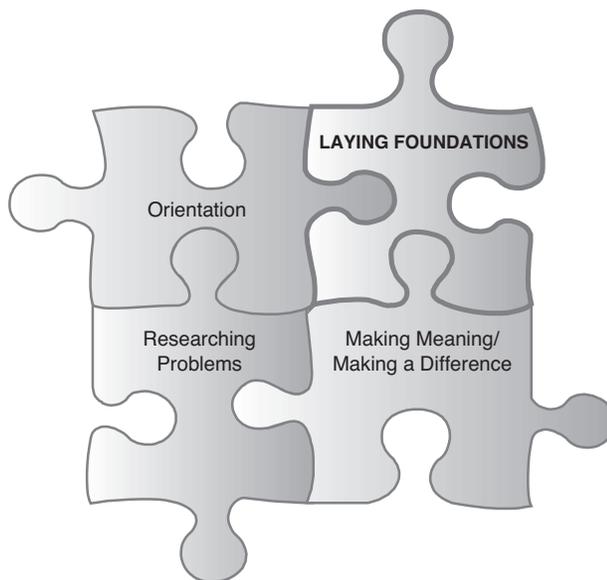


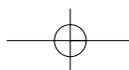
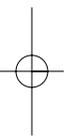
Part Two

Laying Foundations

- 2 From Real Problems to Researchable Questions
- 3 Preparing to Research Real-World Problems
- 4 Striving for Integrity in the Research Process
- 5 The Quest for 'Respondents'
- 6 Selecting Appropriate Data Collection Methods



Chapters 2–6 lay the foundation for understanding and engaging in research that tackles real-world problems. Chapter 2 explores the complexity inherent in problem situations and works you through the challenge of moving from tangible problems to 'researchable' questions. Chapter 3 covers some of the essential preparatory work necessary for credible research, and provides context and method for drawing on literature, before working you through the challenges of methodological design and proposal development. The focus of Chapter 4 is research integrity and looks at issues of credibility in relation to the production of knowledge and ethical issues related to responsibility for the 'researched'. The aim of Chapter 5 is to help you think about the need to define and select your respondents, while the final foundational chapter takes you through the issues and techniques associated with real-world data collection.



2

From Real Problems to Researchable Questions

Chapter Preview

Problems and Possibilities
Identifying Problems Suitable for Research
Unpacking Problems
From Problems to Research Questions

'The scientific mind does not so much provide the right answers as ask the right questions.'

- Claude Lévi-Strauss

PROBLEMS AND POSSIBILITIES

A world without problems ... sounds good doesn't it? But if you really think about it, wouldn't a world without problems also be a world with no motivation to transform, progress or evolve; wouldn't it be a world that is, well, stagnant? Okay, you might argue that there would be no need to 'progress' if there were no problems, but that indicates an ambivalent attitude towards change and a framing of the word 'problems' that is less than constructive. For if it is problems that motivate change, perhaps it would be more productive to see problems as opportunities – to frame problems as potentialities. Problems could then be more than just dilemmas, impediments and obstacles – problems could, in fact, be challenges that open up a world of possibility.

Defining problems

So how do we define a problem? Well, defining a problem is something quite different from simply providing a definition of the word. For example, a definition of 'problem' might be:

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Problem: A situation where there is a gap between what is real and what is ideal or desired.

The definition itself is fairly straightforward. What is more complicated, however, is the challenge of figuring out what qualifies as or constitutes a problem. In the case of the definition above, it means knowing what is 'desired' and what is considered 'ideal'. And that can involve factors such as worldviews, personal and societal norms, ethics, morals, values, politics, law, economics etc. In other words, problems are far from universal.

Take, for example, euthanasia. Is it less than what is 'desired'? Is it less than 'ideal'? Well the answer will certainly rest on perspective. For the Catholic Church, euthanasia would certainly be a moral problem. For a hospital it is likely to be an ethical, political, legal and economic (if there is one thing hospitals hate, it's lawsuits) problem. When it comes to the patient's family, some may find it a moral and/or legal nightmare. But for others, it may be exactly what is 'ideal' and 'desired' within a desperate situation. For these family members, and many patients themselves, euthanasia is a solution rather than a problem.

Situations can also become defined as problems as times and cultures shift and transform. For example, the state of the environment was not a problem until it was defined as such – first by activists and more recently by governments. Domestic violence was not a problem until a cultural shift saw wives as something more than the property of their husbands. And wage differentiation based on race has certainly been deemed a problem in the West, but unfortunately there are still many cultures in which race determines both social and economic status.

So if problems are defined by a gap between what is 'real' and what is 'ideal or desired', and what is ideal or desired is very much dependent on perspective and cultural/historical realities – then deciding what constitutes a problem has the potential to open up tremendous debate. The implication for researchers is that problem identification needs to involve consideration of the various realities and perspectives that inform an issue.

IDENTIFYING PROBLEMS SUITABLE FOR RESEARCH

Now the real-world problems you are likely to identify will come in all shapes and sizes, for example social problems, political problems, economic problems, workplace problems, policy problems, global problems, local problems etc., etc. So the challenge is knowing how to choose, where to start and how to focus your inquiry so that you and your research can make a real impact – make a real contribution to change.

Well it's probably worth keeping in mind that one identifying feature of researching 'real-world' problems is that the task tends to be 'applied' – the

Real Problems to Researchable Questions

conduct of research is undertaken with the express goal of problem solving or facilitating situation improvement. It is more than just an academic exercise or a task designed to satisfy curiosity. So whether your problem is identified through your own knowledge and experience, from broader societal issues, or from listening to the needs of others, it is important to remember that researching real-world problems is an inherently practical affair. Problems suitable for research are problems where you can make a difference.

Drawing on knowledge and experience

Many times it will be your own insights and experiences that point you towards a problem that clearly needs to be researched. Take the workplace for instance. Just about anyone who has ever had a job will tell you that workplaces are rife with problems. For example, what employee doesn't grapple with red-tape, inefficiencies, ineptitude, incompetence, decision makers not in touch with the coal-face, corruption, profit before service, morale and motivation? Your own frustrations are often tied to the frustrations of many – and if they can also be tied to the goals, aims, objectives and vision of the organization, community, or institution in which they sit, then there is a good chance those very frustrations will have 'research' potential.

On the other side of the coin are problems that are not so much attributed to inefficiencies or inadequacies within the workplace itself, but are tied to the client groups with which you might work. Some examples here might be unmotivated students, communities reluctant to recycle waste, or patients who simply cannot/do not follow dietary guidelines. Again, if these frustrations are widely accepted and can be linked to organizational/community goals – then they are likely to have research potential.

Exploring broader issues

Extending beyond your own workplace experiences are problems tied to broader societal/political agendas. For example, timely or contemporary issues such as inadequacies in health care systems, blue-green algae blooms in the local catchment, a sudden increase in high school dropout rates etc., which overlap with your own interests and/or organizational goals, are good issues to explore. At times, growing political interest, sudden media coverage, or even new legal requirements may be enough to motivate a need to conduct research into a particular problem area.

Another sure fire way to locate 'researchable' problems is in literature. The importance of reading for research cannot be overemphasized. When you are conversant with topical literature it becomes quite easy to find researchable problems. You can explore whether an important aspect of a problem has been ignored; whether assumptions underpinning problem investigation need to be re-examined; or whether further questions related to a particular problem have been posed by

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researchers at the end of their research papers. If you can identify gaps and holes in the literature, you can quite readily generate relevant research topics.

Learning to identify 'needs'

Another approach to identifying suitable research problems, which can open up a whole world of possibilities, is to consciously work towards identifying the needs of others. For example, say your insights suggest that there is a problem – perhaps a sense of dissatisfaction coming from a particular group or from within an organization. It may be worth undertaking a preliminary investigation that can identify the problem or problems from the perspectives of the group. In fact, uncovering these perspectives might end up being a major research question in its own right.

Identifying problems and needs might also come from following media coverage, reading letters to the editor, or listening to stakeholders at various forums including town council meetings, workplace meetings, or any other place where stakeholders may gather to express their concerns.

Now at times, needs identification can be overlooked. There is a real history of researchers 'knowing' there is a problem and 'assuming' that their knowledge is paramount. The researcher is the expert who identifies problems and works towards solutions. But this 'non-reflective' approach to problem identification has left many researchers scratching their heads and wondering why so little sustainable change has come from their research initiatives. Keep in mind that it doesn't matter how legitimate your research findings and recommendations are if they're not meeting the needs of those facing the problems you have identified. Sustainable change is often dependent on making sure that what an expert deems is a problem is actually identified and prioritized as a problem by stakeholders themselves. In short, listening to, and identifying the needs of stakeholders is paramount.

A good strategy here is to undertake a stakeholder analysis. This generally includes:

- Identifying the scope, extent, or number of people/organizations likely to be: (1) adversely affected by a problem situation; (2) causal to a problem situation; and (3) involved in potential problem alleviation. For example, if your problem was a lead smelter operating close to a residential area, stakeholders would include those adversely affected by the smelter, i.e. local residents, local schools, parent groups, smelter employees etc.; those causal to the problem, i.e. representatives from the smelter and/or their parent company; and those who could help in problem alleviation, i.e. the health department, local government authority, the Environmental Protection Authority etc.
- Finding out whether, how and why the problem at hand is seen as an issue or priority issue by the various stakeholder groups identified above.
- Recognizing that even within various stakeholder groups there can be a diversity of attitudes and opinions.

Real Problems to Researchable Questions

Remembering practicalities

We'll talk about practicalities more fully when we look at the potential methods you might use to investigate your 'problem', but even at the stage of problem identification it is worth keeping practicalities like researchability, funding and political support in mind.

Now we know the world is full of problems, but not all of these problems can be solved through research, and fewer still can be solved through short-term, relatively small-scale research projects. This makes being practical exceedingly important. For example, the threat of a major asteroid strike is a real problem for the human race, but it's not a problem likely to be solved through the conduct of a small-scale research study with a limited budget. Or say the problem you have identified is that your manager is a real *#^&! In this case, not only do you have to think about how a research study might or might not inform/help alleviate the problem, but how you might be able to do such research and keep your job at the same time! Now this is not to say that a problem needs to be politically judicious to be researchable. It does, however, highlight the fact that before you decide to research any particular problem, you need to be prepared to carefully consider and sensitively manage political (and financial) realities. In selecting a problem suitable for research you need to think about what you can do, but also what you can't do (see Box 2.1 for a few examples).

Box 2.1 *Selecting Problems Suitable for Research*

Below is a list of research 'problems' some of my current students/clients are working on and how/why these problems were selected.

1. **A large percentage of non-recyclable materials in household recycle bins** – problem identified and researched by a frustrated council officer in charge of waste management who was undertaking a higher degree.
2. **Decision making in a health promotion centre without any evidence base** – problem identified by the new centre director who was unsure how to prioritize issues.
3. **Violence towards nursing staff in emergency wards** – problem identified by an ex-nurse undertaking an occupational health and safety postgraduate degree after being forced into a career change due to a patient attack.
4. **Bastardization in university residential halls** – problem identified by a student who went through such practices in her first year at university.

(Continued)

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Box 2.1 (Continued)

5. **Subcontractors in the construction industry with poor safety records** – problem selected by an occupational health and safety student because of current media coverage related to the topic.
6. **Underutilization of experiential learning in the classroom** – problem identified by an education student through literature she came across in the course of her degree.
7. **Disregard for fire alarms in Hong Kong high rises** – problem identified by fire safety officer undertaking a higher degree who was in charge of an investigation where seven people died because they ignored an alarm.

UNPACKING PROBLEMS

Running alongside the task of problem identification is the need to 'unpack' problem situations. Remember, problems generally have a complex, multi-faceted and sometimes inconsistent nature. So the challenge for any researcher attempting to select a relevant problem is to be able to explore the assumptions embedded within problem definition.

In fact, before any problem is approached through research it is essential that researchers critically explore the assumptions that underpin the nature of the problem at hand. They also need to examine how they as researchers have come to understand a particular problem situation.

As shown in Figure 2.1, and further explored in Box 2.2, this 'unpacking' should involve: exploring the dominant worldview; exploring your own perspectives; and exploring the range of perspectives held by various stakeholders.

Exploring the dominant worldview

Whether it be the broader cultural milieu or the dominant culture that exists within a workplace, we tend to be immersed in settings that are not value-neutral. For example, the Western world, and many of the organizations within it, tends to be dominated by values that emerge from the legacy of Christianity, patriarchy and capitalism. And often situations that come to be defined as problems are the ones that those in power see as bumping up against these dominant worldviews. For example, increases in the percentage of teenagers having sexual intercourse, increases in divorce rates, or falling profits.

Okay, so how does this impact on those wanting to conduct research into real-world problems? Well, there are several ways. First, exploring the dominant

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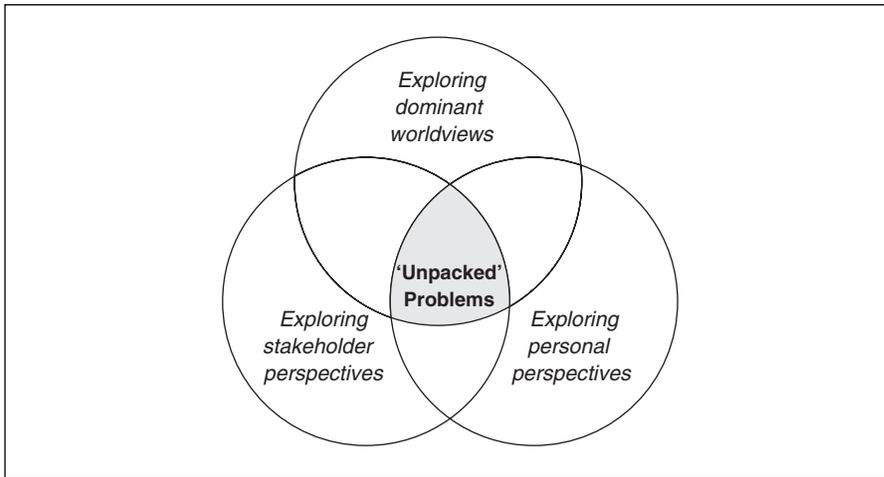


FIGURE 2.1 OPENING UP ASSUMPTIONS

worldview will allow you to understand issue prioritization so that you can work effectively within a system. If your research interests fall neatly within the dominant paradigm, getting support and even funding for your research may not be too difficult. Your problem is likely to be a priority to those in power.

On the other hand, if your 'problem' sits outside the dominant framework you may have to do some work to get the support you need. Now keep in mind that anyone (even those without a strong power base) can define a real-world problem – the trick is getting support to enable you to conduct research related to that problem. And this is where understanding the view of the dominant, the view of those with power, is quite important. For example, take the issue of workplace stress. Getting stressed at work is a common phenomenon – and one that is often ignored or simply not prioritized by management. Now for a big corporation the worldview is capitalism and the bottom line is profit, so priority problems will tend to be those that directly effect profit lines. Support for the conduct of research into the problem of workplace stress may therefore rely on arguments that link stress to more than just employee happiness and satisfaction. It is more likely to rely on arguments that highlight a loss of productivity, high turnover and decreased profits. In this scenario, understanding the dominant worldview can help you effectively advocate for research into 'problems' not generally given priority status by those in power.

Finally, being able to 'name' the dominant paradigm can be help you identify problems that often face the 'marginalized'. For example, in education, a dominant 'modern' worldview that incorporates a very hierarchical power structure might suggest that problems sit with unmotivated, uninterested, disrespectful students. If, however, you were to recognize that the education system you are exploring has such a paradigm in operation, you may begin to think about the paradigm

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itself as problematic. The problem may lie with an education system that acts to in fact create students who can be readily classified as unmotivated, uninterested and/or disrespectful. In this case, understanding the dominant paradigm allows you to do more than just work effectively within it. It also allows you to think critically about the paradigm itself and its implications for those subjected to it. The ability to think outside the square and question the system can provide an invaluable contribution for shifting organizational or societal cultures in radical ways.

Exploring your own perspective

'Few people are capable of expressing ... opinions which differ from the prejudices of their social environment. Most people are even incapable of forming such opinions.'

- Albert Einstein

Just because you take on a researcher or practitioner-researcher role, you won't suddenly become immune to the cultural forces that shape and surround you. We tend to make sense of the world through the rules we are given to interpret it. But when you are immersed in these rules and surrounded by them, they can, in fact, be very hard to see. Our sense of loyalty, our understandings of family, our belief in justice and equity, for example, are embedded within us and are reinforced on a daily basis. They become part of how we understand and make sense of the world ... and how we might go about researching it. It is, therefore, crucial to remember that being able to critically unpack a problem relies on a level of self-reflection.

In order to be a reflective researcher you need to:

- Consider your own worldviews, beliefs, biases, prejudices and subjectivities as they relate to a particular problem situation. The ability to 'name' your own positioning is a prerequisite for being able to unpack any problem situation. In fact, stating your political/personal positioning in relation to your research topic is now a common, and in some genres, expected part of a research thesis.
- Accept that others may not think or process the world as you do. There are a million different ways to view the world. It's important to remember that your particular understanding is not a universal understanding.
- Be sensitive to issues of race, class, culture and gender – these fundamental constructs can be associated with wide divergence in how situations can come to be framed as 'problems'.

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Exploring the range of perspectives held by various stakeholders

Problems are generally complex, and as highlighted in Box 2.2, unpacking problem situations demands that consideration be given to all the varied perspectives that can come from those with vested interests. Now there are two points to consider when looking at stakeholder perspectives. First is that there are likely to be a range of stakeholders involved in, or impacted by, any particular problem situation. Second is that there will not always be a common perspective within a particular stakeholder group. Keep in mind that full understanding will rely on hearing an array of voices. If you only listen to those talking the loudest, you will surely fall short of rich understanding.

Box 2.2 Weighing Up the Problem – Prezlee's Story

Being a new mother I was interested in knowing why pregnant women must suffer the humiliation of being weighed by a nurse during a prenatal visit rather than being able to weigh themselves. It seemed simple enough and important enough – disempowerment is certainly a critical issue in the medical field. But then my supervisor asked me to think about the following questions:

Why is this a problem? I could answer this one and proceeded to talk about the whole disempowerment of women/patients issue.

Is it a problem for you? Easy question – definitely, and not just for me, but also for my friends that had gone through a similar experience.

For all pregnant women? Uh oh – when I thought about this I realized I really had no idea if this was a problem to women from different cultures or a different class structure ... I guessed it was, but I really had no idea – my first assumption uncovered.

For doctors, nurses, administration? Again, when I then sat down to think about the issue from the perspective of doctors, nurses and administration I realized that from their perspective it may actually be a practice that affords them benefits such as consistency, control and empowerment Hmmm ... I'd have to explore that.

(Continued)

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Box 2.1 (Continued)

What dominant paradigms are in operation here? I enjoyed mulling over this one. Power structures of the medical profession, patriarchy, class and gender discrimination – I thought my question was a simple one – I didn't realize the issue could be so rich.

What prejudices, biases and preconceived notions do you have? I could think of four: (1) that being weighed in is humiliating; (2) that the medical profession is disempowering to patients; (3) that this needs to shift; and (4) that I sometimes assume that everyone else thinks just like me.

How will the answers to these questions influence your work? I realized that if I hadn't thought these issues through I would have probably found exactly what I was looking for because I would have only been ready to hear what I already knew, probably from pretty like-minded people. After thinking through my answers I realized I'd have to go back to the drawing board and really explore what it is I know about this issue, what I assume, and what complexities I will need to work through.

In the end I did end up exploring this particular issue – but after running through the questions above – I certainly went into my study with a much richer understanding of my 'simple' problem.

FROM PROBLEMS TO RESEARCH QUESTIONS

Okay – so you've had a good think about various problems you might want to explore and you've chosen a practical problem by drawing on your insights and experiences, assessing needs and perhaps even delving into a bit of literature. You've also taken the time to reflexively consider the nature of this problem, its relation to various paradigms and your own biases and subjectivities. You've even attempted to appreciate the problem from the viewpoint of various stakeholders. It must be time to start researching.

Well not quite. There is just one step to go, and that is moving from a problem suitable to research to a **research question**. Now you may ask, 'Is this really necessary', and the answer is an unequivocal 'YES'. You might be surprised at just how many options there are in moving from a problem to a question, and I am a firm believer in the importance of a well-developed research question. It is an absolutely essential starting point for the research journey.

Real Problems to Researchable Questions

The nature and importance of research questions

There are a lot of people who feel ready to jump into a research project before they have taken the time to really think through and develop a research question. Research questions are, however, fundamental and the ability to articulate ideas into well-formed research questions is an essential skill.

So why is it that research questions are so crucial? Well, it's because, when you get right down to it, to conduct research is to embark on a decision making journey. The process, in fact, demands that you constantly engage in decision making that is logical, consistent and coherent. And what do you think the benchmark for logical, consistent and coherent decision making is? It's that the choices you make will take you one step closer to being able to credibly answer your ... research question. So without clear articulation of your question – you're really travelling blind.

Put simply, research questions:

- *Define an investigation* – a well-articulated research question can provide both you and your eventual readers information about: your topic; your context; your aims and objectives; the nature of your question; potential variables; and relationships that might exist between these variables.
- *Set boundaries* – along your research journey you are likely to find yourself facing plenty of detours and diversions. A well-defined question can remind you that your research project needs to have boundaries.
- *Provide direction* – it's worth remembering that if you don't know what you want to know, you will not be in a position to know how to find it out. A well-defined, well-articulated research question will provide direction and point to: the theory you need to explore; the literature you need to review; the data you need to gather; and the methods you need to call on.
- *Act as a frame of reference for assessing your work* – not only does your question provide continuity and set the agenda for your entire study, it also acts as a benchmark for assessing your decision-making processes. As stated, the criteria for all decision-making related to the conduct of your research will be whether or not choices lead you closer to credible answers to your research question.

Developing the question

Hopefully you are now convinced that research questions are indeed pivotal to the research process. But how exactly do you go about articulating one? Well, if you have thought it all through and feel comfortable with the problem situation you want to explore, but you are not quite sure how to best articulate your research question, I'd suggest working through the following five-step process:

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1. Using only short one- or two-word responses, write down the answers to the following questions:
 - What is your topic? i.e. chronic back pain, recycling, independent learning ...
 - What is the context for your research? i.e. a school, local government authority, a hospital, community ...
 - What do you want to achieve? i.e. to discover, to describe, to change, to explore, to explain, to develop, to understand ...
 - What is the nature of your question? i.e. a what, who, where, how, when, or why question.
 - Are there any potential relationships you want to explore? i.e. impacts, increases, decreases, relationships, correlations, causes etc.

2. Starting with the nature of the question, that is, who, what, where, how, when, begin to piece together the answers generated in step 1 until you feel comfortable with the eventual question or questions. For example, say your problem was a large percentage of non-recyclable materials in household recycle bins (as discussed in Box 2.1). The answers from step 1 might lead to a number of questions:
 - **Topic:** recycling; **Context:** domestic/community; **Goal:** to explore why there is a lack of efficiency; **Nature of your question:** who and why; **Relationship:** correlation between demographic characteristics and inefficient recycling
Question: Is there a relationship between household recycling behaviours and demographic characteristics?
 - **Topic:** recycling; **Context:** domestic/households; **Goal:** to understand how individuals go about the task of recycling; **Nature of your question:** how; **Relationship:** N/A
Question: How do individuals engage in decision making processes related to household domestic waste management?
 - **Topic:** recycling; **Context:** domestic/community; **Goal:** to describe the nature of recycling inefficiencies so that an effective community awareness campaign can be developed; **Nature of your question:** what; **Relationship:** N/A.
Question: What are the most common non-recyclable items found in household recycle bins?

3. If you have developed more than one question (remember any one problem can lead to a multitude of research questions), decide whether you need to select one or more questions and make that selection.
4. Narrow and clarify until your question is as concise and well articulated as possible. Ambiguity can often arise when questions are broad and unwieldy, so being precise makes the research task easier to accomplish. Remember, the

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first articulation of any research question is unlikely to be as clear, helpful and unambiguous as the third, fourth or even fifth attempt.

5. Assess the question(s) in relation to the question checklist that follows.

The real-world research question checklist

Once you come up with a research question, you need to assess if it's going to be researchable at a practical level. Run through the following checklist. If you find yourself feeling uncomfortable with the answers, it may indicate a need to rethink your question.

- ☑ **Is the question right for you?** There's a bit of a double-edged sword here. On one side you need to consider whether your question has the potential to hold your interest for the likely duration of the study. You don't want to take something on that you don't have the motivation to see through. On the other side, however, you need to consider whether interest or perhaps passion over a particular issue will threaten the credibility of your study. Are you biased? And can you control your biases so that your study results will stand up to scrutiny?
- ☑ **Does the question have significance for an organization, an institution, a group, a field, etc.?** You need to consider whether relevant stakeholders will regard your findings as significant. Remember, the role of research is to do one or more of the following: advance knowledge; aid individuals; improve professional practice; and/or impact programmes and policy. Research questions need to be significant – not only to you, but to a wider group of stakeholders as well. If the response from parties who should be interested is, 'so what/who cares' – you need to go back to the drawing board.
- ☑ **Can it lead to tangible situation improvement?** A distinguishing feature of researching real-world problems is that the problems you will attempt to research are, in fact, real – and there is a genuine motivation to fix or alleviate problems and improve situations. A key criterion for your research question is thus assessing whether your findings are likely to stay in the realm of theory and do little more than sit on a shelf, or whether your results will actually be useful for enacting change.
- ☑ **Is the question well articulated?** A research question not only indicates the theory and literature you need to explore and review, it also points to the data you will need to gather, and the methods you will need to adopt. This makes clear articulation of research questions particularly important. The question needs to be as unambiguous and clearly defined as possible.

Take the question, 'Is healthcare a problem in the US?' As a question for general debate, it's probably fine. As a research question, however, it needs a fair bit of clarification. How are you defining healthcare? What boundaries are you putting on the term? How are you defining problem? Social, moral, economic, legal, all of the above? And who are you speaking for? A problem

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for whom? The more clarity in the question, the more work the question can do, making the direction of the study that much more defined.

- ☑ **Is the question researchable?** Perhaps the main criterion of any good research question is that you will be able to undertake the research necessary to answer the question. And there are a number of constraints to doing this.

First, you have to assess whether the question can be answered through a research process. For example, the question, 'Would the national education system work better if only women were allowed to teach?' This can lead to speculation, but unless you fire all male teachers and see what happens, you will not be able to come up with any definitive answers to this particular question.

Now even if your question is 'researchable' in theory, you also need to consider if you will be constrained by time, funding, expertise and ethical clearance. Making sure your question is feasible and that it can lead to a completed project is worth doing early. Nothing is worse than realizing your project is not 'doable' after investing a large amount of time and energy.

- ☑ **Does the question have a level of political support?** Research into real-world problems almost always occurs within a political context. It might be a government body, a business, a community etc., but common to all of these settings is that political agendas will often direct which projects get off the ground. Therefore, when it comes to finalizing your question, it makes a tremendous amount of sense to assess that question in relation to the current political landscape. Funding, dedicated workload and access can all be dependent on your ability to be political.

Changing focus

So you now have the perfect research question; it meets all the criteria you feel and you're ready to go. Let's set it in stone. Well maybe not – research questions can, and often do, change, shift and evolve during the early stages of a project; and not only is this fine, it is actually appropriate as your engagement in the research process evolves both your knowledge and thinking. Yes, developing a clear question is essential for direction setting, but it is important to remember that the research journey is rarely linear. It is a process that generates as many questions as it answers, and is bound to take you in unexpected directions.

As you get started on your research, you may come across any number of factors that can lead you to: query your aims and objectives; see you modify your question; add questions; or even find new questions. The challenge is assessing whether these factors are sending you off the track, or whether they represent developments and refinements that are positive for your work.

A note on hypotheses

Before concluding this chapter, I want to briefly discuss the role of, and need for, a research hypothesis. Now a hypothesis is basically a logical conjecture (hunch

Real Problems to Researchable Questions

or educated guess) about the nature of relationships between two or more variables expressed in the form of a testable statement.

The role of a hypothesis is to take your research question a step further by offering a clear and concise statement of what you think you will find in relation to your variables, and what you are going to test. It is a tentative proposition that is subject to verification through subsequent investigation.

For example, let's consider the question 'Is there a relationship between household recycling behaviours and demographic characteristics?' Your hunch is that age has a large impact on recycling behaviour – basically, you suspect that young people put anything in the recycle bin. Here you have all the factors needed for a hypothesis: logical conjecture (your hunch); variables (recycling behaviours and age); and a relationship that can be tested (recycling behaviours *depend* on age). It is therefore a perfect question for a hypothesis – maybe something like 'children and teenagers are more likely than adults to put inappropriate materials in recycle bins'.

Basically, if you have a clearly defined research question – and you've got variables to explore – and you have a hunch about the relationship between those variables that can be tested, then a hypothesis is quite easy to formulate.

Now not all research questions will lend themselves to hypothesis development. For example, take the question 'How do individuals engage in decision making processes related to household domestic waste management?' Now remember that a hypothesis is designed to express 'relationships between variables'. This question, however, does not aim to look at variables and their relationships. The goal of this question is to uncover and describe a process, so a hypothesis would not be appropriate.

Generally, a hypothesis will *not* be appropriate if:

- *You do not have a hunch or educated guess about a particular situation* – your goal may be to build broad understandings.
- *You do not have a set of defined variables* – your goal might be to simply identify relevant variables.
- *Your question aims to explore the 'experience' of some phenomena* – for example, what is it like to start school with English as a second language.
- *Your question centres on developing rich understandings of a group* – for example, what it means to be a cancer survivor.
- *Your aim is to engage in, and research, the process of collaborative change* – in 'action research', methodology is both collaborative and emergent, making predetermined hypotheses impractical.

In short, whether a hypothesis is appropriate for your question depends on the nature of your inquiry. If your question boils down to a 'relationship between variables', then a hypothesis can clarify your study to an extent even beyond a well-defined research question. If your question, however, does not explore such a relationship, then force fitting a hypothesis simply won't work.

Laying Foundations

Chapter Summary

- Defining the word problem is different from understanding what constitutes or qualifies as a problem. Problems are not universal and are dependent on worldviews, perspectives, history and culture.
- Identifying problems suitable for research involves looking for problems that can be addressed through the research process. Insights can come from your own knowledge and experience, the exploration of broader issues, and learning to identify the needs of others.
- Problems are generally complex and multi-faceted – and therefore need to be explored so that embedded assumptions can be uncovered. This process involves exploring: dominant worldviews; personal subjectivities; and stakeholder perspectives.
- Developing a well-articulated research question is an important part of the process because it defines the investigation; sets boundaries; provides direction; and acts as a frame of reference for assessing your work.
- The process of question development involves: working through your topic, context, goals, nature of the inquiry, and potential relationships; articulating those components into a relevant question(s); and narrowing and clarifying until the question is as concise and unambiguous as possible.
- In order to assess your question you will need to explore whether your question: is right for you; will be of broad significance; can lead to tangible situation improvement; is well articulated; is researchable; and will have political support.
- Redefining your questions is a normal part of the research process. Forming the right 'questions' should be seen as an iterative process that is informed by reading and doing at all stages.
- Hypotheses are designed to express relationships between variables. If this is the nature of your question, a hypothesis can add to your research. If your question is more descriptive or explorative, generating a hypothesis may not be appropriate.