. This is surely a matter for us all - whether research students, academics or simply humans working towards the good life!

Why might it be important to work in a tradition?

There certainly are good reasons for a scholar to work in a tradition.

When Kuhn heavily used the then relatively obscure word 'paradigm' in his *Structure of Scientific Revolutions* he did not mean the kind of broad approach to research that is often discussed in methods textbooks, and indeed his meaning is better reflected in the way the term is sometimes used by psychologists to refer to a model of a particular type of experimental research design.

Kuhn was actually heavily criticised for not being precise about what he meant by the term. Indeed one commentator suggested the term was used in over 20 different ways in his seminal book, although some of these distinctions were subtle. Kuhn himself accepted that he was using the term to mean two related, but quite different types of thing.

One of these uses was paradigm as exemplar. Kuhn thought that most work in any mature discipline was undertaken within a particular tradition, and was largely a kind of puzzle-solving. (I will come back to that point later.) Kuhn thought that induction into scientific research, relied on the novice working through standard sets of examples of how previous workers in the tradition had solved particular puzzles, so that these exemplars (or paradigms) could act as bases from which to allow those working within the tradition to tackle new puzzles.

I think it is Kuhn's other use (or cluster of uses) of paradigm that is more interesting for scholars working across a wide range of academic areas. He later suggested other terms for this second meaning, including the disciplinary matrix. A disciplinary matrix is shared by a community of specialist researchers from *within* a discipline. The communities that Kuhn referred to were much smaller than disciplinary communities such as those of chemists, sociologists, or historians. The critical level of academic community was even smaller than such sub-disciplines as might be reflected in labels such as economic history, physical chemistry, analytic philosophy, or social psychology. Rather, Kuhn thought that the communities of interest comprised those people who were arguably following the same research programmes, and addressing the same fundamental cluster of research questions.

Now the term research programme has been especially explored by a different scholar, the philosopher Imre Lakatos. So I could be accused here of sloppy scholarship because I am conflating Kuhn's ideas with those of Lakatos. However, one scholar's careless conflation is another's creative synthesis, so I will carry on regardless. Although these are quite different models, I think, at least as a first approximation, they can work together well.

So what is, in Lakatosian terms, a research programme? Such a programme has components labelled, a little unfortunately perhaps, a hard core, a protective belt, and positive and negative heuristics. If this seems a little abstract, it may help to think about your own research, and ask how it might be understood in terms of such a model as I examine these components.

A research programme can be considered to have 'content'. There will be certain things that are known, believed, posited, conjectured, assumed, conceived of, imagined, supposed or whatever. Some of this content will be theoretical - it will comprise of conceptual content in the form of ontology and associated principles, laws, conjectures, theories and so forth. By ontology I simply mean the kinds of entities that are considered (or conjectured, suspected) to exist within this programme. So, in one research programme the ontology might include the concept *working memory*, in another it may be *social class*, or *self-efficacy*, or *gifted learners*, or *habitus*, or *race*, or *the failing school*. Other theoretical content sets out properties of the entities described by these concepts and the relationships between them.

Other content will be empirical. By empirical content I mean the collected evidence that has been conceptualised in terms of the conceptual/theoretical apparatus, and is understood to provide support for it. As a purely hypothetical example, if a research programme centrally included the principle that *literature written for consumption by Victorian children tends to include magical creatures as a device to reinforce belief in the supernatural*, then one would expect the empirical content of the research programme to include a canon of examples of Victorian children's literature that had been interpreted in this way. It would be rather odd for a well-established research programme to include such a principle if there was no supporting evidence.

Now the theoretical content of a research programme is considered to be made up of a hard core, and a peripheral belt. These terms actually give a good sense of the difference. The hard core is established *with* the research programme - it is there from the start. And these are *hard core commitments*. So, if there was behaviourist research programme in psychology (as indeed there once was) and if a hard core commitment was that behaviour could be explained without considering internal mental states and drawing upon notions such as mind, then someone working *in the programme* would not explain research results in terms of internal mental states and the mind.

There may be room to argue over whether these terms are meaningful, whether they relate to entities that might actually exist, but there is no scope *within that programme* to draw upon them in explanatory schemes - as it is a hard core assumption that such ideas have no explanatory value. This is a bit like the principle in modern science that phenomena are to be explained in terms of natural causes: that does not mean a scientist cannot be a theist, just that when doing science it would be cheating by explaining natural phenomena as simply being the will of God.

Or, to offer a different analogy, an outfield association football player is not allowed to pick up the football and run with it. Of course a soccer player is perfectly capable of doing that, and there is nothing to physically prevent it, but it is not an allowed move within the rules. Indeed, it is supposedly the illegal actions of a football player who did break the rules in exactly this way which led to the creation of rugby football. You can play soccer, **or** you can handle the ball, but then you are no longer playing soccer but something else. Your practice is now part of a different programme.

And this links to Lakatos's idea of the negative heuristic. We might think of heuristics as rules of thumb: they are not set out anywhere as formal regulations, but people learn to adopt them as appropriate practice. For example, there is no university regulation that says that when a research supervisor gets ill, their research students should wish them well. But a lot of students would feel (entirely without self-interest, I am sure) that it is an appropriate thing to do. So that is more following the positive heuristic, part of the socialised etiquette of acceptable behaviour than a formal rule. There is also no rule that says that a research student should not publish articles critical of their supervisor's scholarship. However, we might think the negative heuristic of graduate study guides a research student not to do this.

So the negative heuristic in a research programme is the common sense 'rule' that one does not waste time and energy seeking to disprove what is taken for granted.

Chemists no longer do experiments to show atoms do not exist.

Anthropologists are unlikely to seek to show that culture does not exist.

The positive heuristic work in a similar way.

A psychologist working in a research programme taking personal construct theory as part of its hard core will just assume that it is sensible to interpret a person's psychology in terms of their personal constructs.

A research programme is not just for Christmas…but need not be for life

Now that is not to say that such commitments need be for life. A person working within a research programme is allowed to come to the view that some of the hard core commitments are invalid - but at that point they should look for a new research programme (like changing codes from Soccer to rugby league). Otherwise we might think of a priest who has lost their faith, but none-the-less gets up in the pulpit every Sunday to preach about sin, redemption and eternal life. It's my job, he says, and I'm good at it. We might feel that was hypocritical?

The positive heuristic refers to the guidelines adopted within the programme about how to further the programme: perhaps that some key concept needs to be developed, or some auxiliary conjecture needs to be tested; or the range of application of findings of some existing research need to be explored beyond the scope of the original study.

When a research student reads the literature that already exists, they will find position papers that set out directions for research. And the concluding sections of empirical studies often suggest how those studies move the programme forward, and recommend the next steps that are needed to build on that research.

The protective belt comprises the content of the programme which is built up around the hard core. It *builds on* the hard core, in response to the work undertaken following the positive heuristic. This belt is *protective* in the sense that it can be sacrificed if need be if further research brings it into question. That is, it protects the hard core. So, if a research study leads to unexpected results that are inconsistent with the existing content of the research programme then it is admissible to modify the protective belt of theory, to protect the hard core commitments.

As one example, Piaget's research into child development posited a sequence of stages through which is was claimed that all normally developing humans passed. In this model conceptual developmental level was seen as reflecting a core feature of cognition that would be applied across all domains of experience. This development was a one-way process - once you have achieved formal operational thinking, it will not disappear. This is not the only possible model, of course, Perry's model of intellectual development in contrast allowed regression - but it was a hard core assumption of Piaget's programme of genetic epistemology.

Yet, research undertaking within the Pigetian programme showed that children would sometimes concurrently demonstrate different stages of thinking in different domains. This could mean the assumption of a fixed sequence of stages was invalid, but as that was a hard-core commitment, new content was developed in the protective belt, notions of décalage, to explain the findings without jettisoning the core commitment.

Are there research programmes in education?

This model of research programmes was initially intended to refer to mature fields such as most of the natural sciences so might be seen to have limited potential to inform educational researchers. The social sciences such as education, and the humanities, may not only differ from the natural sciences in terms of the 'maturity' of research fields, but also in terms of the complexity of the phenomena studied, and indeed the extent to which the phenomena of interest are embedded in social contexts from which they cannot be simply excised for study.

You could ask yourself whether *you* are working in an area of scholarship that might be considered to be an established tradition within a mature field. Your response may potentially reflect whether you see your research question and design to be primarily based on an inductive or deductive approach? If you are testing a hypothesis, then you presumably feel there is an obvious choice of theoretical perspective for your research, and I suspect it is likely your work is located in an established tradition. If you consider your work as more exploratory, and open-ended, then I imagine you do not considered yourself as clearly located in a single well-established research tradition?

As part of preparing to undertake a research project, students are normally expected to undertake a programme of research-training, learning about research methods. Within an education faculty where projects are very diverse the research training is also diverse. This Faculty has taken a stance that we are not just supporting students to do a particular research project, but also to become educational researchers - and that requires familiarity with, and understanding of, the diverse range of approaches and methods used in the field. It is interesting to see how our students respond to this. I might offer a somewhat informal typology of three types of research student.

A typology of research students

The first type might be seen as goal-oriented. This student knows what they want to do. They have already pretty much decided on a theoretical perspective for the study, and the methodology they will use. They want to learn about those things that are *directly* relevant to their project, but have more sense than to waste time on material they know will not be of use in their dissertation. They may get grumpy in class when taught about material that seems too esoteric or irrelevant, but more likely are simply selective about which classes they attend. After all, they are the customer, and the customer knows best!

The second type of research student is deferential. This individual recognises they are student in a hierarchical institution. They have a course leader and a supervisor. If these people ask them to attend classes on topics that seem to have little relevance for their project, then they will do so. They are only a student, and they know that the professor knows best. They may not fully appreciate *why* the Professor thinks they should read some of *this* material or go to *that* lecture, but a good student will comply, and will be happy enough to have complied. And, perhaps if there is a little internal voice saying 'why do I need to spend time on this?' they simply remember that one day *they* may be the professor, and get to set the curriculum. As one of my relatives said to his father when he was a small child: "One day, I will be the daddy."

My third kind of research student also happily engages in all the readings and lectures, without worrying which may prove to be directly pertinent to her project or feeling obliged to do so, but not out of sense of duty, but rather because they have come to University to learn (and have perhaps sacrificed or invested considerably for the privilege) and it is great that there is so much opportunity to learn about so many different things on the course. After all, even if some of this material is not going to be *directly* useful in the short term - coming to Cambridge is not just about getting a research training, or even a degree, it is about getting an education. I call this third type of student - enlightened.

I wonder if you recognise yourself at all in one of my three categories? I assume that I probably today only speaking to two of my three classes of graduate student.

Working within a tradition seems a safe approach for the research student, as there is guidance on what you should take for granted, and on what is worth enquiring into. The positive heuristic likely offers a lot more than that. Kuhn's second meaning of paradigm was the disciplinary matrix, which consists of elements of the shared practice within that specific research community.

Does the notion of a disciplinary matrix refer to your project? Are you located within a research tradition that is sufficiently well-established to be described in this way? I can offer some guide questions you might consider:

> Are there particular conferences where you know it would be best to present your work?
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> Are there specific journals where you should best send your work, because it is likely the editors and peer reviewers will readily appreciate the kind of study you are doing?
>
> Do the studies in your area all use the same basic methodological approach, or at least a small number of related approaches?
>
> Are there particular research techniques which are widely used in studies in your field?
>
> Are there analytical approaches which seem to be widely approved of in your area of work?
>
> Are there specific ways of representing results in tables, graphs or other figures that are conventionally used in studies?
>
> Are there specialised concepts that are regularly used by the different researchers in your area?

> Are there technical terms that are commonly used, and which would need to be defined and explained to a broad academic audience, but which can be used without explanation in the specialist journals?

And so forth.

If your answer is 'yes' to many of these questions then you do indeed seem to be puzzle-solving in Kuhn's terms. You are working within a pre-existing framework, a disciplinary matrix.

Kelly's models of how knowledge progresses

George Kelly, like Thomas Kuhn was something of a polymath. Kelly worked as a psychologist and psychotherapist, but took degrees first in maths and physics, then sociology, then education, before shifting to psychology. Kelly is best knows for developing personal construct theory as a basis for his work as a therapist as he was unsatisfied with the approaches he had been trained in: approaches that offered insight into his client's troubles, but were less useful in offering them ways forward.

Kelly adopted the metaphor of person-as-scientist: the idea that in our everyday lives we all naturally adopt a scientific approach to problem-solving by informally collecting evidence, testing hypotheses, and so forth. In this, he might be thought to reflect the pragmatists who sometimes saw life as in a sense an ongoing research project, a position later reflected in the notion that action research can be seen as methodology to be applied whenever we as human beings feel our values are not being reflected in our experience of life. The person-as-scientist metaphor is not meant to imply that people are always very good at being scientists, but does commit to assuming people's behaviour is largely rational.

This is helpful for a therapist, or indeed any of us perhaps, as it means that when someone seems to be behaving in an illogical way, perhaps in a self-defeating and self-destructive way, we do not dismiss them as being crazy, but seek to understand the logic of their position. This brings to mind the work of the psychiatrist R. D. Laing, who made the argument that the apparently illogical behaviour of schizophrenic patients could be understood as a perfectly rational response to finding themselves in a mad situation.

There are also parallels here with the work of the historian who seeks to understand why people in the past sometimes seem to have had behaviours or beliefs that now seem to us very odd. Generally people are rational, so when they *seem* to be irrational it is probably that ***we*** have not fully understood their worldview. Language can be an unreliable friend here, if we can assume that common words meant quite the same thing in another time and place as they do to us today. Indeed, people of other cultures may not share our ontology of the world - they may have different conceptions of the 'joints' at which nature can be carved (to borrow a common, if violent, metaphor), in which case their language reflects a different typology than ours, and their own worldviews might be 'incommensurate' with ours as Kuhn would have said.

That does not mean comparisons are not possible, but that there is no objective viewpoint that can allow us to see both worlds equally clearly. Kuhn's argument was that translation may never be adequate in such a situation, and if we want to understand the other, we rather have to learn their language in order to see their worldviews.

Something similar is true in anthropology - cultures may seem to us to have bizarre rites and rituals - but that may be because we are still stuck in the outsider view. Thus the expectation is that ethnographic researchers spend extended periods of time living with those they research, so they can start to see the word as it appears *within* the culture. Neither the historian nor the anthropologist need to 'go native' in the sense of *accepting* the worldview they study - but rather they seek to understand its internal logic on its own terms.

Something very similar is true in my own area of research into learner's thinking about science. In studying learners' ways of understanding we do not stop at simply evaluating whether they have got the science right or wrong, but rather trying to make sense of how and why they conceptualise some phenomenon or topic as they do.

Of course, suggesting that people naturally act as scientists assumes we know what *the practice of science* is. Kelly compared two models of research:

One was '*accumulative fragmentalism*', which saw research as analogous to a collective endeavour to complete a vast jig-saw puzzle, where each piece in turn needed to be found and carefully verified, and fitted into its right place; before moving on to the next piece. Of course the problem with the jigsaw analogy is it assumes both that there is a single bigger picture to be seen, *and* that the parts of the puzzle so far completed result from genuine pieces of this puzzle, correctly fitted into the right place in the picture.

This implies that there is a single objective reality (which is generally taken for granted in the natural sciences, but a rather dubious assumption for many of the topics explored in an education faculty), and also that it is within the capacity of the human intellect to find and recognise it when it is uncovered. Why assume this?

Imagine an ant colony that is foraging across an old Roman mosaic - there is a 'bigger picture' to be seen, but who would assume that the ant colony could ever 'see' it, regardless of how long they spend exploring, or how many pheromones they exchange in the process. Of course, people are not ants, but the assumption that humans are capable of comprehending the mysteries of nature derived from religious beliefs, and once detached from any notion of humans being God's elect species seems rather arrogant.

There is an irony of course in that the modern scientists who are most scientistic - those who assume that science can and will understand everything in time - are often also those who are the most committed to atheism, and so would seem to have the least basis for thinking that evolution will provide humans with the conceptual capacity to understand the fundamental nature of the universe. Even if one leaves this aside, the idea that we are all completing a jig-saw that has been carefully turned out of its box, so we carefully keep all its pieces in sight, rather than one that includes both some counterfeit elements and pieces that belong to other pictures from other boxes, seems rather bold.

Indeed, in just about any field of scholarship there is much work that was once taken very seriously, but is now considered fatally flawed, so anyone, in any research tradition, who assumes that what is taken as trustworthy and reliable in their field right now, at this particular historical moment, is here to stay, is surely being unrealistic. The groundwork in the field you are working in has been set out by fallible human beings, and in many cases fallible human beings from a very limited range of social and cultural backgrounds. Some of these were exceptionally insightful scholars, some more sloppy workers, and a few even rather loose with their handling of the evidence: sometimes putting personal progression above academic integrity.

Yes, there are falsified data out there, and wish-fulfilling interpretations, and selective reporting of only-what-fitted-my-hypothesis (or my worldview) - and no doubt it is not just in the arts that some well-respected works should be filed under fiction.

Kelly rejected this model of research, even in science, and instead proposed what he called '*constructive alternativism*'. This was a kind of radical constructivist position, that we know the world by imagining possibilities, and then, if they seem to fit experience, treating that conditionally as reality - at least until such time as it becomes obvious some rethinking is needed. This approach does not reject the idea of an objective reality external to the human mind, but if such a reality exists then we would be unwise to assume the human mind is fully grasping it!

As Kelly pointed out, "reality is subject to many alternative constructions, some of which may prove more fruitful than others". From this perspective, progress comprises of inventing new constructions that seem useful for a while, but which may ultimately be found unsatisfactory, and so need to be replaced. In research, as perhaps in life itself, it may be sensible to enjoy the journey and not fixate on the destination. Sometimes re-imaginings, paradigm-shifts are needed.

This raises the question of where novelty comes from - from where the new ways of imagining arise which can allow a scholar to break out of habitual ways of thinking to offer an alternative conceptualisation. It is clear that this is not always easy: humans have a strong *confirmation bias* that tends to channel us to perceive the world in ways that fit our expectations.

Such bias works against innovation, and indeed much of the time it does make sense to best fit our perceptions into familiar patterns of experience. It is not the case that rain only sometimes makes us wet, or that food only sometimes satisfies hunger, or even that buses only sometimes stop at bus-stops - even when such patterns are not universal, they can still be pretty reliable.

So it is with scholarly work. If all cultures studied to date have notions of kin, then it is sensible to expect this concept will be useful in meeting a new culture for the first time. If *self-efficacy* or *distributed leadership* or *maths phobia* or *teaching style* has been found to be a valuable way of conceptualising much previous research in a programme, then it may well be useful in the next study as well. Indeed, without a reason to think otherwise, assuming it is likely to be relevant is the more sensible position to take. The problem comes when one holds on to such ideas despite having to increasingly force the data into the expected pattern.

Measurement errors are ubiquitous - so how much deviation between expected and measured outcomes are within an acceptable range? Not all our concepts or principles are expected to be universal - so how many exceptions are needed before we move beyond testing the rule, to mistrusting the rule?

Given that there are so many potential sources for things to go wrong in research - questionnaires which do not measure what they claim to measure, samples which are not truly representative of populations, the difficulty of setting up rapport and trust with research participants, the challenges of translating between languages, and so forth, the bar for deciding when to reject a widely used and well-established way of making sense of phenomena in a particular field should sensibly be a high one.

Lakatos talked of the idea of putting anomalies into quarantine, a concept we are all quite familiar with at the moment. That means that when research results do not fit with our expectations we *should recognise* an anomaly, and not simply dismiss it, but often it is sensible to take the attitude that although there is something of a puzzle to be addressed, perhaps it is not our immediate priority to address it now: it can be put on the back-burner so to speak.

So, there is a realisation that because of the complexity of research, and the many things that go wrong, it is neither illogical nor poor scholarship to sometimes put anomalies into quarantine, and carry on regardless. We notice them; we also report them; we recognise that we should not be totally content till they are understood - but we put them aside for a rainy day, in the knowledge that other work in the programme may well come to explain them as a matter of course without any special effort - and if not, we need to get round to dealing with them - one day.

Of course, that raises the question of at what point do we decide that an anomaly is too serious to ignore any longer - do we wait until our quarantine cupboard is bursting at its seams? The great thinkers, the intellectual revolutionaries, the most lauded researchers, are often those who decided an anomaly had to be attended to: it was time to discard hard core commitments, find a new conceptualisation of the field, and develop a new research programme based on different assumptions.

But how did they come up with this new way of making sense? Clearly one answer is that they were visionaries - they used their imagination. But that does not really offer an answer, just rather pushes the explanandum back a step. Why were these people able to imagine some possibility that their colleagues did not? What gave them the vision?

No doubt there are interpersonal differences here which relate to factors such as genetic make-up and early life experiences. But it is worth noting that creativity is seldom about producing something which has no precedent - in some sense there is, as the old proverb claims, nothing new under the sun.

Often the novel is found not to actually be completely new but more often borrowed from somewhere else by analogy, or that the novel is actually a new association of familiar components. So Kuhn's notion of paradigms was an extended use of an existing word that had been common in language teaching, and his idea of the incommensurability of different paradigms was the borrowing of a technical term used in mathematics and applied metaphorically in a different field. Arthur Koestler, the novelist and essayist, wrote about the importance of 'bisocation' in creative synergy - the bringing together into a new compound two discrete ideas that had not previously been seen as related - ideas from different domains, as it were.

Working outside a tradition

This process seems to reflect one of the debates within grounded theory approaches to research. In such enquiry, it is sometimes recommended that one enters the field having washed one's mind clean, except if that were literally true one would be empty-minded and not of much use as a researcher. When grounded theory methodology is followed correctly, the researcher is supposed to delay the literature review until after collecting the data so as not to develop a mind-set which might bias the way the data is understood, After all, the data is meant to speak for itself. In this kind of research, theory is built upon data, not used as a template for conceptualising it.

Except data is just data, and only becomes evidence or, of use in an argument, once it is interpreted in one way or another. So data can never really speak for itself. The way cognition works is that the analyst has to have some idea in mind to test its match to the data. The notion that the external world can directly impose its nature on the mind is something that was discredited a long time ago.

So the analyst (in grounded theory or any other kind of research) can only test out those ways of making sense of data that they can conjure up in their imagination. In genuinely deductive research this is not a problem, as there is only one pertinent way of making sense of data and that is predetermined, so either the data are found to fit or they do not. But if we are doing exploratory work then we rely on the process of post-inductive resonance that seeks to test the match between the data and the pattern our mind has imagined *might* fit - the 'induction'. That is, an insight.

If a strong commitment to a prior theory offers a binary test: an 'empty mind' has little chance of offering any pattern for testing. What the grounded theorist needs is a v*ery well populated mind* that has been exposed to a wide range of ideas that can trigger all kinds of imaginings, all sorts of potential insights that can be compared with data.

This goes beyond being aware of two or three alterntive perspectives that might be used in a particular field (in competing research programmes perhaps) as in grounded theory the assumption is that existing theories may not do the job, and something novel may be needed. And if there is nothing completely new under the sun, that may actually mean the application and modification of some idea that derives from a very different domain.

So when the grounded theorist is told to defer the literature review, this should not be understood as going into the field ignorant, rather going into the field very widely read, but with a broad brush, rather than as a specialist who has bought into a particular way of thinking, with its hard core commitments. It is not reading which is deferred, but the highly focused reading around some specific perspective or concept.

That is the best kind of preparation for grounded theory research, and so also for any scholar who thinks they might one day break the mould in their own field.

Advice to the ambitious research student

When I was a PhD student I remember coming across the advice that one should put some time aside everyday to write. It was assumed that getting down to serious academic writing was challenging, at least for many new to such work, and therefore it was important to develop habits of scholarly writing.

Writing for publication was ideal. Writing for the thesis was important. But just writing as a tool for thinking things through and for later self review and critique was a useful starting point. I think this advice is important, especially for students from disciplinary backgrounds where such writing is not already a well-ingrained habit.

However, I wish to offer some different advice today. This is that you should try to put aside some time each day for reading. Clearly that is easier for full-time students than those that are combining study with work, who may at best only be able to set out several periods of designated reading time each week. Yet even with full-time students, what I am suggesting may not be so straight forward. For I should have explained that what I am really recommending is that you should try to put aside each day some time for reading material *that does not seem to be directly relevant to your current research project.*

You can only actually know what is potentially relevant if you adopt Kelly's *accumulative fragmentalist* perspective: that is, if you think there is a single picture to be produced, and that what you already know unambiguously and infallibly offers part of that picture. If, instead, you adopt Kelly's *constructive alternativism* then you know there are different pictures that can be made, and it requires human ingenuity to imagine possible constructions that might be put on reality.

Of course, I am not suggesting you need to commit to reading material in topics or subjects in which you have absolutely no interest: this is not intended as some kind of scholarly self-flagellation. But the world is full of interesting material on all sorts of topics informed by a great many perspectives from across all disciplines. Read the material you find interesting, preferably fascinating, but read broadly.

You should read serious scholarly works, and read them both to open up your thinking to new areas, but also to look for parallels, or contrasts, with your own area of work; to look for analogies or potentially useful metaphors. Do not force this - read for interest, but remain alert to possible relevancies. If you do not find any now, you may find this reading plants seeds that offer you useful insights next month, next year, next project, next stage of your scholarly career.

Perhaps you do not need to take this advice too seriously if your only goal is to complete you current research degree, and you are confident that you are able to work in an entirely deductive manner within a closed framework that you have already completely characterised.

You also can ignore this advice if seek an academic career, but you are confident that you are working within a well-established research tradition that will continue to be a progressive research programme, and provide you a niche, for the decades ahead. If you are confident that all the anomalies can stay in quarantine, and no paradigm-shift will be indicated during your working life, and if you are content to remain a puzzle-solver, rather than become an intellectual revolutionary, then you can shelter safely within the discipline of your comfort zone.

That is, of course, unless your idea of the good life depends upon life-long learning for its own sake, because you experience genuine wonder and enjoyment in widening your horizons. Then perhaps you might at least want to step outside your disciplinary matrix when off-duty!

Thank you.