

the purpose of letting us know that their authors knew something.

Johanne Wolfgang von Goethe 2

Envisioning the Thesis as a Whole

In dreams begin responsibility.
W. B. Yeats 1

What is your dissertation about? And what contribution do you aim to achieve? What will be new or different about your work? How would you justify the time and resources that you will devote to it? These fundamental questions will seem very pressing in the beginning stages of your research, as Yeats' intangible process of locking you into a long-run project begins. But they do not go away later on. You can often push such issues into the background in the central stages of the thesis, during field visits, case studies or the hard slog of library or archive work or data collection and analysis. But they tend to return during the 'mid-term slump' in morale that often afflicts dissertation authors. And they invariably crop up again when you have a first draft of your complete thesis, and have to fashion it into a polished and defensible final version. This chapter is about the importance of thinking through some reasonable answers before you invest too heavily in a particular research topic and approach. I consider first how to define one or several questions that will inform your project as a whole. The second section looks at the demands of doing 'original' and interesting research.

Defining the central research questions

Certain books seem to have been written, not in order to afford us any instruction, but merely for

At the most fundamental level any doctorate is a contract, of a rather peculiar kind. For a 'big book' thesis the specific nature of the contract is that the author develops and communicates a question, and then proffers an answer to it. For a papers model dissertation there will rarely be one big central question, but instead a set of more loosely related and more specific or detailed research issues. Then the examiners or dissertation committee determine if the research text produced actually answers the questions posed. If there is a close fit between the question and answer, either at the whole-thesis level or within each 'paper', then the dissertation passes successfully. But students must not offer a mushy set of materials undirected to a clear question. They must not promise what they cannot deliver, or claim to achieve what they have not established. An equally common problem is that the question asked in a dissertation and the answer provided may not connect in any discernible way. The author may be convinced that they are doing X, but to the readers it seems as if they are doing Y, a significantly different enterprise. Or the question may be so broad that the answer the student provides relates to it in only the haziest way. Alternatively the question may be specific but the answer given may be too vague or ill defined to relate closely to it. Finally if some of the answer does not fit with the question asked, or if part of the question is left unaddressed or unanswered, the thesis may seem problematic. Hence the thesis contract is a demanding and constraining one both for students and for those assessing their work.

But equally the contract provides students with a great deal of protection and additional certainty. The examiners or dissertation committee are not allowed to invent their own questions, nor to demand that the doctoral candidate address a different question from the one she has chosen. The assessors *have* to take the candidate's question as the basis for assessment, within certain minimal conditions. These tests essentially require that the PhD author should establish a clear question, whether for the whole thesis or for each of its component 'papers'. She must

show that the question is a serious one and a legitimate focus for academic enquiry, which is to say that it must relate to the existing literature and debates in some sustained way. But once these conditions are established, the interrelationship of the question and the answer has to be the touchstone for accepting or failing the work undertaken.

You define the question: you deliver the answer. The unique features of this situation are often hard to appreciate. Throughout all our earlier careers in education someone else defines the question. At first degree and masters levels we can concentrate solely on delivering an answer that satisfies this external agenda. So it can be quite hard to understand the implications of instead defining and then answering your own question. Beginning PhD students often believe that they must tackle much bigger or hard-to-research questions than could possibly be answered in a PhD, just because this is the way that questions are framed in the research literature that they read. But professional researchers in universities will typically have many more resources for tackling big issues (such as large budgets, sophisticated research technologies at their disposal, large cooperative research teams, or squads of people to assist them). What is a good question for professional researchers to address is not usually a good question for someone doing a PhD thesis in lone-scholar, no-budget mode.

If attempting an unmanageable or overscaled question for a doctorate is one danger to be wary of, then veering to the other end of the spectrum carries opposite dangers. Here PhD students choose topics of perverse dullness or minuteness, thinking not about a whole readership for their thesis but only about the reactions of a few examiners or members of their dissertation committee. A topic is chosen not to illuminate a worthwhile field of study but just to provide a high certainty route to an academic meal ticket. Such defensively minded theses focus on tiny chunks of the discipline. They may cover a very short historical period, a single not very important author or source, a small discrete mechanism or process, one narrow locality explored in-depth, or a particular method taken just a little further in some aspect. The titles for such research dissertations are usually descriptive, without theoretical themes, and often circumscribed by deprecatory or restrictive labels ('An exploratory study of ...' or 'Some topics in ...').

A closely related syndrome is the gap-filling thesis, designed solely to cover an uninhabited niche in the literature rather than to advance a wider intellectual purpose. Such projects can exactly replicate an existing established analysis in a new area, or fill in a small lacuna in knowledge between a set of already studied points. There are two problems with empty regions, however. The first is that gaps often exist for a good reason; for instance, because the topic has little intrinsic interest or is too difficult to undertake. The second problem is that the most obvious holes in the literature that are worth studying may easily attract other researchers. Hence someone else may publish research or complete a PhD on the topic over the three or four years that it will take you to produce a finished thesis. Potential competition from other people's doctorates or from well-funded research projects is a serious risk for any gap-filling thesis. A study whose chief rationale is that it is the first treatment of something may be substantially devalued by becoming the second or third such analysis.

There are longer-term problems with picking a defensive or an overcautious topic just to get finished. Once your PhD is completed its title will have to be cited on your résumé or curriculum vitae for many years to come. Your doctoral subject will only cease to matter professionally when you have built up quite a body of later work to succeed it, especially a later book. So while a completed PhD is a fine thing, a very dull, off-putting, or unfashionable subject is not a good foundation for getting hired into your first academic job. Especially at the short-listing stage, most university search committees operate with only a small amount of paper information. Unless you have a set of different publications already in print, they naturally tend to read a lot into your PhD subject, seeing it as expressive of your character and temperament. In addition, it may be very hard to spin off any worthwhile publications from a completely dull PhD.

It's no good running a pig farm for thirty years
while saying 'I was meant to be a ballet dancer'.
By that time pigs are your style.

Quentin Crisp ³

These considerations can be magnified by the psychological effects of fixing on a boring or tiny subject for 'manageability'

reasons. Most doctoral students experience some form of mid-term slump in their morale, one or more periods when they lose confidence in their project and wonder if it is worth continuing. If your topic is inauthentic for you, if you are not genuinely interested in your thesis question and committed to finding an answer to it, then it will be all the harder for you to sustain your confidence and momentum through such periods. It is also pretty demotivating at this stage to become aware that you have picked an uninteresting or uninspiring topic that is unlikely to maximize your later career prospects. So it is important to take seriously the scope to configure what your research will be about, avoiding both overreaching topics and underambition. Your own personal commitments and interests count first here, of course. But other people's views do as well.

The challenge posed by having to explain your thesis topic can also be a salutary stimulant to clarifying your own thinking. During the course of your doctorate there will be grueling some occasions, at dinner parties or drinks with strangers, when someone turns to you and asks what it is you do. Once you admit to working on a doctorate, your conversation partner's inevitable follow-through is to ask about your subject. From this point on you have typically about two minutes to convince your normally sceptical inquisitor that you know what you are doing and that it is a worthwhile thing to be at. As a PhD student you are often assumed to be highly committed to and closely bound up with the subject you have chosen. Both insiders and outsiders to university life may think of your personality as reflected in (even defined by) your research topic. People doing doctorates are invariably seen as more committed to (even obsessed by) their particular subject than would be true of professional academics doing research later in their careers. So the 'dinner party test' is always a frustrating experience to undergo, and many students feel that it is an impossible one for them to pass. To expect them to be able to capture the essence of their sophisticated and specialized topic, and to convey it in a few lines to a complete stranger, is just absurdly to underestimate what they are about. Yet in my view the test is a good one. If you cannot give a synoptic, ordinary language explanation in two or three minutes of what you are focusing on and what you

hope to achieve, the chances are very high that in a very fundamental way you do not yet understand your thesis topic.

You define the question: you deliver the answer. This proposition means that every effective PhD thesis should be genuinely personalized in some way. You should take a manageable part of the existing literature's questions or concerns, and then tailor or modify that topic so as to shape it so that it can be feasibly answered. The way that the question is shaped should be reasonably distinctive, coming at a subject from a personally chosen angle. If you have such a personalized (even mildly idiosyncratic) perspective then it is less likely to be adopted by other researchers during the course of your studies.

It is best to try and frame your thesis around an intellectual problem or a paradox, not around a gap. It needs to focus on a set of phenomena that ask for explanation, which you can express as a non-obvious puzzle and for which you can formulate an interesting and effective answer. The philosopher Robert Nozick recently asked, 'What is an intellectual problem?' and concluded that it had five components.⁴ The first is a goal or objective which can tell us how to judge outcomes, how to see that an improvement has been achieved. The second is an initial state, the starting situation and the resources available to be used, in this case usually the existing literature. A set of operations that can be used to change the initial state and resources forms the third component of an intellectual problem, perhaps new data and a toolkit of research methods. Constraints are the fourth element, designating certain kinds of operations as inadmissible. The final element is an outcome. A problem has been solved or ameliorated somewhat if a sequence of admissible operations has been carried out so as to change the initial state into an outcome that meets the goal without breaching the constraints in doing so. In French doctoral education this broad approach to defining a topic is often characterized as a search for 'une problématique'. The synonymous English word 'a problematic' is too ambiguous with the adjectival 'problematic' (meaning 'difficult') to play an equivalent role. However, if you think of 'problematising' your thesis question – setting the answer you hope to give within a framework which will show its intellectual significance – then you will get near to what the French term means.

Father Brown laid down his cigar and said carefully, 'It isn't that they can't see the solution. It is that they can't see the problem.'

G. K. Chesterton 5

Most problems people face cannot be specified with such exactness. And often people do not simply *face* given problems; their task is to *make* a problem, to *find* one in the inchoate situation they face.

Robert Nozick 6

Many PhD students from countries or disciplines with more empiricist approaches, or placing more emphasis on intellectual or social consensus, find the idea of problematizing their thesis topic difficult and odd. They often regard their chosen topic as obviously worthy of study or intrinsically interesting and important in common-sense terms alone. They see no puzzle or enigma in front of them, merely an empirical landscape only partially painted by previous authors, which is their opportunity. This is a dangerous state of mind to be in at the start of a doctorate. It is often associated with people picking overly derivative topics important at some previous levels of education, or taking on very conventionally framed subjects from the existing literature which are too large or difficult to resolve in a PhD. Above all, an 'unproblematized' thesis topic normally provides students with no worthwhile intellectual focus or protection at the examination stage. It leaves open too many questions along the lines of: what is this thesis for?

You define the question: you deliver the answer. The proposition is symmetric, with equal scope for you to intervene on both parts. The quickest way to get a great fit between the question asked and the answer delivered in a thesis is to try and work out what you will be able to say, or hope to be able to say. Then frame your research question so as to fit closely around it. You must find legitimate ways to leave out bits of the research literature's questions or concerns that you are not going to be able to answer or will not feel comfortable tackling. That means you must think about the practicalities of research and your capabilities and resources from the word go, 'guesstimating' results and outcomes at the same time as you formulate a topic.

In a sense this exercise is like turning to the answer pages at the back of a maths textbook before you work out how to derive the right result. It is no use formulating a great topic that depends on your achieving a theoretical breakthrough that has eluded previous scholars, or turns completely on your empirical analysis producing results of a particularly clear or convenient kind.

It is fine to be hopeful and to think about a best possible case: what would you be able to say if everything went just as you hope that it will? But you also need to build in some insurance outcomes, things you can do or say if high-risk elements of your plan do not turn out as hoped. For instance, if you initially believe that you can achieve a theory advance, there is still a risk that it will prove more elusive than you anticipate. In this case, can you fall back on something more reliable and predictable, such as the exegesis of and commentary on an important author's thought in the same area? Or if you hope to establish a strong relationship between variables A and B in an empirical analysis, what will be gained from finding that this linkage does not exist or is only marginally present? These considerations mean that you must structure your question robustly, with a measure of redundancy in your research plan, so as to cover what you will do in your thesis even if some elements of the plan do not turn out as intended. Above all, you need to shape the thesis question to showcase your findings, to bring out their interest and importance and to give a sense of completeness to the whole.

These things are not easily accomplished. They are not tasks to be finished in a single effort at the outset of your thesis and with a high level of determinacy. Instead they mostly have to be discovered a bit at a time, and then worked up in successive attempts. Shaping your question to fit around your answer involves repeated iterations where you define a plan and formulate some ambitions. Then you do some lengthy research and painfully produce some text expressing your understanding of the results. After that you consider how far the thesis plan requires alteration (perhaps including wholesale redesign) as your ideas and level of information have changed. Your early ideas on what your thesis will look like, in your first six months or first year, will be like those of a sculptor choosing a block of stone and marking the crudest 'rough form'

concept on it, before embarking on the long job of chiselling out a finished piece.

Doing original work

All good things which exist are the fruits of originality.

John Stuart Mill 7

We never think entirely alone: we think in company, in a vast collaboration; we work with the workers of the past and of the present. [In] the whole intellectual world ... each one finds in those about him [or her] the initiation, help, verification, information, encouragement, that he [or she] needs.

A. G. Sertillanges 8

Authoring and thinking go together. You will very rarely work out what you think first, and then just write it down. Normally the act of committing words to screen (or pen to paper) will make an important contribution to your working out what it is that you do think. In other words, the act of writing may often be *constitutive* of your thinking. Left to ourselves we can all of us keep conflicting ideas in play almost indefinitely, selectively paying attention to what fits our needs of the moment and ignoring the tensions with what we said or thought yesterday, or the day before that. Writing things down in a systematic way is an act of commitment, a decision to firm up and crystallize what we think, to prevent this constant reprocessing and reconfiguring. Like all such resolutions of uncertainty, making this commitment is psychologically difficult, possibly forcing each of us to confront the feebleness or inadequacy of our own thought. This potential for disappointment can in turn create incentives for us to postpone starting to write, a chain of reactions which may culminate in 'writer's block' even for very experienced authors (see Chapter 6).

For beginning doctoral students, however, the most characteristic source of uncertainty closely associated with a choice of

topic is whether their work will fit the normal 'original work' requirement. All good universities in either the classical or the taught PhD models still demand that the thesis or dissertation should be novel research making some form of distinctive contribution to the development of knowledge in a discipline. What kind of work meets this criterion is famously difficult to pin down. Most European universities' doctoral rules (or rubrics) are almost silent on how originality is to be determined. Instead they concentrate on process, requiring only that suitably qualified examiners be recruited to sign off on the presence of original work (whatever that is). The University of London has a much more explicit specification than most, but even this tells examiners only that a doctoral thesis can show originality in two ways. Either it will report 'the discovery of new facts', or it will display 'the exercise of independent critical power', or both.⁹ 'New facts' are the result of empirical researches, and can be established by undertaking an investigation of something not hitherto available. For instance, this could include reading and commenting on little-analysed documents; exploring unreported or unpublished parts of an archive; conducting a case study in a locality or organization not previously or recently studied; running a survey, or collating together published quantitative information, and then statistically analysing the data; and so on. 'Independent critical power' is almost as vague a criterion as 'originality'. But presumably the idea here is that the thesis author shows that she can marshal some significant theoretical or thematic arguments in an ordered and coherent way, and can explore already analysed issues from some reasonably distinctive angle or perspective of her own.

The notion of 'independence' is an important one at the doctoral level. A candidate for PhD is supposed to speak with their own distinctive professional 'voice' on major issues in their discipline. This aspect may be less visible in those countries where PhD students are expected either to generally assist their supervisors in their work, or to be apprentices labouring in their department's vineyard on a designated topic (while also undertaking activities like teaching). It can be disconcerting for these students to appreciate the importance of the 'independent work' criterion for awarding the doctorate. Newly PhD-ed

people cannot be clones of their supervisors, nor even just walk in their footsteps. Often this realization dawns on students quite late, sometimes in the run-up to a final draft, or perhaps in the final oral examination itself. For highly insulated PhD students it may even come much later, when their attempts to publish papers or a book from their doctorate are rebuffed, or when appointments at other universities prove elusive.

Framing your own view while still grounding your work in an established academic tradition and some part of the contemporary discourse of your discipline, is a knack that takes time to develop. There are two common ways in which beginning students may go wrong: either being overly derivative from the existing literature on the one hand, or overclaiming about the novelty or value of their own contribution on the other. The first excess is to structure your opening chapter or chapters exclusively or extensively around summaries of a succession of previous books and articles. Here references and quotations are obvious crutches, used to limp along from one point to the next. A telltale sign of this syndrome is a long succession of paragraphs where the opening words of *every* paragraph are somebody else's name and reference: 'Smith (1989) argues...' or 'According to Jones (1997)....'. Writing in this manner simply signals to readers an unintended message: 'Here comes yet another derivative passage.'

If you speak of nothing but what you have read,
no one will read you.

Arthur Schopenhauer 10

Do not read, think!

Arthur Schopenhauer 11

Especially for students doing 'big book' theses, the scale of the research literature's questions often suggests that they should begin their own work by writing long literature reviews in an effort to try and somehow absorb it within their covers. This exercise can produce many thousands of words in exegesis. But surveying other people's contributions typically yields only superficial coverage or criticisms of earlier studies. It does not necessarily get you any closer to finding your own distinctive

question. And it can accentuate an inability to cut down or personalize your thesis topic. Perverse effects here are often serious. It can be very depressing to set out trying to answer someone else's question, and progressively discover that with the limited time and resources at your disposal it cannot be done. An overextended literature review can also consume vast amounts of time, often leading students to postpone doing any creative work of their own for a year or 18 months. Even so, less confident supervisors and advisers often encourage this pattern of behaviour. If they are unfamiliar with your precise topic, a literature review can seem functional for their needs, providing them with a quick potted education about it.

The experience of doing a literature review may also subconsciously foster in you an illusion that is the occupational hazard of text-orientated intellectuals – the idea that the solution to conflicting theoretical positions, and to identifying a particular position of your own, can be found in conducting a super-extended trawl. Somehow the lure of the hunt or the quest often persuades people that with a bit more effort they can turn up 'the answer'. But a solution to your theoretical, methodological or empirical problems does not necessarily lie out there in the literature. Reviewing more and more of other people's work will not in itself throw up the insight or angle you need.

The world does not contain any information. It is as it is. Information about it is created in the organism [a human being] through its interaction with the world. To speak about the storage of information is to fall into a semantic trap. Books or computers are parts of the world. They can yield information when they are looked upon. We move the problem of learning and cognition nicely into the blind spot of our intellectual vision if we confuse vehicles for potential information with information itself.

Ivan Illich 12

Of course, it is still always a sensible precaution to undertake some form of systematic documentation and bibliographic search at the outset of any PhD, so long as you assign it a strictly

limited time frame. A speedy but comprehensive review of previous work on your topic is especially easy now that Web systems and computerized bibliographic tools are available. They offer much more sophisticated search facilities and far faster access to source materials than was state-of-the-art even five years ago. Electronic journal archives should mean that you can now instantly download the abstracts and full text of potentially relevant academic papers. These tools have also extended the reach of searches to include possible rival PhDs already ongoing or just starting in your own or other countries. A search for closely similar PhDs is a worthwhile precaution to take before committing to a topic. But again do not fall into the trap of thinking that the originality of your work hangs on your 'owning' a PhD topic exclusively. Your best defence against being trumped by other people's ongoing research lies in a distinctive and personalized framing of your thesis question and approach, not in having a deserted niche or a 'gap' topic all to yourself.

A second frequent mistake is overdoing things. Beginning students often overclaim about the novelty of their ideas or approach. They make rash promises of theoretical or empirical breakthroughs that do not materialize. Or they adopt and promote various innovations at the start of their theses that do not seem to be justified by actually doing any useful work later on. Academic readers are especially resistant to 'neologisms' (the invention of new terms and vocabulary). They also will hate your interpreting established terms with a different meaning from those already in use. And many will resist the introduction into your analysis of novel 'conceptual frameworks' or algebra or diagrams, *unless* these strategies seem to add significantly to your analysis. Any of these tactics may encourage readers to anticipate more from your work than is actually going to appear, and so risks a major failure in managing their expectations. The quickest and surest way to boost readers' resistance is to set out your views while denying *any* influence from earlier work in your discipline, or insisting that ideas already in common currency have somehow originated or re-originated with you alone.

Somebody says: 'Of no school I am part,
Never to living master lost my heart;
No more can I be said

To have learned anything from the dead'.
That statement – subject to appeal –
Means: 'I'm a self-made imbecile'.

Johanne Wolfgang von Goethe 13

An artist who is self-taught is taught by a very
ignorant person indeed.

John Constable 14

Steering a middle way between being a non-independent voice and overclaiming is a difficult course. One foundation is to recognize that any new work rests on an accumulation of previous and current literature, as the epigraph from Serpillanges at the start of this section makes clear. A useful device to bear in mind here is the 'value added' concept, which also links back to Nozick's issue of how you 'solve' or progress an intellectual problem (see pp. 23–4 above). A business 'adds value' when it pulls in resources at price X and then recombines or processes them to create an output which can be sold on for a higher price Y. The difference between X and Y is the 'value added'. Focusing on your own 'value added' means keeping a critical eye on the extent to which you have transformed or enhanced or differentiated the starting materials of your analysis. Then tailor your claiming behaviour to fit closely with that. It also means retaining a strong relational pattern of argument in which you appropriately acknowledge the extent to which you draw on the existing literature. But you can perhaps ensure that you seem on top of rather than overly dependent on previous work by treating these debates in a more organized way, as a competition between clearly labelled schools of thought, each of which has merits or insights but also limitations.

A new theory, even when it appears most unitary
and all-embracing, deals with some immediate
element of novelty or paradox within the
framework of far vaster, unanalysed, unarticulated
reserves of knowledge, experience, faith and
presupposition ... We neither can, nor need,
rebuild the house of the mind very rapidly.

Robert Oppenheimer 15

The nature of academic debates is such that complete closure of many controversies is unlikely. More than four centuries ago Blaise Pascal remarked about the incomplete establishment and yet persistence of religious belief: 'We have an incapacity of proof, insurmountable by all dogmatism. We have an idea of truth, invincible to all scepticism.'¹⁶ Something of the same condition is the best that can be hoped for of any academic viewpoint in contemporary debates. Participants often share a common vision of what disciplinary advance consists in, but disagree strongly on which contending position best meets these criteria. No one 'line' will ever sweep the field or be without its critics and dissenters. The normal 'resting state' of most academic disciplines is that there is a 'conventional wisdom' in J. K. Galbraith's sense of a mainstreamed, seen-as-unproblematic viewpoint.¹⁷ This position usually controls the intellectual commanding heights, the councils of professional bodies and the editorial control of the (most) prestigious journals. However, there will also usually be one or more 'insurgent' critical views – new or previously minority positions that are attracting support. Often there are also one or more 'legacy' views critical of the orthodoxy as well. These are older positions now displaced in large part by the conventional wisdom but still staging rearguard actions or successful guerrilla attacks. The maintenance of continual academic debate means that you need to think through carefully the position that you expect to adopt. Bear in mind the likelihood that intellectual viewpoints will significantly change over the course of the three or four years it takes you to finish your doctorate. There may well be extensive jockeying for position or even a change of mainstream approach in your discipline during this period.

Once you have a good sense of where your interests lie, and can relate your question effectively to the research literature, the hard part is to sit down and try to contribute, that is to push ahead knowledge in some particular area or endeavour. A potent reason why we all tend to overextend literature reviews is that doing so postpones this psychologically taxing moment when we have to think through ideas for ourselves. Facing a blank sheet of paper and attempting to jot down new thoughts or make interesting connections can often seem threatening. In a university environment surrounded by the massed ranks of

learning in the library, and by so many other people seemingly adept at the task, not all the influences to which you are exposed are necessarily supportive ones. For instance, being in an institution with a strong historical tradition of advanced study in your discipline can be encouraging for creative thought in some circumstances, as you seek to emulate previous generations of doctoral students. But such an apparently favourable context can also be intimidating and disabling in other ways, for instance suggesting that many of 'your own' ideas have already been devised by others.

Most people would die sooner than think; in fact they do so.

Bertrand Russell ¹⁸

Few people think more than two or three times a year; I have made an international reputation for myself by thinking once or twice a week.

George Bernard Shaw ¹⁹

Thinking on your own is also difficult because genuine learning has a kind of dialectical feel to it. Just as you cannot build up stronger muscles in a limb until you have in effect strained or torn the ones you already have by vigorous exercise, so you cannot really internalize new ideas without losing something of the previous mental framework you used to make sense of the world. Hence we all encounter a small dread that we will lose confidence in what we previously believed, yet without replacing that earlier, thoroughly familiar, and competently working model with a new set of ideas that we can use as effectively and felicitously. If that happened we would know that we did not know how to interpret some phenomena, and be worse off. Perhaps we would be aware of the set of ideas we really need now, but still be unable to thoroughly master or understand them. For PhD students, aspiring to operate on the frontiers of knowledge at a professional level, this outcome would be an especially disturbing one. This risk adds a further twist to the asymmetry noted by Jean-Baptiste Biot: 'There is nothing so easy as what was discovered yesterday, nor so difficult as what will be discovered tomorrow.'²⁰

You have learnt something. That always feels at first as if you had lost something.
George Bernard Shaw 21

I'm not afraid of failure ... If you are learning anything new, you have got to get through humiliation.
Eddie Izzard 22

One does not set out in search of new lands without being willing to be alone on an empty sea.
André Gide 23

So far, perhaps you feel, so depressing. But there are also definite routines and regimes which you can develop to help you do creative thinking more easily and frequently. Recognizing the difficulties in being original is a crucial first step, for it means that you can take appropriate encouragement from small forward steps, rather than setting your sights unrealistically high. As the quotations from Russell and Shaw above make clear, a key first step could simply be to set aside time so as to purposefully try and develop your own ideas. Make sure that these session times are sufficiently long to be worthwhile, usually at least an hour or two. On the other hand, there may also tend to be diminishing returns in much longer sessions. It may not be realistic to seek to be creative for hours on end. Develop the habit of thinking in a fairly disciplined way that works for you, splitting your think-time into separate stages where you try to do only one discrete operation at a time.

It is *always* best to begin by surfacing or 'brainstorming' ideas in a deliberately uncritical mode for at least 15 or 20 minutes. During this time jot down everything that occurs to you about or around a topic, without editing, evaluating or scrubbing out any of your ideas at all. When this period is over, you should have a full ideas sheet (covering one or several pages), littered with jottings and annotations and stray thoughts. Once this stage is over, you can move on to evaluation and organization, spending an equivalent amount of time thinking carefully about how each of the elements on your ideas sheet relates to your central question or problem. At this point cross through or

marginalize jottings or possibilities on the ideas sheet which are not really relevant, or which will not work as you wish. (But since it's also easy to be too self-critical, cross things out lightly, so you can still read what's there.) Then think about how to organize or sequence the remaining ideas, using graphic devices (boxes, lines, arrows etc.) to structure your ideas sheet. As you make progress, take the skeleton of one subset of ideas and expand it onto further ideas sheets of its own, seeing if you can flesh out and expand what you have got.

Jotting thoughts down whenever you have them is a second seemingly obvious but actually crucial aspect of increasing your creativity. Nothing is so evanescent as your own good ideas, so fleetingly present and so easily lost. One of the most famous social psychology articles sheds light on this issue, focusing on 'the magical number seven, plus or minus two'.²⁴ Empirical research shows that on average we can all of us hold only about seven ideas at the forefront of our attention. Very clever people are perhaps able to focus on nine ideas at once, while less adept people (like me) may only be able to concentrate on five ideas at a time. When we are confronted by larger sets or longer lists of ideas we tend to react by *randomly* dropping some elements from the forefront of our attention. Hence if you think of a lot of ideas *without* jotting them down, you may appropriately be anxious that you will forget them.

The best way to get a good idea is to get a lot of ideas.

Linus Pauling 25

There is no such thing as a logical method of having new ideas or a logical reconstruction of this process.

Karl Popper 26

One way we normally counter this fear of forgetting is to keep recycling the same seven (or five or nine) things in the forefront of our attention, the repetitions serving to reassure us that the original notions are still there, still retrievable. The more stressed we get (often without noticing it) the more we may repeat this operation, squeezing out having any new ideas. To get new ideas you need to break out of this cycle of anxiety

and recycling. Jotting things down as notes in a regularly maintained or filed notebook, or in a well-saved and cumulative file on your PC, is a key step. It creates what Montaigne called a 'paper memory', which normally helps enormously to give you the psychological security to move on and think of additional ideas, secure in the knowledge that you will not forget what was value-added or worthwhile in today's session.²⁷

Chance gives rise to thoughts, and chance removes them; no art can keep or acquire them.

A thought has escaped me. I wanted to write it down. I write instead, that it has escaped me.

Blaise Pascale 28

Creative research is a problem-generating activity.

Problem discovery cannot be a scheduled activity. It can happen at any time.

Lewis Minkin 29

Jotting everything down also means keeping a notebook of problems or questions or possible ideas for development with you constantly – for use in seminars, during conversations with friends and colleagues, when you are out and about, and even perhaps by your bed at night. It is best to have a system for your jottings that allows you to keep your records safely, but also allows you to extract sheets for refiling in appropriate folders or files. Using a PDA (personal digital assistant) may also let you transfer ideas or jottings directly onto a PC-based filing system. You cannot afford to have these materials floating around on whatever scraps of paper are to hand, for then they may still get lost again, undermining the psychological security you need to stop recycling what you already have and to instead think of new ideas. You can also use this notebook (or a PDA linkable to the bibliography file on your PC) for securely capturing references to potentially relevant literature (see the second part of Chapter 5).

If you assiduously jot things down you can also take full advantage of the well-documented tendency for people saturated in a field of study to get creative ideas or breakthrough insights by chance associations, almost when they are not

looking for it. This pattern may reflect your subconscious helping out by processing difficult issues in the background over long periods. It may also reflect the fact that as your knowledge of an area builds up, so your anxieties about forgetting or not understanding tend to ease, as you gain the confidence and psychological security to think about things afresh rather than relying on other people's insights.

In the field of observation, fortune favours only the prepared mind.

Louis Pasteur 30

It is in our idleness, in our dreams, that the submerged truth sometimes comes to the top.

Virginia Woolf 31

Strengthening your motivation for doing original thinking is important too. Making a commitment of some kind – to an intellectual approach, a particular school of thought in your discipline, or a broad world view – all these can be helpful in suggesting an angle of attack for you, as the quote from Hamilton below suggests. Of course, you will always need to retain a capacity for relational argument. You must be able to recognize when a view you might want to hold is not credible or defensible. But so long as these conditions are met, the impetus provided by a reasoned commitment can be a helpful spur to ingenuity, encouraging you to look harder for particular ways of surmounting difficulties. Again some students who take an empiricist or 'common sense' view of what they are doing in their doctorates find this advice hard to apply to their work. But there is no worthwhile 'purely factual' research, even in the physical sciences.

Those who stand for nothing, fall for anything.

Alexander Hamilton 32

Making a commitment does not entail *over*-theorizing your work, or linking it to unnecessarily high-flown ideas with little relevance to the value-added elements of the dissertation. Avoiding extraneous materials is an important part of keeping