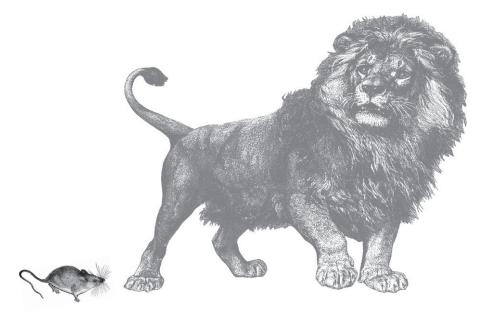# WORKPLACE RESEARCH

WORKPLACE RESEARCH

Conducting small-scale research
in organizations

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3

DEVELOPING PRACTICABLE AND VALUABLE RESEARCH QUESTIONS

Chapter preview

Issues before questions

Deciding on appropriate evidence-based questions

Getting the research question on target

Pitching your research question

ISSUES BEFORE QUESTIONS

When you are attempting to develop a practical and valuable research question, it pays to first have a look at what types of issues organizations face, and where you might be able to contribute.

Organizations tackle a host of issues – how to allocate resources most effectively, how to build a successful future and how to prioritize among competing demands and challenges. The good news is that issues are everywhere. Indeed, just about anyone who has ever had a job will tell you that workplaces are full of them. For example, what employee doesn't grapple with red tape, inefficiencies, ineptitude, decision-makers not in touch with what's happening on the ground, profit before service, morale and motivation? These are not limited to large multi-national corporations, but are endemic in any complex organization. Even academia is not immune! 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 issues 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 unsatisfied

customers, communities reluctant to recycle waste, or patients trying to follow dietary guidelines. If these frustrations are widely accepted and can be linked to organizational/community goals, then they are likely to have research potential.

Organizational problems and opportunities

If we get down to it, issues are often problems, but they can be opportunities. Both are rich playgrounds for developing research questions.

Issues as problems

A world without problems … sounds good doesn't it? But if you really think about it, a world without problems would also be a world with no motivation to transform, progress or evolve. It would be stagnant. Surely there would be no need to 'progress' if there were no problems, but perhaps that simply indicates an ambivalent attitude towards change and a framing of the word 'problems' that is less than constructive.

Precisely because (1) there are so many challenges out there, (2) there is a true dedication to problem solving from global to local scales and (3) research is recognized as central for effective and informed decision-making, there is a real call for 'applied' research, or research expressly designed to contribute to solving practical problems. How then do you hone in on a problem conducive to a small-scale workplace-based research project?

Generally speaking, small-scale applied workplace-based research will see you engaged in problems, or aspects of problems, that while still important and significant, are local, grounded and practical. This research responds to real and tangible everyday needs. So whether it is in local or national government, non-government organizations, aid agencies, communities, corporations or, in fact, any workplace, if there are problems to tackle, then there is a need for research that can aid problem resolution.

Perhaps it would be helpful to conceive of problems as a gap between what is real and what is ideal or expected. Of course, 'ideal' is subjective and a matter of some judgment. You might consider some problems as the gap between organizational outcomes and the organizational mission. Where does the organization or department fall short of its ideals and broader goals? What are some of the causes and how can they be tackled? And once there is a design to tackle them, is it a good one?

One of the most woeful research gaps is one on the efficacy of existing solutions to address solutions. As Kurt Vonnegut, Jr wrote, 'A flaw in the human character is that everybody wants to build and nobody wants to do maintenance' (2000: 198).

Finally, broader societal/political problems can illuminate the workplace. For example, timely or contemporary issues, such as an ageing workforce, which intersect with organizational goals are good issues to explore. 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.

Issues as opportunities

> We are continually faced with a series of great opportunities disguised as insoluble problems.

<div align="right">John W. Gardner</div>

Workplace-based research is not only about problems, but also about opportunities. Yes, we are drawn to problems, for it is problems that motivate change. Perhaps it would be more constructive 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.

Opportunities abound in organizations, for there is always something that can be improved. Your research question focuses on how to make things better, EVEN if they are fine at the moment. Opportunities, for instance, could include researching how to:

- increase efficiency (processes, systems, etc.);
- improve safety;
- improve sales or marketing of a particular product or service;
- improve training for new recruits;
- boost wellness at work;
- expand the reach of the organization or its mission;
- utilize smarter technology.

These opportunities may be perfect for small-scale workplace-based research projects. Process improvement can produce rapid and visible results with even small tweaks. When done well, updating a training program or improving a tool can save countless hours or dollars.

Every day in organizations we are surrounded by events, situations and interactions that make us wonder, stop, think or become frustrated. If you see something that could be done a better way, perhaps this could be the research opportunity you've been looking for. For an example, see Box 3.1.

BOX 3.1

Seeing opportunities: Keira's story

An experience in my first week with the company helped me select my research topic. There were three 'newbies' in the same department, but we'd showed up right as the team was finishing up a big client project. Apparently someone had volunteered to do our orientation, but had gotten called away and everyone else was busy. Someone set us up in

<div align="right">*(Continued)*</div>

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a separate room with a pile of HR documents including the company policy handbook and a bunch of confidentiality forms and told us to look through them. And then they kind of forgot about us for about a week. Some of us got through the material quickly then just sat around and played online. It was pretty boring and wholly unproductive.

For my research question, I thought about how the intern on-boarding process could be improved to be more engaging and efficient. Clearly, depending on an employee to take time out of their week to voluntarily shepherd us was not ideal, but nor does the company really want to waste a week's worth of labour when new workers could be contributing.

As a result of my research, the company changed its orientation process! Instead of dividing interns up by department, the new process involved all of the interns together across multiple departments. I was tasked with producing a new orientation booklet, highlighting relevant policy and guidelines. I also created quizzes to test knowledge and designed a group activity for confidentiality/ethics training. Another idea I had was to collate a list of tips from previous interns. Since some of the previous interns had gone on to join the company as employees, I arranged for one of them come in to answer questions. Best of all, the new process only took 2 days instead of a week. The company saved time and money by getting their new hires on board quickly and energetically. This was more than a year ago, but I hear the program is still being used!

Perhaps you're thinking, 'Process improvement? I don't even understand the existing processes, like operating the printer!'. First, this is perfectly normal. Everyone hates the printer; in the film *Office Space* the employees celebrate their last day at work by destroying one. It is normal to doubt your ability to impact change at the beginning of your placement, but you'd be surprised how quickly you can adapt and contribute. At first, students tend to stick rigidly to the templates in their organization and double-check everything with their manager. But by the third week or so, students begin to see things slightly differently and offer suggestions about how things can be improved. When processes ossify over time, it sometimes takes just a single individual (like you) to ask 'Why?'. By critically examining processes, reports and tools, you may able to identify opportunities for improvement and, therefore, opportunities for research.

Identifying needs of the organization

A firm's basis of competition … has changed from tangible products to intangible information.

Kuan-Tsae Huang, Yang W. Lee and Richard Y. Wang

Whether you are looking at problems or opportunities, it's really all about *need*. Your workplace-based research project should take into consideration the needs of the organization (or by extension its customers, clients and constituents).

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 opportunities might also come from annual reports, media coverage, or listening to stakeholders at various forums including workplace meetings or customer forums. In short, listening to, and identifying, the needs of stakeholders is paramount.

A good strategy here is to undertake a quick 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 the organization needed to close a lead smelter operating near a residential area due to community protests, stakeholders would include representatives from the parent company and the smelter, employees and local residents, as well as 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.

The same could be said of opportunities. Say, for example, that the organization wanted to launch a new product. Your stakeholder analysis would include potential consumers, product developers and testers, consumer groups, management, retailers and, depending on the product, regulators. If the product is a child's educational toy, your stakeholder analysis would also include children and their parents, educators or developmental psychologists and consumer groups.

A second strategy to hone in on organizational needs is to examine urgent and important issues.

Identifying urgent vs important issues

What happens when organizations (or individuals) are faced with multiple problems at once? Usually the noisiest problem is dealt with first, while the quieter strategic issues get sidelined. You've probably experienced this. We assure ourselves that important issues in our life will receive due attention 'after this next project is finished', or 'as soon as exams/tax season/the holidays are over'. Companies do the same. Managers even commandeer technological jargon into rationalizing these choices: 'Our team just doesn't have the bandwidth right now.'

In his best-selling time-management handbook *The 7 Habits of Highly Effective People*, Stephen Covey (1989) explains why in our professional and personal lives

we tend to prioritize the urgent over the important. Urgent matters are those that solicit and demand our immediate reaction and, certainly, these are appropriate for small-scale applied workplace-based research. But important issues are ones that deal with long-term value – they contribute to the growth, value and mission of the organization and/or individual and are also worth research consideration.

Now by their very nature, urgent issues are aggressively pressing, highly visible and even annoyingly auditory. In short, they are a 'ringing telephone'. Ringing phones are disruptive and hard to ignore, even if you are in the middle of something important. Covey's metaphor is even more apt now that we carry our phones with us literally everywhere we go.

The ringing phone can compel people to disrupt an interview or first date, ignore their partner on holiday, or even enrage a whole audience of movie-goers. There is a reason that film previews now have an announcement to turn off your phone rather than put it on vibrate or silent. If we realize it is ringing, we are drawn to it like a moth to a flame. We've even heard of a wedding where the minister answered his phone in the middle of performing the ceremony!

It appears to be impossible for some people to overcome the allure of the ringing phone, buzzing text message or (ding!) email notification. It is the same with urgent activities. At work, the ringing phone is combined with the bottom-less email inbox, a veritable to-do list of the trivial and the impossible as others seek to prioritize our time for us. No one ever got an award for keeping their email inbox clear, much less a promotion. Rather, successful individuals (and organizations) find a way to balance priorities between urgent issues and important long-term issues.

To identify organizational needs, and consequently research questions, it may be worth asking what is not only urgent, but also important. You could be ideally placed to pick up that important issue or initiative that has been languishing on the shelf and move it a few metres down the field. What might some of these important issues look like? They are usually masquerading as unmade decisions. What decisions are the organizations grappling with? Where are the concerns, complaints or pressure from various stakeholder groups that have kept this deci-sion from being made? How can you contribute valuable information to assist with a resolution?

As you are undertaking a small-scale workplace-based research project, you may end up answering only one of these questions through a rigorous research process. But that is okay. Remember small-scale workplace-based research is about gathering data for evidence-based decision-making, so a rigorous contribution, even if offering only one stream of data, can be worthwhile.

Where can you contribute?

Imagination is more important than knowledge.

Albert Einstein

Whether addressing problems or taking advantage of opportunities, your job is to translate an organizational issue into a research project. So, how do you go about doing this? By knowing what is important, what's needed and how data can facilitate a solution. And by keeping in mind how your project can facilitate decision-making and improvements. In Chapter 1, we discussed how evidence-based decision-making can contribute to better policy and outcomes not just for organizations, but also for communities and individuals by:

- highlighting the urgency/importance of an issue and thus helping to secure funding and resources for a solution;
- updating policies, programs and actions so that they respond to needs, and by not creating unintended negative consequences of their own (see Box 3.2);
- enabling information-sharing among key groups within the organization, and between the organization and others (such as clients, customer groups, constituencies, etc.), which can enhance decision-making.

Findings, results and conclusions can lead to practical recommendations, genuine change, great opportunities and real problem-solving. Organizations need your insights. Yes, you! If you are just joining a new organization, you can provide the fresh eyes to see problems and opportunities in a new light. If you have been a part of the organization for a while, you may be well placed to connect the dots between problems spread across departments. Either way, you can add rigor and precision to the process of analyzing organizational needs. Your project could be the first step in a new direction, or the last piece of the puzzle that needs solving. Perhaps your small-scale applied workplace-based research project will have broader ramifications down the road that you might not even be aware of.

BOX 3.2

The Law of Unintended Consequences

Many a product, policy, program or initiative has sought to remedy a problem, only to create another one entirely. This is known as the Law of Unintended Consequences. For instance, in 1990, the Australian state of Victoria made safety helmets mandatory for all bicycle riders. The result was that the number of cyclists actually decreased, a counter-productive outcome in terms of health. Moreover, through a phenomenon known as 'risk compensation', serious injuries for the remaining cyclists actually increased as the helmet encouraged more risky behavior!

Understanding how unintended consequences happen can provide an avenue for research enquiry as you seek to modify policies or create new ones.

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There are three types of unintended consequences:

- An *unexpected positive* outcome: Surprise! A drug developed to regulate X is also found to cure Y. Assuming there are more consumers with the Y problem, this could mean a surreptitious profit line. For instance, Listerine was sold as a surgical antiseptic for 40 years before it was found to be a magical cure for bad breath. Viagra, the erectile dysfunction drug, was actually developed to treat high blood pressure and heart disease. During the original clinical trial, the um, side effect was noted and then swiftly marketed. The drug now makes an estimated $1.9 billion dollars annually.
- An *unexpected negative* outcome: Oops! Perhaps the problem has been overcome, but the solution has caused a few ill effects of its own. Sticking with the pharmaceutical theme, a drug designed to help those suffering from depression may also cause side effects (listed in the smallest possible type at the bottom of the advertisement), including drowsiness, itchiness, sensitivity to sunlight, inability to control speech volume and explosive diarrhea. Sounds like you've traded one problem for many others. When these side effects are worse than the disease, we've arrived at the third type of unintended consequences.
- A perverse outcome: #$%! The aim has not been achieved, and the solution has compounded the problem, or spawned others far worse. In 2013 it was revealed the National Security Agency had compelled American technology manufacturers such as Apple and Cisco to create 'backdoors' in their products that the government could use to break encryption (*The Economist*, 2015). The goal was to keep Americans safe by foiling terrorist plots. Over several years of this practice, the agency did not stop a single terrorist attack, but it did endanger millions of citizens by creating a systemic vulnerability that has since been exploited by hackers and criminals.

The research potential across all three of these consequences is immense.

The important thing is to recognize that you can contribute something valuable. You don't need to be an expert, but you do need to develop a clear and well-defined research question (more on that in a bit). Once you have that, the second thing you need is confidence. Research shows that confidence is just as important as competence for success (see Box 3.3).

BOX 3.3

Speak up! Clare's story

Though I had a good idea for a research project, I kept putting off discussing it with my manager. I'm a perfectionist and I worried that the project idea wasn't yet ready, and

anyway my boss was very busy – directing six new employees, running a large department and shepherding several large projects to an upcoming deadline. 'He doesn't have time for this right now,' I rationalized. 'I don't want to be a bother.' I then watched six weeks sail past! Disappointed that I wasn't really contributing full value to the organization, I finally decided to write my boss a brief email with my idea, and the types of data and reports I would need access to. I fully expected to be dismissed as 'demanding' but to my relief and surprise, I received an enthusiastic reply shortly thereafter. In the subsequent meeting with my boss I received access to the data that I needed to finish the project, and congratulations from my boss on my idea. My advice? Have confidence. Speak up!

Be confident in your intelligence, your problem-solving skills and your ability to rise to the challenge. This is especially true if you are new to the environment. If you are particularly green (see Chapter 2), it is natural to feel a little out of your depth at first. Just remember that every competent and confident professional you see started out exactly where you are.

Just because you are new to research doesn't mean you aren't capable of doing it. Sure it would be easy to say 'Um, actually, I've never done that, I'm really not sure …', and politely suggest a more experienced individual, but why not give it a go! After all, you learn by doing. Project proposals can be reviewed by a manager and revised before they are final. The organization will likely not fold if your research is not PhD level. Don't conflate inexperience with incapability. We are talking about small-scale applied workplace-based research; if something is carefully considered and appropriately managed, it is well within your scope.

DECIDING ON APPROPRIATE EVIDENCE-BASED QUESTIONS

Now you may be thinking, 'I have a pretty good idea about what I want to research. Is working on my actual question so important?'. Well, the answer is an unequivocal 'Yes'. Some people want to jump right into their research project without taking the time to really think through and develop their research question. Some have ideas about their topic, but they are not clear on the aspects they want to explore. Others will have their ideas pretty much narrowed down, but have not clearly articulated this in a researchable question. We are real sticklers for good research questions. Why? Because they are absolutely fundamental to good research; and your ability to articulate one is essential. After all, how will you know when you have found the answer to your question if you can't say what your question is?

Remember: research is 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 is the benchmark for logical, consistent and coherent decision-making? It's that the choices you make take you one step closer

to being able to answer your research question with credibility. So, without clear articulation of your question, you are really traveling blind.

The value of research questions

Research questions are essential because they:

- *Define an investigation* – A well-articulated research question can provide both you and your eventual readers with information about your project. It can: (1) define the topic; define the nature of the research endeavor – to discover, explore, explain, describe or compare; (2) define the questions you are interested in – what, where, how, when, why; (3) define your constructs and variables – income, age, education, gender, etc.; and (4) indicate whether you foresee a relationship between variables – impacts, increases, decreases, relationships, correlations, causes, etc.
- *Set boundaries* – Along your research journey you are likely to find yourself facing plenty of tangents, detours and diversions; and a well-defined question can help you set boundaries. When faced with an interesting tangent, ask yourself: 'What does this have to do with my question?' There are more potential answers to this question: (1) actually nothing – I will have to leave it and maybe pick it up in my next project; (2) actually it is quite relevant – if you think about it, it really does relate to … (this can be exciting and add new dimensions to your work); and (3) well nothing really, but I actually think this is at the heart of what I want to know – perhaps I need to rethink my question.
- *Provide direction* – A well-defined, well-articulated research question will act as a blueprint for your project. It will point you towards the topic you need to explore, the information you need to review, the data you need to gather and the methods you need to call on. If you do not know what you want to know, you will not be in a position to know how to find it out.
- *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, but it also acts as a benchmark for assessing decision-making. The criteria for all decisions related to your project will be whether or not choices lead you closer to credible answers to your research question.

A research question is therefore crucial to putting you on the right path for your project. But does your research question need to be a simple enquiry or must it be framed as a hypothesis?

Do you need a hypothesis?

A hypothesis is basically a logical conjecture (hunch 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 office recycling habits and demographic characteristics?'. Your hunch is that age has a large impact on recycling behavior – 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 behaviors and age); and a relationship that can be tested (recycling behaviors depend on age). It is therefore a perfect question for a hypothesis – maybe something like 'Millennials are more likely than Gen X or Baby Boomers 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 office waste management?'. 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 use a new technology product with English as a second language;
- your question centers on developing rich understandings of a group – for example, what it means to be a female CEO;
- your aim is to engage in, and research, the process of collaborative change, making predetermined hypotheses impractical.

In short, whether a hypothesis is appropriate for your question depends on the nature of your enquiry. 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.

Defining the research topic

All this talk about the need for an appropriate research question is fine, but what if you're not sure what to pursue? Well you're not alone. Yes, there are

plenty of students who are quite clear about their topic early on, but there are also a lot of students who really struggle with the idea of generating a research topic. In fact, many feel that coming up with something worthy of research is beyond them. If you are struggling for a topic, try these three suggestions.

- *Listen* – When we are stressed, nervous, tired, or otherwise spending our entire cognitive budget on just keeping up, it can be difficult to notice anything that doesn't immediately impact us. Try to slow down and pay attention. What are the common themes of office conversation – is there a new project under way, or a long-standing problem that appears to be neglected? Could more information assist with resolving the issue? Could evidence communicated to decision-makers facilitate a resolution or improve practice?
- *Read the internal news* – Company newsletters, usually distributed through email, summarize current projects, initiatives or programs. One of them may catch your attention as a research topic, or at least identify the current priorities of the organization in which your data collection efforts could align.
- *Read/watch the external news* – Familiarize yourself with the news around your industry. Watch for how the industry or organization is portrayed in the media. Has part of their work become the focus of global attention? How might government policy or action impact on the company? By gaining a broad understanding of your organization and how it fits within the industry and broader issues you can start to connect the dots regarding what needs may be pressing or just around the corner.

For more ideas on selecting issues suitable for small-scale applied workplace-based research, see Box 3.4. If you're still having trouble, consider setting a deadline for yourself. You may find your mind works best under pressure.

BOX 3.4

Selecting issues suitable for small-scale applied workplace-based research

Below is a list of research topics that some of our students are working on and how/why these issues were selected:

- The cost-effectiveness of brand awareness strategies – selected by a marketing student in a non-profit organization.
- The inclusion of climate change risk as a factor in fire management planning – selected by a manager in the local fire station who recognized the need for currency in planning processes.

- Decision-making in a health promotion center without any evidence base – selected by the new center director who was unsure how to prioritize issues.
- Subcontractors in the construction industry with poor safety records – selected by an occupational health and safety student because of current media coverage related to the topic.
- Underutilization of experiential learning in the classroom – selected by an education student through the literature she came across in the course of her degree.
- Using decision-making frameworks to assess whether the company should invest in the Australian solar industry – selected by a dual science/business major who was placed in the investment arm of a major international research organization in China.
- Adapting the proposed National Disability Insurance Scheme – selected by a student working in a traveling outpatient health care service provider regarding new proposed legislation.
- Risk products to reflect recent changes in the financial industry regulations – selected by a student in the financial industry interested in money-laundering regulations.

Practical questions

As you explore possible research topics and construct a draft research question, make sure you consider practicalities. 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. For example, the threat of a major currency fluctuation is a real problem for your financial firm, 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 a particular manager who is a real work bully! 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.

No matter how interesting a topic appears, in the end your project must be 'doable'. Now doability is something we will talk about quite extensively when we look at the potential methods you might use to carry out your study, but even at the point of topic identification it is worth keeping practicalities like appropriateness, supervision and funding requirements in mind:

- *Appropriateness* – There are many students who come up with ideas that are not relevant to the organization they work within, nor relevant to their degree training. What if you're a marketing student who wanted to undertake a research project on financial risk modeling? This may be the time to sit down

and really think about your research, academic and career goals … and seek alignment.

- *Organizational support* – If an employer has given you a research area, you may not be able to shift topics. Even within a defined project, however, there can be scope to concentrate on particular aspects or bring a fresh perspective to an issue. Open negotiation and even a 'sales pitch' covering the relevance and potential benefits of your proposed research can give you more creative potential.
- *Academic supervision* – If your research project requires an academic component or is part of an academic program, remember that you may need to seek and secure supervisory support within your university. Finding out whether appropriate supervision for your topic is available before you lock yourself into a project is well advised.

As you select organizational problems and opportunities as research questions, consider the practicalities around the topic. The research question must be a good fit between you, the organization and the resources (including time!) available.

Politically relevant questions

In Chapter 1, we broached the requirement of political relevance. Your research question should be interesting, useful and relevant to the organization. It should speak to an organizational need and build on institutional support from managers, decision-makers and colleagues. Sounds simple, right?

But problems are amazingly complex things. What might appear straightforward at first glance can have a plethora of complexity hidden right below the surface. One level of complexity that you must remain sensitive to is the political landscape. The political landscape is a set of formal and informal hierarchies that describe power and influence within an organization. It's shaped by the size of the organization, whether resources are equitably distributed throughout it, and the personality of key leaders.

This landscape can be thought of like tectonic plates, generally stable but occasionally cataclysmic. You need to be aware of this political landscape and its shifts in the course of your research project. Say that you have started work in a department in which your supervisor has announced that cost-cutting is a priority. You notice some inefficiencies that could be rectified and save the company money so you decide to direct your research effort in that area. A short while later, your supervisor announces she is leaving the company. Should you tweak your research focus simply because the political landscape has changed? The answer may be 'Yes'. If the cost-cutting measure was a personal initiative of your supervisor (and not the broader department or organization) and he/she is the only one with access to the data you require to finish it, or if the successor has different priorities, then you may need to tweak your focus. Why? Because your question is now not as politically relevant.

To be politically relevant, the question should have key decision-makers on-board and even anticipating your results. This involves cultivating and leveraging individual relationships, which can sometimes change. Using the above example, say your supervisor is leaving the department because she has been promoted within the company. Do you still need to redirect your focus? Well, that's a different story. Your supervisor may likely still be very supportive of your project (and thus still able to provide you with resources you require). Moreover, as supervisors/department managers sometimes contribute to hiring their replacement, they may be able to pass along their support of your project to the new manager. If anything, your manager's promotion may make your work even more politically salient, as she is now closer to more powerful decision-makers. For an example of the political landscape in action, see James's story (Box 3.5).

BOX 3.5

A shifting landscape: James's story

I was just a few weeks into my placement when I noticed people in my department behaving strangely. Once open doors were now constantly closed, and colleagues talked in hushed whispers. My boss, the manager, was especially preoccupied, out of the office frequently and then on the phone the rest of the time. What was going on?

Eventually I was told that the manager was moving to another position in another company. The manager left the next day. I was surprised what an impact it made in the office. Apparently his departure was unexpected and it would take a while to find someone else. My research basically stopped. The department was too busy to deal with me and since I was so new, I felt my questions were more of a hindrance than a help.

I had made an effort to make contact with individuals in other departments as part of my research proposal and with their help I was transferred to another department. Looking back, it was the right thing to do. I was able to contribute in the new department without being a burden in a very stressful time.

GETTING THE RESEARCH QUESTION ON TARGET

While expansive questions can be the focus of good research for PhD-level work, there simply isn't time for ambiguous and unwieldy research questions in small-scale applied workplace-based research projects. Being precise makes the research task easier to accomplish within the allotted timeframe. A narrow scope tends to be the number one difference between getting it done on time or not getting the project done at all!

Narrowing the focus

Typically, research questions start out big and need to be wrangled into something manageable. The first step is to clarify your own thoughts through some structured analysis. For narrowing in, try using the four-step question generation process outlined below.

Four-step question generation process

1. Using only one- or two-word responses, write down the answers to the following questions:

   a) What is your topic? That is, client satisfaction, legislative requirements, profit, triple bottom line, etc.

   b) What is the context for your research? That is, a for-profit corporation, non-profit, government office, community body, local authority, etc.

   c) What do you want to achieve? That is, to discover, to describe, to change, to explore, to explain, to develop, to understand …

   d) What is the nature of your question? That is, a who, what, where, how, when, or why question.

   e) Are there any potential relationships you want to explore? That is, impacts, increases, decreases, relationships, correlations, causes …

2. Starting with the nature of the question, i.e. 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.

   a) Topic: promotions. Context: HR department of a large organization. Goal: to discover trends in promotion history. Nature of the question: who and what. Relationship: correlation between individuals who get promoted and the results of their previous evaluations.

   b) Question: What is the relationship between employees who get promoted and their evaluation assessments?

   c) Topic: promotions. Context: HR department of a large organization. Goal: to understand how promotion determinations are made. Nature of the question: how. Relationship: none.

   d) Question: How do managers engage in decision-making processes related to employee promotions?

   e) Topic: promotions. Context: HR department of a large organization. Goal: to determine promotion factors aside from evaluation results. Nature of the question: what and why. Relationship: correlation between promotions and a range of factors.

   f) Question: What factors in addition to evaluation results play a significant role in promotion determination?

3. If you have developed more than one question (remember: any one problem can lead to a multitude of research questions), decide on your main question based on interest and practicalities as well as the advice of your supervisor and manager.

4. Narrow and clarify until your question is as concise and well-articulated as possible. Remember: the first articulation of any research question is unlikely to be as clear, helpful and unambiguous as the third, fourth or even fifth attempt.

This generation process should go a long way in helping you define a solid research question. But getting there can be a process.

Assessing your research question

A good question is one that works for you, given your interests, resources and time available. This means doing your due diligence first to ascertain if there is a need, support for and resources accessible to address the problem. You might informally broach the topic with your colleagues and supervisor; see if there's been any other work done in the company that could inform your question. When assessing your research question, ask yourself the following questions.

Is the question right for me?

Common wisdom suggests that setting a realistic research plan involves assessing (1) your level of commitment and (2) the hours you think you will need to dedicate to the task – then doubling both. It is very easy to lose motivation or get distracted with other responsibilities and you are likely to need a genuine interest to stay on track. On the flip side, questions that can truly sustain your interest are usually the ones that best bring out your biases and subjectivities. You may want to give careful consideration to any questions where you know you have an axe to grind. Deep-seated prejudices do not generally lend themselves to credible research.

Is the question right for the organization?

Research questions need to be aligned with organizational needs. An early task in the research process is to be able to clearly articulate a rationale for your study that outlines this connection. You need to be able to articulate why the research question is important, and how the findings might lead to improvements for the organization (or by extension its customers, clients, constituents, etc.).

Is the question well articulated?

A research question should be as clear and specific as possible. This is extremely important in workplace-based research whereby timeframes are short. Say you have an interest in big data, including financial metrics and survey results, and you've noticed what you think is the beginning of a trend. Instead of asking 'Why are profits falling in the third quarter?', try to be more specific – identify a particular product or service that is underperforming. You could examine more closely its targeted customer group, or identify what unsatisfied customers have in common. By narrowing the focus to 'Why are X customer groups leaving Y product?' you can contribute to a targeted resolution.

Is the question doable?

Perhaps the main criterion of any good research question is that you will be able to undertake the research necessary to answer the question. Now that may sound incredibly obvious, but there are many questions that cannot be answered through the research process. Take, for example, the question, 'Do romantic feelings for a manager negatively impact an intern's productivity?'. Not researchable. For one, how do you define romantic feelings? And even if you could define them, you would need to find a way to measure them – not things to which many will want to admit. And even if you could do that, you are left with the dilemma of correlating romantic feelings to productivity. It is an interesting question, but not easily researchable.

Other questions might be researchable in theory, but not in practice. Small-scale applied workplace-based projects are often constrained by:

- a lack of time;
- a lack of expertise;
- a lack of access;
- a lack of funds.

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 get the tick of approval from those in the know?

When it comes to articulating the final question it makes sense to ask the advice of those who know and do work or research in the area. Most supervisors have a wealth of research and supervisory experience, and generally know what questions are 'researchable' and what questions are likely to be valuable to the organization. Run your question past your academic supervisor, your organizational manager, senior colleagues and any 'experts' you may know.

Once you have an idea for a research question, it pays to run it through the Good Question checklist (Box 3.6).

BOX 3.6

The Good Question checklist

Is the question right for me?

- Will the question hold my interest?
- Can I manage any potential biases/subjectivities I may have?

- Is the question right for the organization?
- Will the findings be considered useful for the organization?
- Does it have the ability to effect change?
- Is the question well articulated?
- Are the terms well defined?
- Are there any unchecked assumptions?
- Is the question doable?
- Can information be collected in an attempt to answer the question?
- Do I have the skills and expertise necessary to access this information? If not, can the skills be developed?
- Will I be able to get it all done within my time constraints?
- Are there any potential ethics problems?
- Does the question get the tick of approval from those in the know?
- Does my supervisor/manager think I am on the right track?
- Do 'experts' in the field think my question is relevant/important/doable?

So you now have the perfect research question; it meets the criteria and you feel you're ready to go. Let's set it in stone. Well maybe not – research questions can, and often do, 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. 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, modify your question 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. With workplace-based research, above all, you must be mindful of timeframes. At a certain point, changing the direction of your question is no longer possible, without jeopardizing your chance of finishing your project.

When you're satisfied that you're on the right track with your research question, the next step is getting the organization on board.

PITCHING YOUR RESEARCH QUESTION

In order to move forward and gain approval for your research project, you will need to convince the power people that this is a worthwhile issue. You need to sell your idea, and sell yourself as the right person to implement it. And this might sound a bit uncomfortable.

In *To Sell Is Human*, business author Daniel Pink (2012) identifies three common conceptions of sales:

> To the smart set, sales is an endeavor that requires little intellectual weight – a task for slick glad-handers who skate through life on a shoe-shine and smile. To others it's the province of dodgy characters doing

> slippery things – a realm where trickery and deceit get the speaking parts while honesty and fairness watch mutely from the rafters. Still others view it as the white-collar equivalent of cleaning toilets – necessary perhaps, but unpleasant and even a bit unclean. (p. 2)

I certainly felt this way when I first started working. One early pep talk involved the memorable phrase, 'After all, more than 90% of jobs are in sales!'. At the time, I silently wretched, thought 'Not the most interesting jobs!' and aimed to find the elusive 10% within a year. I didn't realize that my boss wasn't referring to *my* conception of sales, which was limited to cheesy car-sale commercials. He meant sales in the form of persuasion, negotiating and exchanging time or money for value.

Selling involves traditional selling of goods and services, but also any exchange of resources. I'm selling when I apply for a job or a promotion – selling my skills as a great investment. I'm selling when I convince a friend (or five friends) to help me move; I'm negotiating for them to exchange their time for a steak dinner. In its most expansive conception, everything is selling: from pitching projects to colleagues, to persuading funders, to negotiating with rowdy toddlers (I'll exchange my time for one more story in order to have the next two hours free while you nap!).

Make no mistake, sales are absolutely necessary and positively booming in unlikely places, like education. When I convince students to part with their resources – time, effort, their smartphones during lecture time – I am selling them the value of the class and its content. Likewise, you'll need to convince your boss (and relevant stakeholders) that they should part with their resources as well. How do you do this? After gathering information on the problem, its urgent or important nature, and possible avenues to research its resolution, you'll need to pitch your idea for the project.

Constructing the elevator pitch

The Otis Elevator Company carries the equivalent of the world's population in its elevators every five days. But even Otis would be surprised to know that his machine carries the potential for real change. What other part of your regular work day gives rise to the opportunity to present your case to those compelled to listen by sheer physical entrapment? Yes, the 'elevator pitch' or 'elevator speech' has long been touted as an opportunity to connect, network and communicate your idea to a host of influential people – all of whom are too lazy to take the stairs.

Now it's more common to hear of the 'elevator pitch' as a concise summary of your value proposition that should take no more time than a theoretical ride with Otis, but it's also perfect for selling ideas.

How you present it is flexible and personal, and completely up to you. Of course, remember timing is important – an elevator pitch shouldn't take more than two minutes. You may need to time yourself. Better yet, pitch it first to your friends, relatives, roommate and your goldfish. There's no set framework for an elevator pitch, but the STAR and SWOT method may be useful in helping you get started.

Using the STAR method

The STAR method is a common business analysis tool. STAR stands for:

- S – Situation
- T – Task
- A – Action
- R – Result

This method is commonly used as an interview response technique. A typical interview question is to describe an example of a challenge you overcame in a team project. The STAR method can help you frame your response: 'In the middle of a large group project last year, a group member became extremely ill and was hospitalized with pneumonia (Situation). We had to figure out how to deliver the project, due the following week while missing an important contributor (Task). I approached our manager with a request to borrow a member from another team, while I negotiated a brief extension with the client, and a team member obtained some crucial details from our colleague during his recovery (Actions). As a result, we were able to deliver the project on budget for the client with only a very minimal delay (Result).'

With a bit of tweaking, this framework can be applied to your elevator pitch. Think of the **S**ituation as the problem the organization is facing, the **T**ask is what you've done to look into it so far, the **A**ction is the proposed research project to investigate fully, and the **R**esult is the ways in which you think the research will add value to the company.

For example, say you work in a consulting firm and you notice that client reports feature only basic graphs and charts of the line and bar variety. You think that this diminishes from the hard work that went into the report and makes the findings seem less compelling and memorable overall. You pull some reports from your competitors that are available online and confirm your suspicions that they are moving ahead with more powerful visualizations. Using the STAR method, what would your pitch look like? The **S**ituation is that the company reports are falling behind their competitors in using outdated and basic presentation of findings in the report. The **T**ask is that you've compared the last few reports going out to clients with the samples available online from your two main competitors and found they are blazing ahead with best practice in data visualizations. The **A**ction you propose is to overcome lost ground by conducting a research project on how to skill up some employees in the new techniques. The **R**esult will be knowledge for creating a catchy document to be used to attract new business through advertising.
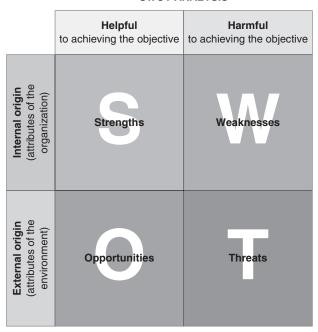
Using the SWOT method

The SWOT technique was developed by the Stanford Research Institute as part of a research project on why corporate planning fails. It is one of the most widely

SWOT ANALYSIS

Figure 3.1 SWOT analysis framework

used business analysis frameworks. As illustrated in Figure 3.1, SWOT stands for Strengths, Weaknesses, Opportunities and Threats.

This framework can be utilized in myriad ways. Say a company is trying to choose between three options for its flagship store. For each location, a SWOT analysis would be conducted and then compared. **S**trengths for one location could be the high visibility on a major city street, **W**eaknesses could be expensive rent and a lack of parking for customers, **O**pportunities would be the presence of wealthy foot-traffic customers in a high density area, **T**hreats could be the competition for city retail spots so rents could escalate quickly in the location.

US President John F. Kennedy once said, 'There are risks and costs to a program of action, but they are far less than the long-range risks and costs of comfortable inaction'. Pitching a SWOT analysis can help you make this type of argument. For an example of how SWOT can be used in developing a pitch see Box 3.7.

BOX 3.7

Pitching with SWOT: Max's story

I work in a large multinational corporation which suffers from an email overload problem, a common affliction. Every company announcement, update or policy change is

communicated by email, but the volume has grown with the size of the company. Now email policy updates border on the overwhelming. A recent attempt has been made to summarize developments on a weekly basis in the form of a newsletter to cut down on the sheer number of announcements, but these emails are also usually relegated to the 'trash bin' – unread. I decided to research whether there is a better way to distribute new policy updates, rather than email.

To create an elevator pitch, I gathered a couple of examples about mishaps when policies weren't followed – policy updates not received/read were linked to a poor outcome for a particular client, group, or employee. This was a Weakness that threatened to erode the company's Strength (our organization's high safety record). I then pitched my research (Opportunity) by pointing out the continued vulnerability of miscommunication (Threat). The framework helped organize my pitch and present it to decision-makers.

Wading into the political stream

The pitch is the first step in wading into the political stream. In order to get support and resources (including access) to answer your research question, you will likely need to pitch to several different individuals, perhaps in different departments. Start with those in nearest proximity first – your own supervisor, colleagues and manager. These people will likely suggest others who would be interested, helpful or necessary in supporting your project. Think of this process as forming broader and broader concentric circles around you and your research project. The wider the circles you have, the more potential access, support and resources you may be able to call on when things get tough.

Cultivating a multi-stakeholder and organizational perspective

> If there is any one secret of success, it is in the ability to get the other person's point of view and see things from his angle as well as your own.
>
> Henry Ford

In order for your elevator pitch to be persuasive and politically relevant, try to cultivate a multi-stakeholder perspective. The easiest way to conceptualize what that means is to envision its opposite. What is the opposite? One that is focused on *you*, exclusively. You may have heard of Dale Carnegie's book, *How to Win Friends and Influence People.* It's still a bestselling text 80 years after its first publication in 1936, truly unprecedented for a self-help book. Why is it still relevant despite the fact that it was written during the Great Depression? Because it teaches people how to sell by solving problems from the other person's perspective.

If you are considering beginning your elevator pitch with any combination of 'I need to do a project on', 'I am interested in' or 'I think we should …' stop right there. If you want people to approve, sign off on, fund, or otherwise take an interest in your

idea, you need to approach it from their perspective. What are their needs, interests and priorities? You need to consider the perspective of the department or the larger company. Once you have an idea, try putting it into different frameworks to see its various sellable angles. You can increase the persuasiveness of your pitch by cultivating a multi-stakeholder perspective and accentuating the benefits to others of your research.

Using the example from the STAR analytical framework, say you would like to research a few training programs that will upskill company employees in new data visualization techniques using open-source technology. You've broached the topic with your manager and the head of several departments and there is enthusiasm for the idea. What could a multi-stakeholder pitch look like?

First, consider organizational goals – to deliver high-quality and cost-effective client consulting reports. High quality visuals would help facilitate this goal but you need to be specific about how this is beneficial to different departments. One strategy is to identify influential groups in the company – typically this is sales (or client facing) and finance. Depending on the type of company, this may also include IT or others. Make sure your proposal meets departmental needs, to increase sales and decrease costs, respectively. For the sales team, you could get their buy-in that high-quality visuals would be a value-add to client reports and a marketing tool to sell more services. For finance, you need to prove cost-effectiveness. The final benefit is the employees themselves; data visualization is a growing market demand and being an organization on the cutting edge could help retain talent. By utilizing a multi-stakeholder perspective, you can get buy-in from various influential groups as well as the decision-makers themselves.

CHAPTER SUMMARY

- Organizational issues can be divided into problems and opportunities, but whichever you pursue, both must respond to an organizational need.
- For a research question to be useful to an organization it must to be practical and politically relevant.
- A good question is one that works for you, given your interests, resources and available time.
- One of the most important parts of your research project is selling it! An elevator pitch is essential for garnering support and resources to address your research question.