TWO
Misperceptions, challenges and judgement calls in research

2.1 INTRODUCTION

Despite the importance of research, there are always concerns about why research is necessary. For example, why would history matter such that we have to do a background literature search on every research study that we do? How are these past research findings applicable to the current context at this current time? Are they still relevant? Undergraduate students always wonder why they would engage in postgraduate research studies. What is the value of having another (research) qualification? Does a postgraduate degree enhance their job prospects? Does enrolling in a research Masters degree make any sense to a potential employer? The list of questions goes on and on. Yet, many of these questions are often left unanswered.

While there may be answers to some of the above questions for some, for others the uncertainty surrounding these issues generates some degrees of misunderstanding with what research means. This also leads to misperceptions of research, some of which are covered in the next section of this chapter.

In the process of engaging in research, there are some challenges that one can argue are generic to the research process. While there are specific individual challenges within each element of the research process that will be covered in subsequent chapters, this chapter will cover the more generic challenges for management and business researchers. First, there is the challenge of the choice of research area to work in, followed by the settings one should investigate and focus the research question on. This is followed by the challenge of ensuring there are substantive theoretical developments and the use of hypotheses and their development. Finally, there are also challenges relating to uncertain research outcomes and time constraints that we often face in our research activities.

As Chapter 1 has indicated, research is by no means a straightforward activity and involves a simple process. It is complex and will always include judgement calls. This chapter will discuss some of the judgement calls across the research process, though these are not exhaustive.
2.2 MISPERCEPTIONS OF RESEARCH

2.2.1 Research is only for people who want to become academics and has little significance for practice

With the business community pushing for more research-informed managerial decisions, the statement that research is only for people who want to become academics must be the most significant misperception. Research is certainly at the core of someone pursuing an academic career. While most universities also push for teaching excellence as part of the performance expectations of faculty members, research remains a key performance criterion for academics.

However, in the current business climate, firms have to constantly generate competitive advantages and wish to make decisions in a more organized way. This is especially imperative for larger organizations. As such, executives often turn to making research-informed recommendations by engaging in projects with more significant research elements. Many organizations even set up in-house research teams. Hence, over time, people with research skills are in high demand in the markets. Moreover, market research and consulting companies also hire those employees with research skills. Their investments in such human capital are also a reflection of their clients’ – the business community’s – desire for research-informed decision-making.

There is a perception that research is ‘in here’ and practice is ‘out there’. How can one apply what is ‘in here’ to a context ‘out there’? It is indeed true that there is a ‘gap’ between research and practice (Ireland, 2012; Rynes et al., 2001). ‘Gap’ implies some misconnections between the two modes. However, this does not mean what is being researched by researchers is more advanced than what is being practised by managers and vice versa. This stereotyping actually reinforces the ‘gap’, making it seemingly difficult to bridge research and practice.
In reality, given that more executives are returning to business schools to engage in advanced learning and that many more are working on research-informed projects, this gap is likely to be bridged gradually. This is helped by academics’ increasing participation in research-led teaching and consulting projects. The onus is on the academics to bring the latest thinking in management and business research to the business community and for managers to apply what they learn in the classroom to their work environment.

2.2.2 Projects related to practice do not need research rigour

The section above suggests that good research should have relevance to practice. Now, should good practice be well grounded from research? For any good practice to be trustworthy, it has to appeal. How does one’s idea appeal to many others? This leads us back to the ability of an idea to be convincing. Part of the reason for organizations to increasingly engage in research-informed decision-making is this purpose of convincing power. This also means that any practice project will need to have some degrees of research rigour for it to be convincing. In this vein, Staw (1995) and Vermeulen (2005) put forth strong arguments for the need for rigour in research in practice-oriented projects.

2.2.3 Research is isolating and frustrating

When you attend a taught course in a research programme, you are surrounded by your fellow classmates who will move through the learning processes together with you. However, as you reach the research phase of your study, very often you start to feel isolated. This could mainly be due to the fact that none of your peers are working on a research topic related to yours. As a result, discussion around your research topic can hardly occur as everyone is busy with their own research work. Moreover, it is also difficult to expect others to learn about your work in order to provide constructive comments. It is either too difficult for you to explain everything (including the literature) from scratch or too difficult for them to absorb your
MISPERCEPTIONS, CHALLENGES AND JUDGEMENT CALLS

materials, not to say there is likely to be little incentive for them to do so. As a result, you might find yourself feeling the ‘boredom’ of the research process.

CONVERSATION BOX

Professor: Diane, come and present your research idea to my second year undergraduate class, it will be useful.

Diane [PhD student]: I’m happy to do that, Professor. But do you think they will appreciate what I’m doing given that this is a very specialized area and they are not into specialization in their curriculum yet?

Professor: The presentation, even without specific feedback, will help you to work out how clear you are with your idea. So just come and enjoy the process. A good presentation will always be appreciated by the audience.

Nowadays, business schools have larger cohorts of research students. A cohort allows students to meet other students engaging in research. While it is not easy to find many others who work on the same area of research, discussions about the research process and talking to others about your research is a practice that is encouraged. Discussing your ideas with others will help you crystallize your thoughts. As a rule of thumb, when your listener does not seem to grasp your broad research idea, then there is cause for concern. It could either be a case that your explanation is unclear or that there are loopholes in your thinking. One would hope that the former is the case. Very often, discussions can also lead to new ideas that eventually result in collaborative research. The bottom line is that there is always room to create your own active research environment so that you can avoid feeling isolated.

The nature of research is such that the execution of the components are far from sequential and that it often takes a longer time than you expect. Every step of the process is filled with hurdles. You spend time in trying to find a workable research topic – bouncing off many ideas that are found to be not feasible. You spend time on crafting your hypotheses and propositions yet they do not seem to be flowing smoothly. You try to collect the data and find that access to organizations and individuals are harder than you expect. Even archival data are not available on your chosen sample. Worse still, the results from your analysis do not support your hypotheses and you wonder what went wrong. For the paper that you crafted for a journal, the reviewers came back with lots of comments that disagreed with your viewpoints and rejected your paper. The last item here is particularly frustrating, even for seasoned researchers.

All these issues are part and parcel of the research process. For those who are very keen on research, there is no intention to discourage you from engaging in research activities. You just have to dig in further in the hope of a good outcome after more work. You might find some of these challenges to be mountainous
and become demoralized. But do not feel that way. Under most circumstances, there is always a way out. Try to work yourself out of the situation with the help of your supervisor or faculty staff members. You should also talk to other researchers who might be able to provide help or give insights. Facing up to such challenges will only make your research better, and at the end of the process you really will find doing research to be satisfying and enriching rather than frustrating.

2.2.4 Research is subjective

Professor: Class, when you do your background literature work, please adopt a critical perspective to it.

Joan [Honours student]: Professor, surely we can trust articles that have been published? And with our limited knowledge at this point, how can we adopt a critical perspective?"

Professor: Indeed, it might sound like an unfair expectation. But while we know that most studies have undergone a good quality control process, the bottom line is that some research is better than others. Some research is slightly more objective than others too. To have a critical perspective is important so that you have to be convinced of what you are reading and citing.

Research students usually do not challenge what is in print – be it in journals or magazines. They assume some sort of checks have been in place and thus anything that goes to print has a certain quality attached to it. However, even reliable, print-published research papers may have some subjective opinions attached to them. As research students are less exposed to the difference between journal articles and articles that are published in other outlets, such as magazines, they are likely to assume that journal articles can potentially have an inherent subjective element to them, as with articles in other outlets.

It is clear that not all journal articles and research are objective. Very often journals publish opinion pieces that incorporate an expert’s view on a particular topic. However, most journals are peer-reviewed. Thus, a paper submitted to one of these journals has to be reviewed by two or more persons who are specialized in the topic area of the paper and are able to assess its quality. In this way, subjectivity of research is checked and hence kept to a minimum. It is not easy for subjective views of individual authors to pass through such a rigorous process of quality control. Therefore, to a large extent, peer-reviewed research outputs that you commonly see do have a low degree of subjectivity.
The more the merrier seems to be the norm of thinking in the minds of junior researchers and research students. After all, wouldn’t there be more (chance of) contribution given the larger scope of the study proposed? Moreover, would it not be fair to say that in management and business research (and in social science for that matter) the complexity of phenomena would suggest that many factors need to be taken into account in our research to rule out alternative explanations?

These are all valid points in favour of a wider scope of coverage in a research work. But in reality, these are only ‘theoretically’ right. Let’s refer back to the conversation box above. Tina is proposing 20 factors in her research proposal. It certainly provides good coverage, but only if she is not doing all the research herself! Imagine trying to collect data for all of the factors in the same research project. In a world of research without other forms of constraint, such as time and resources, this might be possible – but nonetheless, it is unlikely to be feasible in practice.

When a sizeable number of factors are introduced, it begs the question as to which one(s) might provide the key contribution. It is unlikely that all of them will contribute equally to the research. This means that you will still have to sort out the key factors for contribution. This assessment will also allow you to know which other factors are peripheral to the research work. It is these factors you should consider eliminating when the time comes for narrowing the focus of the research. Some topics are considered advanced in their area. For these cases, the desired form of research work is always about deepening our understanding rather than broadening our understanding. Proposing broad topics in these areas is likely to lead to work with marginal contribution.

So the key point here is that more coverage does not mean better. In fact, ‘less leads to more’ will prevail in most areas of research in business and management. The value of more work with marginal contributions has been questioned (e.g. Shaver, 2013). Studies with broad coverage will fall into that category. As Chapter 1 has highlighted, we should seek parsimony in our work, and thus more work that deepens our understanding in our field will be desirable.
2.2.6 The research process cannot be linear and prescriptive

CONVERSATION BOX

**Professor:** Neil, can you look into how to measure these constructs that you’re proposing in your research?

**Neil** (supervisee): I’m still doing my literature search. Once I finish this off, together with hypotheses development, then I’ll look into the constructs. This might take me another month. Will this be alright?

**Professor:** You should work on some of these research components concurrently.

**Neil:** But I thought this should follow a sequence, like most research books suggest?

**Professor:** Not always true in practice though.

Partly as a means to deal with uncertainty and unknowns when they are exposed to research, students always desire a step-by-step approach. How many times have academic staff members been asked by their supervisees to provide them with specific itemized schedules to allow them to complete their research on time? As briefly considered in Chapter 1, the research process is by no means a linear one. Research always follows a cyclical process, whereby feedback loops link the components.

Not only will there be no answers to a request for a step-by-step approach to research, there are also many tacit practices in the research process. These are often referred to as judgement calls, some of which will be considered in the last section of this chapter. Some rules of thumb exist in the research world, and we have to make judgement calls all the time. While there is certainly codified knowledge in research, there are also many areas in the research process where there are no specific answers to questions such as ‘am I right to do this?’ and ‘is this OK?’ More often, it is about having an answer that is right for 80% of the time, or ‘more right and less wrong’.

2.3 CHALLENGES IN THE RESEARCH PROCESS

2.3.1 The choice of research areas and settings

While it seems there is an abundance of research areas that a business and management researcher can work on, and researchers have freedom of choice of research interests, there are some extraneous factors that often shape our interests in one direction or another. For students, the choice of research area can be constrained by the research expertise of the faculty members. As a research student, you will be allocated to research supervision under a faculty member who will provide expert advice on your research topic. Supervision is easier when your research topic is well aligned with the research areas of the faculty.
Misperceptions, Challenges and Judgement Calls

member. In some schools, whether a student can proceed with a particular research agenda or not actually depends on the availability of faculty staff to supervise in that area. As the management and business field is extensively diverse, many of the research areas become inter-disciplinary. It may then not be uncommon to find that some research areas are not represented in the business school you are enrolled in as a research student. Thus, this might influence your decision in choosing a research area of interest, or might even not enrol in a particular university.

Some research settings are chosen by design – the research topic requires the choice of certain types of organizations or groups for observations. Others are chosen due to convenience. Take for example a researcher who wants to study empirically the dynamics of group behaviour. While groups exist in most organizations, over-sampling of organizations for the study of groups within them introduces a lot of ‘noise’ due to differences across the organizations. In such cases it will be appropriate to choose organizations that usually operate with groups, such as consulting companies. When a research design is chosen by convenience, it is common for a researcher to adjust the research issue depending on the organizations he/she is able to access. This is necessary as this convenience selection is likely to be scrutinized for objectivity and validity.

The familiarity with particular methods is another factor that usually influences the research topic choice and setting. Researchers have a tendency to choose their most familiar methods for research instead of exploring alternative new methods to investigate the research topic (Kaplan, 1964: 28). In fact, Trow (1957: 35) suggests that researchers even choose research issues that are easiest to resolve using the methods they are familiar with. This would be an extreme case, but not entirely impossible to envisage. The ability to execute a research study requires multiple sets of skills that can stretch what an individual is capable of. In many such cases, individuals overcome this hurdle by collaborating with others.

2.3.2 Theory and hypotheses development

When attending any research-related course, you will often run into the word ‘theory’. We often associate theory with ground-breaking understanding in science, such as Einstein’s Theory of Relativity. In our research, we also run into various theories such as transaction cost economics and agency theory (both borrowed from economics), resource-based theory, knowledge-based theory, etc. There are many more micro theories relating to business and management.

When you are doing your assignment or research proposals, you are often asked ‘what is your theoretical contribution?’ This question often immediately puts stress onto you: what is theoretical enough? You will wonder why you are being asked this question, as you are definitely not in the business of generating ‘new’ theories. You also hear about ‘theory building’. What does that mean? What constitutes the process of theory building? How does a researcher know when he/she has a theoretical contribution? These are some of the issues that will be
discussed in this section. In addition, some of the implicit rules of theorizing that make some research more rigorous than others will also be highlighted.

Paradigm development

Paradigm development refers to the technological uncertainty associated with the production of knowledge in a given scientific field or subspecialty (Lodahl and Gordon, 1972). Every field and subspecialty goes through different stages in their development; some fields are more developed than others. Accumulation of knowledge depends a lot on the progress of research in the field, which is reflected in the extent of theoretical and methodological understanding within the field. Normally, this requires some levels of consensus among researchers within the field or subspecialty. Consensus refers to the common understanding of theories and methodologies judged to be suitable and fit for the field by researchers within the field.

Failure to have consensus will lead to fragmentation of a particular field and will slow down progress. Zammuto and Connelly (1984) have argued that the fragmentation of the field of organizational science has obstructed its scientific progress. Social science, unlike physical science, often requires more time to be spent in explaining concepts that are complex and it is often difficult for researchers to come to a consensus. Highly developed paradigms have fewer such issues and thus allow easier communication and consensus (Salancik et al., 1980). This in essence has many implications for the business and management field. For example, it takes a longer time for research to be published in good management journals as compared to journals from the more paradigmatically developed scientific fields (Beyer, 1978; Hargens, 1988).

As relatively new and complex disciplines, the business and management field faces significant challenges in obtaining consensus on various aspects of research. The lack of consensus in business and management also makes training in the field difficult. In management research, we often run into multiple theories to explain a particular phenomenon. The research findings we gather from our literature review process often show a lot of inconsistencies in a research area. These complexities make it difficult for students to distinguish between good and bad theories, and can lead to a longer time period needed to complete their research degrees (Zammuto and Connelly, 1984).

As the management field also has a liking for novelty, there are always efforts to seek new ways of looking at phenomena or new areas of research. That is, there are proactive efforts to detract from mainstream thought in the field. This tendency can take away good research time and effort from deepening our understanding in the mainstream of the management field. Webster and Starbuck (1988) argued for the need for theories to drive towards more consensus in management research. The degree of paradigm development also differs across subspecialties within management (Pfeffer, 1993). To gain consensus, Pfeffer (1993) suggested that minimizing the disagreements over key research questions, the ways relevant variables are measured and modelled, methodologies,
theoretical models and the rules of determining which of these aforementioned four domains have more or less contribution, are critical to the progress of a field.

What is theory? And what is not?
What exactly is theory? Theory can be briefly defined as the branch of a science or art that deals with its principles or methods, as distinguished from its practice. Thus, theory deals with the underlying logic of phenomena. Given its complexity, however, it is always unclear what a theory is (e.g. Bacharach, 1989). In essence, theory takes various forms and actually differs across fields. While it is difficult to define exactly what a theory is in business and management, Sutton and Staw (1995) provided a good overview of what theory is not.

Referencing is not theory
Chapter 1 has briefly highlighted the ethical behaviour of correct referencing. More will be discussed about referencing in the context of conducting a good literature review. Good references are commonly used to justify the linkage between constructs as an author attempts to convince the reader that the linkage has found support among other researchers. Articles published in good journals and articles by good authors are especially good references for such purpose.

Just as an example, let us say that we are interested to test the relationship of the effect of top management diversity on firm performance. The concept of top management diversity itself does not have any positive or negative meaning attached to it. The concept merely reflects a static structure. However, we also know that some degrees of top management diversity can lead to learning and debates that enhance decision-making and ultimately better firm performance. With this in mind, the tendency is to find evidence from previous research to support the relationship between top management diversity and learning and debates, and between learning and debates and enhanced decision-making.

Finding support and citing references on both these linkages, however, does not constitute theory. Theory involves the articulation of the linkage itself. One needs to articulate the flow of logic from top management diversity through learning and debates to enhanced decision-making and eventually better firm performance. Citing previous works on these linkages does not constitute theory development.

Data and findings are not theory
Related to referencing articles, arguments merely based on prior findings do not constitute theory. For example, if we theorize based on prior findings, we can find ourselves able to link ‘the need to access resources’ with ‘better firm performance’ with prior findings on the link between the need to access resources and the conduct of acquisitions and the positive effect of acquisition on firm performance.
There are two issues here. First, while the two linkages are well established individually, the logic supporting the conclusion is less appealing. Acquisition in this case is just an ‘en-route’. So what is the implication of finding the linkage between ‘a need’ and ‘performance’? Second, these two linkages are supported in different studies, probably in different study periods across different industries and firms. Are we able to connect and assume that the second linkage works for the data used in the first linkage without actually testing that? The answer is a definite no.

Thus, prior findings cannot substitute for causal reasoning. For every research study, the linkages must be well articulated and grounded in theory and not based on bits and pieces of empirical findings derived from different studies using perhaps different contexts.

**Listing of variables or constructs is not theory**

In many circumstances, we are faced with the situation of introducing some new variables to explain a certain phenomenon. Many a time, we feel that we have addressed a gap in the literature by introducing this variable and find an effect on the outcome variable. This new finding can then add to more theoretical understanding of the phenomenon.

However, in actual fact, adding a new variable or variables to test the effect on an outcome variable does not constitute theory or theory development. You need to justify why adding such a variable actually furthers our understanding of the phenomenon under investigation. You also need to argue how this new variable can fit in with existing frameworks and other variables in this literature.

In addition, just stating that there is a relationship between two constructs does not contribute to theory. Ex-post observation of a relationship does not in itself contribute to our understanding. In fact, finding a relationship without arguing for that makes it a ‘data mining’ exercise. Likewise, testing and concluding that some variables have bigger impacts on the outcome variable than other variables also does not constitute any theory development. A comparative test of variables is not the same as a comparative test of theory. In order to contribute to theory, these variables must be connected by logical arguments and articulated as such.

**Hypotheses (or predictions) are not theory**

The deductive approach to research emphasizes the need to have some hypotheses or predictions. Hypotheses may also be formulated in the course of an inductive approach to research. A hypothesis is a tentative explanation for an observation, phenomenon, or scientific problem that can be tested by further investigation. The number of hypotheses in theses, dissertations and published papers varies. The number of hypotheses is not an indicator of the degree of intellectual contribution in the research output. Nor is it necessary to have a pre-determined number of hypotheses. Sometimes you may come across a
published paper with only one or two hypotheses, while other times you may run into a published paper with a dozen hypotheses!

Again, having a good list of testable hypotheses does not constitute theoretical contribution. Hypothesis statements only show the ‘what’ and not the ‘why’ of the relationship between constructs. They are just statements without leading arguments. In order to meaningfully contribute to theory, these statements must be well grounded in logical arguments.

So what might be a theory? And what constitutes a good theory? According to Sutton and Staw (1995), a good theory is often representational (graphical) and verbal (rich process to be described). It must be simple and show some interconnectedness, and it normally starts with one or two conceptual statements that serve as stepping stones for building logical arguments.

The relationship between theory and data is always complicated. The key question has been which precedes the other? How does one construct a research study that starts off with a theory? This is by no means easy. Thus, it is not surprising to find that observation is always the easier and more common way to motivate a research study. After all, the entire consulting industry is built around problem solving research – that theory is very much driven by observations. More recently, however, many researchers have started to highlight the potential intertwining relationships between theory and data (e.g. van Maanen et al., 2007).

There are mainly three forms of papers, namely pure theoretical, pure empirical and mixed approach papers. Pure theoretical papers are always more difficult to write. The contribution has to be substantial. Thus, it is seen as a dangerous route to adopt for researchers, especially less established or new researchers. Of course, research students are ill-advised to engage in theoretical studies as their first research work. As fewer researchers work on pure theoretical papers, the readership of journals focusing on pure theoretical papers is limited. Thus, it is not commercially viable for many publishers to engage in publishing theoretically focused journals. Note that the Academy of Management Review is one of the few top journals with a special focus on publishing theoretical papers. Pure empirical journal outlets are plenty. However, many also tend to be journals of lower quality. The top journals tend to publish articles that have both theoretical and empirical contributions. A mixed focus also ensures greater readership.

The requirements for both theoretical and methodological rigour have escalated in recent years. To advance theory incrementally, building on existing methods in the literature, is possible. However, to advance theory in radical ways, it is often hard to provide support with the help of extensive observations. Sutton and Staw (1995) suggested a need to rebalance the theory–method selection process. An important element is to relax conditions on new or provocative ideas to encourage advancements. In such cases, illustrative data, rather than definitive data, may be deemed adequate.

If you think someone has told you that he/she will teach you how to write theory, you must have either heard wrongly or he/she must have exaggerated
his/her ability to deliver. The ability to write theory can only come with practice and thus is considered by many researchers as a by-product of the system. Even for seasoned researchers, this part of the research process is the most challenging and difficult. This is also a main component of differentiating a lacklustre paper from a good paper, and a good paper from an excellent one.

Hypotheses

Hypotheses are statements derived from an existing body of theory that can be tested using the methods of the particular science. Hypothesis testing establishes the significance of a research finding. The standard research statement of a research objective includes both a null hypothesis and an alternative hypothesis. The null hypothesis almost always predicts that a relationship between the constructs to be tested does not exist. The alternative hypothesis, on the other hand, predicts that a relationship exists between constructs. This prediction can be positive, negative or bi-directional. It can also adopt other forms, such as curvilinear, staggered, etc. In essence, hypotheses guide the direction of the study.

A good hypothesis must have the following three elements:

1. It must be adequate for its purpose. The hypothesis must clearly state the constructs (variables) to be tested and their relationships.
2. It must be testable. A testable hypothesis is one that has stated the relationships between the constructs. It is one in which a researcher can reject or accept the null hypothesis by conducting statistical tests.
3. It must require few conditions or assumptions. It is very unusual to have hypotheses that are generic, i.e. that apply to all contexts, for example, across industries and across countries. However, when a hypothesis is based on too many assumptions or conditions, it is not well formulated, leading to questionable results.

Below we provide a couple of examples of hypothesis statements and suggest some ways in which these statements can be improved in their formulation.

Hypothesis: Good strategy leads to better firm performance

There are two constructs in this hypothesis – good strategy and firm performance. The term ‘leads to’ suggests a causal relationship from good strategy to firm performance. This would look like a straightforward and good hypothesis. But let us take a closer look. How do you measure good strategy? Any researcher familiar with firm-level research would know that a good strategy is one that improves a firm’s performance! Now, does the hypothesis make any sense if firm performance is the way to tell whether a strategy is good? If a firm’s performance is good, then there is a possibility that it has employed good strategy. Thus, the hypothesis statement forms a loop and in effect is not testable. We call this form of hypothesis ‘tautological’.
Hypothesis: a pay for performance policy leads to more employee motivation and less employee turnover

There are three constructs in this hypothesis, namely pay for performance policy, employee motivation and employee turnover. The term 'leads to' suggests a causal relationship between a pay for performance policy and employee motivation, and between a pay for performance policy and employee turnover. Now, we see that there are two relationships involved in this hypothesis. First, pay for performance has a positive impact on employee motivation. At the same time, it also has a negative effect on employee turnover.

Four output scenarios are possible in any empirical test conducted on these relationships:

1. A pay for performance policy has a positive effect on both employee motivation and employee turnover.
2. A pay for performance policy has a positive effect on employee motivation but a negative effect on employee turnover.
3. A pay for performance policy has a negative effect on both employee motivation and employee turnover.
4. A pay for performance policy has a negative effect on employee motivation but a positive effect on employee turnover.

Note that these scenarios are mutually exclusive. We know that the result in (2) will mean that our hypothesis is supported and that in (4) will mean our hypothesis is not supported. But how do we interpret results in (1) and (3)? In these cases, the hypothesis is partially supported. We neither reject nor do not reject the hypothesis. We call this complication a double-barrel. Double-barrel issues often occur when we try to test two different relationships in one hypothesis.

It is important to note that different ways of formulating a hypothesis imply different methodologies for testing the hypothesis. Let’s look at an example: say we are interested in testing the effect of a firm’s foreign investments on its performance:

Formulation 1: The greater the level of foreign investments, the better the firm performance.
Formulation 2: Firms with more foreign investments are likely to perform better.

While the two formulations look similar and have the same objective of testing the effect of a firm’s foreign investments on its performance, they are very different in meaning when it comes to testing them, depending a lot on what you seek to achieve in your study.

Assessment of Formulation 1: Both the constructs in this hypothesis suggest that the measurements are continuous. On testing the hypothesis, we will be able to draw conclusion on whether firms with a greater level of foreign investment perform better or firms with a smaller level of investment
RESEARCH DESIGN FOR BUSINESS & MANAGEMENT

perform worse. Notice that level of investment can mean different things: it may mean the amount of foreign investment or it may mean the number of foreign investments.

Assessment of Formulation 2: This formulation has used the word ‘likely’ as opposed to ‘lead’ or ‘affect’ or ‘impact’ or other similar words. The use of the word ‘likely’ suggests a likelihood component. It means there are categories we compare against. The specific test to be used for testing this formulation would be to divide the firms into groups with varying levels of investment. Then we compare the average performance of each group. This can either be done by using a t-test across groups or by creating dummy variables to represent the groups for regression models. (The details of such tests are covered in Chapters 8 and 10.)

As shown in the example above, the writing of hypotheses is critical to avoid misleading readers. But there is increasing concern that researchers are slowly ignoring the specificities of phrasing hypotheses. Moreover, you will probably find little coverage in any research textbooks trying to educate researchers on hypothesis writing. Researchers tend to do as they learn from reading hypotheses in good journal articles. Clearly, this is an area that deserves significant attention in research education to ensure that research protocols are not lost.

2.3.3 Highly uncertain research outcomes

If there is something that cannot be avoided in research, it is that research always involves high uncertainty, especially with respect to the outcome from the research efforts. While this book discusses a lot of tacit issues you will run into when you actually do your research, many of these issues do not have a fixed answer. In other words, it is possible that some of these will not be resolved in your research situation – making it necessary for you to alter your research plans. Seasoned researchers are better at avoiding some of these potential pitfalls and are generally more able to anticipate these problems. Such anticipation comes with experience. If you are a research student, you will have to slowly learn the trade, both through the hard way of encountering and countering unexpected hurdles and the easier way of seeking advice from your supervisor or senior researchers when you cannot resolve the issue after several attempts by yourself. Over time, you will find yourself better able to cope with the highly uncertain nature of research activities and better able to anticipate potential problems.

2.3.4 Time constraints

Different degrees have different time specifications within which to carry out the research activities. Honours dissertations are usually done within six months and Masters theses usually within a year. PhD studies usually take 3–5 years. Given these time constraints, as a student you may start to envy faculty staff members as you probably think they do not face similar restrictions in their research work. However, that is not true. Faculty staff members are expected to
MISPERCEPTIONS, CHALLENGES AND JUDGEMENT CALLS

produce research outputs and the expectations of these outputs are higher than for the dissertation or thesis that you are writing. This expectation can come from the institution, but will mainly come from the journal that the faculty staff member is targeting to publish her/his research. To get published in a good journal is becoming increasingly demanding. As such, the necessary research is increasingly time consuming – making it difficult to have a fixed deadline. In addition, in order to get greater mileage from research efforts, faculty staff members often engage in large projects that take a longer time to realize the outcomes. While these justify the time needed, faculty staff members also have to have research outputs to show for their work (and employment basically). Some journals have special calls for papers, which have deadlines. Likewise, conferences and grant funding applications all have deadlines. Hence, faculty staff members are also facing time constraints in many of their research activities.

Time consumption and time constraints represent significant challenges to all researchers, whether you are a research student, a new researcher or a more seasoned researcher. Putting these into perspective, it is not surprising to observe that research processes in tertiary institutions mimic those in consulting companies, whereby time availability plays a big role in the research process and outcomes. Sometimes, time constraints can also put stress on our desire and ability to perform our research with the maximum rigour.

2.4 JUDGEMENT CALLS

When conducting research, you will run into many judgement calls in each component of the research process. Unfortunately, there are only so many examples that we can give in terms of judgement calls in this or other books on research design and methods. This section seeks to cover a few of the common ones, but judgement calls that occur in various aspects of the research process will be dealt with throughout the book as they surface.

Judgement calls are made with experience. As a new researcher or a research student, if you reach a juncture where you cannot decide on what to do and there is no documentary guidance on what are the potential solutions, then a judgement call will be necessary. This is like the saying of experts – ‘I don’t know how I know but I just know’. As judgement calls are not well documented, they can be subject to various interpretations. Among those discussed by McGrath et al. (1982) in their book devoted to judgement calls, many are not management- or business-specific. Among the judgement calls in research, there are a couple that are generic to management and business research and which can be particularly thorny.

The first is the issue of ‘what is new?’ How do we gauge what is novel? This is clearly the first hurdle for most novice researchers and research students when they look for a research topic. Even the level of judgement for what is new differs across research areas. For example, it will take a more significant effort to find what is new in the area of organizational behaviour, which has a long history, than
to find a relatively new topic in strategic management, which is a relatively younger area of research. Still, despite this difference, you will often be confronted with how new is ‘new’.

CONVERSATION BOX

Alice, a new research student enrolled in the research Masters programme, is being asked to look for a new research topic for her first assignment. She has found the area of strategic human resource management to be interesting and decided to work on the specific area of how firms that use a diversification strategy can be facilitated or hindered by the adoption of a customized performance appraisal.

Professor: Alice, how did you come up with a topic?
Alice: I have read through some of the recent journal articles in this area and there are very few that are close to the specific issue I have identified.
Professor: So how do you know if that’s new?
Alice: Not sure, but since no one has done it before, especially in the context of Asia Pacific countries, it must be new.

The conversation between Alice and the Professor raises a few issues. First, she relied on a few articles to come up with the topic. This decision is made through a judgement call, though this judgement call can be avoided by conducting a more systematic literature review. Second, she believed that any topic that no one has done before is new. Third, she assumed that even if there have been related studies done before, no one has conducted a similar investigation in Asia Pacific countries, henceforth her proposed topic has to be new.

Two questions arise here. First, do context contributions count? To most new researchers and research students, these are indeed new. But it can be argued that unless your research topic needs to be contextualized, it is irrelevant to label a topic ‘new’ when simply applying it in a different context from what has been done before in previous studies. The contribution is too marginal. Secondly, if no one has done it before can it be deemed new? This is probably somewhat true for half of the time. A topic that has not been done before may have inherent problems, like difficulty in obtaining data for investigation – for example, studies related to ethical practices in organizations. A topic may also not have been investigated before due to its relatively weak appeal to researchers – for example, there has been extensive research on cross-functional teams and how these generate innovations. While this research has largely been conducted in organizations in more advanced economies, there is little reason to believe that conducting similar research in Asia Pacific will yield insightful results unless the attributes under investigation relate to cultural or national differences. ‘New’ should also encompass being interesting and contributing to the research and business communities.

For a younger field, knowledge is relatively green and researchers in the area are more likely to be open-minded towards exploratory research (Sutton and
MISPERCEPTIONS, CHALLENGES AND JUDGEMENT CALLS

Staw, 1995). For example, Singh et al. (2003) argued that strategy is a younger field and concepts are relatively harder to measure and observe. Thus, it is necessary to allow more leeway when trying to replicate strategy studies for validity purposes. They further proposed a loosening of the replication definition of natural sciences to allow ‘good-enough’ replications to be conducted in the strategy field. Due to the uneven evolution of various fields (Nelson, 2003), it then makes sense to bear in mind that researchers have different levels of tolerance towards what is ‘new’ in their field.

Thus, what is ‘new’ constitutes a major judgement call that poses a huge challenge to new researchers and research students alike. Only going deeper into the topic and talking to people will allow you to generate the experience to make judgements on the newness of research topics.

The second major judgement call is parsimony, which we have highlighted as one of the fundamental research expectations in Chapter 1. Parsimony is about the marginal contributions of introducing extra information. In order to be applicable to practice, it is essential that research is parsimonious. Otherwise, it will be difficult to take on board for application.

CONVERSATION BOX

John, a new research student, is looking at conducting a quantitative study on how compensation systems affect firm performance. After going through a literature search, he has identified variables that are of significance to firm performance. Yet, he is confused as to how he would know which ones are important.

Professor: Yes, you definitely need to include these other potential determinants. Otherwise, you cannot ensure that the impact you observe actually comes from the compensation systems.

John: But there are so many variables – environmental, industry and organizational – that affect firm performance. Which ones should I include? How many do I need?

Two issues arise from this conversation. First, how does a researcher decide which variables to include in a study? Other than the key variables, very often we have to include some variables that have an effect on the outcome variable in our analysis in order to control for these effects and single out the effect of the key variables. However, a search of the literature will reveal many such potential other variables to be included. The response to this is that we will have to include enough variables to allow us to single out the impact of the key variables. In order to do that, we will have to balance the marginal contribution of these potential variables and include only those that have substantive effects on the outcome variable.
Second, research students always like to ask for a number – the number of variables to include for analysis. They need this number to help with their decision as to whether to include certain variables or others. Unfortunately, this is again a judgement call. There is no fixed number of other variables you should include in your study. However, if your sample size is large enough, include as many variables that have an effect. If your sample size is small, for example less than 100, then it is advisable to let your sample size determine the number of variables to include for analysis. This is to allow for degrees of freedom, which will be discussed when we touch on research analytical methods. But for instruments like surveys, it is almost impossible to state a priori what will be the sample size of your study. That is, the response rate is not known beforehand. In such cases, the final number of variables to be included will be determined only after the data are collected. Hence, at the start of your research, you will just have to include all variables before narrowing down depending on the final sample size.

Research is conducted at different levels, with researchers going for different outputs: Honours dissertation, Masters thesis, PhD dissertation, non-peer reviewed journal article, average peer-reviewed journal article and top peer-reviewed journal article. Each of these outputs has different expectations. For example, the expectation of the contribution of an Honours dissertation is lower than that of a Masters thesis, which is in turn lower than that of a PhD dissertation. Even within each level, there can be differences. For example, among top peer-reviewed journals there are some that have more theoretical focus while others have more empirical focus. Yet while we know this difference in general, it will be judgemental to address a question like ‘does the Masters thesis have to have twice as much contribution as the Honours dissertation?’ The nature of research is such that it is the researchers who judge the contributions. Thus, while the general expectation of the differences is commonly understood, specific differences will be at the discretion of the individual institutions, journals and gate-keeping researchers – a judgement call in itself!

**KEY TERMS**

- Academics
- Hypothesis
- Hypothesis development
- Judgement call
- Managers
- Misperceptions
- Paradigm development
- Practice
- Rigour
- Subjective
- Theory
- Theory development
MISPERCEPTIONS, CHALLENGES AND JUDGEMENT CALLS

SUMMARY

- The claims that research is conducted by people who wish to become academics and has little relevance for practice, projects related to practice do not need research rigour, research is isolating and frustrating, research is subjective, the more coverage the study has the better it should be, and that there is always a correct way to conduct each of the steps in research and so the research process can be linear, are all exaggerated misperceptions of what the research process is.
- The choice of research areas and settings, theory and hypotheses development, outcome uncertainty of research efforts, and time constraints are some of the fundamental challenges in the research process.
- Business and management research is relatively new, making consensus hard and paradigm development difficult.
- Referencing, data and findings, listing of variables or constructs, and hypotheses are not theory.
- Theory development is a crucial element for the advancement of our knowledge.
- Hypothesis statements list the relationships between constructs.
- Hypotheses are formulated from research questions, and good practice should be exercised to ensure the hypotheses are formulated and tested in ways that allow you to address the research question.
- A good hypothesis needs to be adequate for its purpose, must be testable, and requires few conditions or assumptions.
- Time constraints can affect the level of rigour expected and achievable in research.
- Judgement calls come with experience.

QUESTIONS

1. Why are there misperceptions of research?
2. Research has little relevance on practice – what are your views? Why?
3. Projects related to practice do not need research rigour – what are your views? Why?
4. Why is research often perceived as subjective?
5. The more variables that are covered in a single study, the higher the level of rigour it should have. Do you agree with this statement? Why or why not?
6. Why is theory development in management and business difficult?
7. Based on your understanding of theory development and theorizing, what is theory in your view?
8. With the help of examples, discuss some potential mistakes that may occur while formulating hypothesis statements.
9. What are judgement calls and how do we deal with them?
10. What are some of the judgement calls in the research process? Briefly discuss.

REFERENCES

RESEARCH DESIGN FOR BUSINESS & MANAGEMENT


ADDITIONAL READING


MISPERCEPTIONS, CHALLENGES AND JUDGEMENT CALLS


