

## Invited Article

### Key Pitfalls in Conducting Research in A Business Discipline: Five Actionable Suggestions

Chaiporn Vithessonthi<sup>1\*</sup>

#### ABSTRACT

Many researchers have gone through several typical academic stages, moving from writing a master's thesis to completing a doctoral dissertation to submitting their manuscripts to scholarly journals for possible publication to getting a tenure (and/or academic promotion) based on their research outputs. In the process, some scholars have had great successes, while others might not realize their full potential. In this article, I identify key weaknesses often observed in many rejected research papers and offer five actionable suggestions to help researchers eliminate such weaknesses and achieve better outcomes.

**Keywords:** Empirical research, Research issues, Theoretical research, Publication, Rejection

#### Introduction

In this article, I will discuss key weaknesses (hereafter “pitfalls”) that I, as a reviewer and an associate editor, have often seen in many manuscripts submitted for publication in several good journals or for presentation at many good conferences. In this article, good journals are defined in a broad sense to include scholarly journals that are ranked as B or better, according to the ABDC Journal List.

As there is a wide range of major pitfalls found in many manuscripts (including some of mine), it is a difficult task to discuss them all in this article. I attempt to discuss five key pitfalls that are highly likely leading to a rejection decision. These pitfalls might seem to be slightly broad in nature.

After presenting these pitfalls, I offer some actionable suggestions that might be able to address (eliminate) these key pitfalls, which will, in turn, increase a chance of getting a paper published in a good journal. Put it differently, these actionable suggestions should be taken as good habits to have when researchers wish to conduct a high-quality research project. Preparing a good research plan prior to commencing a research project will make a journey more enjoyable and productive.

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<sup>1</sup> Department of Finance, School of Business Monash University Malaysia

\* Corresponding Author e-mail: chaiporn.vithessonthi@monash.edu

The rest of the paper is organized as follows. I describe and discuss five key pitfalls in Section 3. I recommend five actionable solutions to mitigate the key pitfalls in Section 3 and conclude the paper in Section 4.

## Five Key Pitfalls

In this section, I present five key pitfalls that might, individually or collectively, lead to a rejection decision by a journal's editor. One or more of these pitfalls are often present in papers that are rejected by either a high-quality conference (e.g., the American Finance Association, the Financial Management Association International, the Academy of Management, the Academy of International Management) or a good journal (e.g., Journal of International Business Studies, Journal of Banking and Finance, etc.).

### Key Pitfall #1: Weak theoretical development

Several papers offer weak theoretical arguments for what they attempt to explain. There are several characteristics that would typically reflect quite poorly on the paper (or the author(s)) with respect to how a paper develops key arguments (or lack thereof) for a prediction. I will list a few examples of these characteristics for illustration purposes.

Many papers describe one, two or three well-known theories (e.g., the resource-based view theory, the transaction-cost theory, the knowledge-based view of the firm, and the pecking-order theory, to name just a few) in the literature review section and then propose their main hypotheses. Oftentimes, we find that the hypotheses proposed in the papers can easily be found in many published papers. Basically, the degree of novelty in the hypotheses is almost zero. Although it is not entirely wrong to propose the same hypotheses again in new studies, there must be some degree of novelty in attempting to explain why such a relationship might exist (or disappear).

There are papers that appear to use a range of several seemingly related theories to support a hypothesis. While this set of papers might seem to offer better theoretical development than the first set of papers discussed above, concepts and/or theories that have been used in this set of papers tend to be highly selective. That is, these papers tend to describe and highlight only certain aspects of the cited theories to support their arguments for a prediction (say, a variable X is positively associated with an outcome Y), although some or all of the cited theories can, in fact, be used to support an alternative prediction (say, X is negatively associated with Y). Hence, a discussion of the theoretical arguments is incomplete.

Increasingly, we also observe that many papers attempt to differentiate their paper from a large body of literature by using either one variable as a mediator or one variable as a moderator, or both. While the use of a mediator variable or a moderator variable is plausible, it is crucial to provide a theoretical justification for such an effect (see e.g., Andersson *et al.* 2014 for a more detailed discussion). For instance, a paper might include an interaction term between X1 and X2 in a model where both X1 and X2 are the main effects. A key question here is which

variable strengthens or weakens the effect of another variable. Does X1 strengthen or weaken the effect of X2 on the outcome variable? Does X2 enhance or attenuate the effect of X1 on the outcome variable? It is quite often that there is a lack of discussion about this theoretical argument.

Papers that introduce one variable as a mediator in a model (say, X2 mediates a relationship between X1 and Y) implicitly (knowingly or unknowingly) indicate that there is at least one endogenous variable (say X1) in a model. In this case, their empirical design should be developed to ensure that the endogeneity issue is well taken care of. Unfortunately, the endogeneity issue might not be mentioned at all in many of these papers; therefore, there is no attempt to address the endogeneity issue (see Pitfall #3 below). As a result, these papers are likely to be rejected for publication.

## Key Pitfalls #2: Sample selection

It might not be entirely wrong to say that business research might be quite international. It is naturally desirable to see similar studies being conducted in many different contexts for several reasons. To keep this article short, I will not discuss such reasons. However, it is fair to say that theories should be tested over time and in different contexts. The ones that stand the test of time will be useful for practitioners (e.g., managers). Nevertheless, studies that simply use a set of data from one country without offering sufficient justification for their choice tend to be viewed less favorably by reviewers, especially papers that have been submitted for publication in an international business journal (see e.g., Tung & Van Witteloostuijn 2008 for a detailed discussion about papers might be sufficiently international).

Given that good journals' spaces are limited, there is strong competition for their publication spaces. Just an example, one can find many papers examine a relationship between financial leverage and firm performance. This leverage-performance relationship has been tested many times as single-country studies and multi-country studies (see e.g., McEvily *et al.* 2004; Cao & Lerner 2009; Giroud *et al.* 2012; Vithessonthi & Tongurai 2015). Likewise, we see a proliferation of studies that examine the relationship between corporate governance and firm performance (see e.g., Core *et al.* 1999; Filatotchev & Toms 2003; Giroud & Mueller 2010; Nini *et al.* 2012; Acharya *et al.* 2013; Huang & Kang 2017; Chen & Keefe 2020). For example, there are many papers that hypothesize and test a relationship between the number (proportion) of independent directors and firm performance. A key question that reviewers would often ask when they review a paper that examines this relationship again is what's new here? Papers that simply examine this relationship using a sample of firms in another country without justifying the need to do so tend to be rejected for publication.

Many papers often just say that due to the mixed results observed in the literature, we revisit and empirically test this relationship again using an international data set (i.e., non-US data). Their sample might typically include observations in a single country, say country A, or in several countries, say countries in Asia. Oftentimes, country A has frequently been used as the setting in prior studies. So, we are now looking at replication studies. While it is desirable to attempt to test

whether the findings still hold in the replication studies, such studies should still have to offer some new insights to advance the body of research. Of course, almost all new papers would usually have a longer sample period. Unfortunately, many of these studies fail to say why or how their study will resolve the mixed results. Examining (almost) the same set of firms using a slightly longer period of time will less likely change the results, all else being equal. Hence, it is important to discuss how, for example, the results might depend on the context. Identifying some characteristics that can explain the different results and test whether such characteristics, in fact, contribute to the different results would increase the likelihood that reviewers would be more receptive to agreeing that the paper under review has some kinds of novelty.

### Key Pitfalls #3: Flawed empirical design

Many papers have been rejected due to their flawed empirical design. Again, I cannot cover all the flaws here. However, I attempt to identify a few recurrent issues. One of empirical design flaws is a sample selection, which has already been discussed as Key Pitfall #2. In this subsection, I focus on the endogeneity issue and how a certain choice of an empirical analysis cannot address the endogeneity issue. As a consequence, a paper is very weak in terms of empirical methods.

Many papers still use a cross-sectional data set just out of convenience. It should be clear to almost every researcher that research in social sciences (e.g., in business disciplines) typically involve with non-laboratory tests unlike other disciplines such as pharmacology, chemistry, engineering, etc. Hence, there are many omitted and/or unobservable factors that might affect the studies' outcome variable. For example, many papers in marketing and management use individuals (e.g., managers, employees, buyers) as their unit of analysis (e.g., studying CEOs' decisions, consumers' perceptions, etc.). Many of these papers conduct a questionnaire survey to collect data that are typically cross-section in nature. Then, they conduct their empirical analysis using an ordinary least squares (OLS) regression using the cross-sectional sample. Likewise, many papers in finance might use firms as their unit of analysis (e.g., studying the firm's leverage).

We all know that endogeneity issues are great concerns (see e.g., Coles *et al.* 2012; Reeb *et al.* 2012; Roberts & Whited 2013). For instance, there are omitted variables (e.g., observable and unobservable factors) that affect an outcome variable. For example, individuals' unobservable characteristics might affect their decisions, while firms' unobservable characteristics might affect firms' actions. These unobservable factors might be time-invariant or time-varying. Results obtained from a cross-sectional OLS regression would suffer from the presence of the endogeneity issues. Hence, the results are unreliable. It must be noted that there are papers that use a panel data set to analyze a similar phenomenon by estimating a panel OLS regression with fixed effects or an instrumental-variable two-stage least squares (IV-TSLS) regression to mitigate the endogeneity concerns (see e.g., Vithessonthi 2016; Grieser & Hadlock 2019; Mogstad *et al.* 2021). Even papers that use the IV-TSLS regressions have often been rejected for publication. In such

situation, papers that simply use a cross-sectional data set and an OLS regression stand (almost) no chance at getting published in good journals.

#### **Key Pitfalls #4: Describing, rather than discussing, the results.**

We often see papers that might have a good theoretical development section and a good research design but unfortunately fails short at almost the last hurdle by only describing the results rather than discussing the results. I think that authors of these papers simply shot themselves in the foot. A detailed discussion of the results can make a big difference between getting a rejection decision or an RR (revised and resubmit) decision. A paper that has a good discussion might help better position itself in a large body of research. That is, it clearly emphasizes their key contribution and illustrate what is new in the paper (see e.g., Ireland 2009 for a more detailed discussion).

A recurring theme among many papers is that they present their results by saying whether the coefficients are statistically significant. They might also discuss whether their control variables are significant. However, they do not emphasize whether the results are economically significant. They might not discuss whether or under what conditions the results might change. They might not discuss whether and/or how the results differ from prior studies. A bottom line here is that they just report the results, but they don't discuss the results.

There are papers that do not attempt to test the robustness of their results. For example, many papers use only one proxy to measure one construct although such a construct can be measured in a number of ways. Then, is it possible that the reported finding is sensitive to the measure of the construct? Would the result still hold when using other measures of the construct? Without testing this possibility, the results might not be reliable. Therefore, these papers might not receive a favorable outcome.

We often see papers that do not rule out any alternative explanation for the finding. For instance, a paper reports that a variable X1 is positively associated with an outcome variable Y. There might be, say, three concepts or theories that predict a positive relationship between X1 and Y. These theories might employ a different mechanism to propose the positive relationship between X1 and Y. Hence, it is important to illustrate which mechanisms might actually explain the existence of the positive relationship between X1 and Y. Papers that can empirically rule out as many alternative explanations and/or theories as possible will offer better insights (see e.g., Hsieh & Moretti 2003; Atalay *et al.* 2014; Aktas *et al.* 2018; Boons & Prado 2019). Therefore, such papers have a better chance of getting published.

#### **Key Pitfalls #5: Poor writing**

Although poor writing (e.g., many typos, grammatical errors, etc.) should not often occur, it does! There are many good papers (e.g., those without the four mentioned pitfalls) that are simply so difficult to read. Of course, there are many poor-quality papers (e.g., papers that have two or more of the four mentioned pitfalls) that are badly written. Both groups of papers might

be rejected because reviewers and editors find it hard to proceed further with the paper with such poor writing.

Many papers might contain so many typos, poor choices of words, many grammatical errors. Such papers would naturally cast doubt in the mind of reviewers and editors whether the content in the paper is correct and reliable. For example, when there are several contradictory statements in a paper, a question arises as to which sentences are correct. When there are so many doubtful statements, reviewers would most likely recommend a rejection. This pitfall is perhaps at times the easiest one to fix (at least among the five key pitfalls being discussed so far). So, authors should try their best to avoid this pitfall. Sometimes it is not about the proficiency of English, but rather about the degree of dedication or devotion to ensuring that the paper is free of typos and grammatical errors.

## Five Actionable Suggestions

In this section I present five actionable suggestions to reduce the existence of the five pitfalls have been discussed in Section 3.

Suggestion #1: Ensure that a paper has a strong theoretical foundation. Ask yourself what is new in your paper. If you can't find one, then the paper is still not ready. Keep looking, don't settle (remembering the wise words from Steve Jobs).

Suggestion #2: Choose an appropriate sample that is consistent with the latest published papers on the topic. Your paper has to be better than the old ones.

Suggestion #3: Develop a research design that meets the latest standards used in the literature.

Suggestion #4: Discuss the results extensively and offering at least one key insight. Present the paper in a workshop or seminar series to test the water and get feedback.

Suggestion #5: Write, rewrite and revise the paper until it is extremely ready. There is no shortcut.

## Conclusion

In this article, I articulate five key pitfalls that often contribute to a rejection decision. I present some characteristics or descriptions of these pitfalls as well as offer some suggestions for minimizing or alleviating these pitfalls. Given the space limitation, I recommend readers who find my suggestions useful refer to other sources (e.g., books and articles) for more detailed explanations and suggestions. By eliminating the five key pitfalls, among other minor pitfalls, you will have a better chance of getting a paper published in a good journal. I wish you all the best.

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