

## RESEARCH NOTES AND COMMENTARIES

### WHAT KIND OF ASSUMPTIONS NEED TO BE REALISTIC AND HOW TO TEST THEM: A RESPONSE TO TSANG (2006)

SHUN YIN LAM\*

Marketing and International Business Division, Nanyang Business School, Nanyang Technological University, Singapore

*Tsang (2006) contends that certain core assumptions of a theory, which are typically about people's behaviors or thoughts, need be realistic, because they determine the viability of the mechanism that generates a hypothesized relationship. While Tsang's (2006) article rightly emphasizes the importance of realistic assumptions, it neglects the issue that certain kinds of assumptions are necessarily unrealistic for the roles that they play in theory development and testing. Therefore, researchers should not be criticized for making unrealistic assumptions of the latter kinds. Furthermore, by deliberating on the assumptions underpinning a theory, researchers can construct theories with better explanatory power and further develop existing theories. Tsang (2006) also suggests two approaches for testing assumptions, namely, a structural model and direct inquiry approaches. Although these approaches have certain merits, they also have limitations that may render the evidence gathered unreliable under certain situations. Two alternative approaches, namely, the experimental-causal-chain and the moderator-of-process designs, address these limitations. The researcher could consider adopting these designs as well in order to improve the rigor of assumption testing. Copyright © 2010 John Wiley & Sons, Ltd.*

## INTRODUCTION

Tsang (2006: 1002) maintains that core assumptions of a theory need be realistic because 'an unrealistic core assumption will lead to an unrealistic mechanistic explanation and thus a defective theory.' Furthermore, Tsang (2006) posits two

approaches for checking whether a core behavioral assumption is realistic. Tsang's (2006) call for greater attention to assumptions is commendable as researchers can construct theories with better explanatory power and further develop existing theories by deliberating on the underlying assumptions. Moreover, the realism issue has practical implications for researchers in writing papers for publication. For example, if journal reviewers agree that a particular assumption need not be realistic, then authors will not need to spend time on justifying its realism during the review process.

Keywords: core assumptions; behavioral assumptions; realism; assumption testing; mediation; process analysis

\*Correspondence to: Shun Yin Lam, Marketing and International Business Division, Nanyang Business School, Nanyang Technological University, Nanyang Avenue, Singapore 639798. E-mail: asylam@ntu.edu.sg

While Tsang (2006) discusses why certain core assumptions must be realistic, the current research article articulates why other assumptions need not be realistic. Musgrave (1981) makes a distinction among three different kinds of assumptions—negligibility, domain, and heuristic. Negligibility and heuristic assumptions describe simplifications and idealized cases of the real world, respectively. Although they are unrealistic, they are often maintained for their roles in theory development—for example, allowing researchers to concentrate on developing the focal relationships of their theories and paving the way for theory elaboration. Therefore, researchers need to realize the roles of assumptions that are necessarily unrealistic. In addition, the approaches that Tsang (2006) suggests for assumption testing have limitations that may render the evidence generated unreliable under certain situations. Nonetheless, these limitations could be addressed by other approaches. Thus, in short, the current article's objectives are to: (a) alleviate concerns over assumptions that are necessarily unrealistic; (b) focus the researcher's attention on the realism of behavioral assumptions; and (c) enable the researcher to make more informed choices of assumption testing approaches. To begin with, this article elaborates on the meaning of realistic assumptions and various kinds of assumptions constituting a theory, with particular emphasis on whether they need be realistic or not.

## REALISTIC ASSUMPTIONS AND KINDS OF ASSUMPTIONS

### Realistic assumptions

The core argument in Friedman's (1953) famous essay, *The Methodology of Positive Economics* is that as long as a theory yields sufficiently accurate predictions, whether its assumptions are realistic should not be a cause for concern. There are at least three different senses in which assumptions may be considered realistic (Nagel, 1963). First, a statement is said to be unrealistic if it fails to provide an exhaustive description of an object under consideration. This sense is trivial because no finitely long statement can describe all the traits of the object. The second sense is that a statement is believed to be either false or highly improbable on the basis of the available evidence.

Finally, many theories posit idealized conditions that allow the researcher to conceptualize relations of dependence between actual phenomena but do not actually exist. For instance, in economics, price elasticity is formulated in terms of decisions made by perfectly rational individuals on goods that are infinitely divisible. Since assumptions are *necessarily* unrealistic according to the first and the last meanings of 'realistic,' the current article is more concerned with the second meaning—that is, whether an assumption is false or highly improbable.

### Kinds of assumptions

Samuelson (1963: 232) coins the term 'F-twist' to represent Friedman's (1953) arguments that unrealistic assumptions are actually desirable. To untwist the F-twist, Musgrave (1981) makes a distinction among three different kinds of assumptions—negligibility, domain, and heuristic—which are subsequently elaborated upon and further developed by Mäki (2000). A negligibility assumption is 'the hypothesis that some factor  $F$  that might be expected to affect the phenomenon under investigation actually has an effect upon it small enough to be neglected relative to a given purpose' (Mäki, 2000: 322). Negligibility assumptions enable researchers to focus on the key causal factors postulated by a theory. For example, in examining the motion of bodies falling through short distances, Galileo posited that air resistance would have no (or only a negligible) effect on such motion. Assuming away air resistance focuses attention on the effect of gravitational force under the condition of a vacuum in testing Galileo's theory. In the area of organizational learning research, Argyris asserts that organizational defensive routines identified by his research on U.S. companies 'is generic to all human organizations' (Argyris, 1990: 63). By making such a bold generalization, Argyris (1990) implicitly assumes that the emergence of defensive routines has no or negligible relationship with the nature of an organization. Although a negligibility assumption is unrealistic in that it disregards certain factors in the real world, it is justified when the theory containing it generates results as predicted. Since the results of Galileo's experiments were consistent with the predictions of his theory about bodies freely falling in a vacuum, his assumption of negligible air resistance was warranted.

A domain assumption is 'a hypothesis concerned with the domain of applicability of a theory; it is the statement that theory *T* applies only if factor *F* is absent' (Mäki, 2000: 323). A domain assumption has an important role in theory development because it appropriately specifies the boundary conditions of applicability of a theory. There is a close relationship between negligibility and domain assumptions—when a negligibility assumption is refuted, it may become a domain assumption. Suppose in the foregoing Argyris's (1990) study of defensive routines, a researcher replicates the study using a sample of devoted religious sects. The researcher fails to identify any defensive mechanism because the key objective of enacting defensive routines in organizations, as Argyris (1990) reasons, is face-saving, whereas for members of such sects who are bonded together by religious zeal, face-saving is not an important personal concern. If this empirical finding is confirmed by further research studies, Argyris's (1990) negligibility assumption may need to be replaced by the domain assumption that this theory is not applicable to religious sects.

Now consider Musgrave's (1981) third category of assumptions, which is called heuristic assumptions. These assumptions must be understood in a dynamic context: they are first made and subsequently abandoned. As Musgrave states: 'Heuristic assumptions play an important role in developing any theory whose logico-mathematical machinery is so complicated that a *method of successive approximation* has to be used' (1981: 383, emphasis in original). A good example is transaction costs theory. Williamson's (1975) initially constructed theory focused only on the governance choice between markets and hierarchies, but neglected intermediate governance forms. This theory, though insightful, failed to explain the various forms of strategic alliances that began to flourish in the early 1980s. By relaxing the assumption of no intermediate governance forms, Williamson (1991) further developed transaction costs theory to include hybrid modes, such as equity joint ventures. By their very nature, heuristic assumptions are unrealistic as they are too restrictive to represent the real world. Empirical findings of research that is based on heuristic assumptions generally fail to generate precise predictions. Yet heuristic assumptions constitute an inevitable step toward a more refined theory.

Recently, Hindriks develops a bold argument that: '*we should not fault a theory for its unrealistic assumptions if it has a lot of explanatory power*' (Hindriks, 2008: 336 emphasis in original). Hindriks (2008) posits that the explanatory power of a theory consists of three elements, namely explanatory scope, depth, and breadth. Explanatory scope concerns the number of phenomena that a theory can explain, depth the amount of detail a theory provides about the causal process underpinning the phenomenon at issue, and breadth the number of answers a theory provides to an explanatory question. Hindriks (2008) illustrates this view through the use of the Modigliani-Miller (1958) theorem in finance. This theorem is based on a set of heuristic assumptions: for example, there are no taxes and no bankruptcy costs; individual investors can borrow at the same rate as firms; and so on. These idealizations enable Modigliani and Miller (1958) to elucidate the basic mechanism underpinning a range of phenomena, and to provide several answers to a particular explanatory question. Therefore, the Modigliani and Miller (1958) theorem provides substantial explanatory power. Furthermore, the theorem has heuristic value for subsequent inquiry by encouraging researchers to consider what would happen if certain assumptions are relaxed. Thus, despite the unrealistic assumptions, the Modigliani-Miller (1958) theorem generates a great deal of insights into the financial structure of a firm in the real world, and financial economists regard it as a cornerstone of corporate finance (Tirole, 2006).

To summarize, contrary to Tsang's (2006) view, certain kinds of assumptions do make important contributions to theory development and testing, and hence are maintained although they are obviously unrealistic. On the other hand, some kinds of assumptions need be realistic because whether they are realistic determines the viability of the basic mechanism that management researchers use to justify their predictions. These are typically 'behavioral assumptions' (Tsang, 2006). For example, transaction costs theory assumes that some individuals will, if left unchecked, behave opportunistically (Williamson, 1975), and the Modigliani-Miller (1958) theorem presupposes investors to engage in arbitrage behavior to maximize their profits. If these behavioral assumptions do not reasonably correspond to people's behavior in real life, it is typically doubtful that

researchers can draw accurate predictions of real-world events from the theory concerned. Although at times empirical evidence corroborates predictions based on unrealistic behavioral assumptions, one may wonder whether this consistency happens by chance or is merely a spurious result in data analysis, and whether the assumptions can stand the test of time in yielding other accurate predictions when more phenomena are considered. In addition, the theory based on the assumptions fails in its explanatory role because the relationship that the theory purports to explain is not attributed to the mechanism implied by the assumptions. Furthermore, unrealistic behavioral assumptions could yield faulty implications for practice even though they are supported by empirical evidence.

Friedman describes another way of formulating assumptions, namely the as-if formulation, by using an example: 'under a wide range of circumstances individual firms behave *as if* they were seeking rationally to maximize their expected returns' (1953: 21, emphasis in original). Mäki (2000) shows that this type of assumption can be converted into a core assumption or a peripheral assumption. Core assumptions are about the major causes postulated by a theory. Typically, they concern people's or firms' behavior and hence are also referred to as behavioral assumptions by Tsang (2006). In contrast, peripheral assumptions are about the minor causes of the phenomena under study, and include negligibility and heuristic assumptions. Thus, whether an as-if formulation has to be realistic depends on the nature of the assumption that it is associated with. Given the importance of testing behavioral (core) assumptions, then naturally one would like to know how to test them.

## TESTING BEHAVIORAL ASSUMPTIONS

Tsang (2006) posits two ways of directly testing behavioral assumptions that are central to management theories. The first way constitutes a kind of process or mediation analysis based on a structural model formulation (Tsang, 2006: 1002). The second way is a direct inquiry method whereby the researcher attempts to obtain managers' reports on their decisions rationale (Tsang, 2006). Referring to transaction costs theory, Tsang (2006: 1008) suggests that 'in-depth, unstructured interviews with managers who make transaction cost related

decisions are particularly useful for collecting data about the key factors affecting their decisions. . . . Such data provide a useful preliminary check on the realism of behavioral assumptions.' In proposing the foregoing approaches, Tsang (2006) seems to presume that assumptions are directly testable. Testability of assumptions is a contentious issue. For instance, Machlup (1955, 1956) maintains that fundamental assumptions, which are similar in meaning to Mäki's (2000) core assumptions, are not directly testable. On the other hand, Hutchison (1956) challenges that Machlup (1955) has failed to explain why it would be more difficult to test fundamental assumptions than to test the hypotheses deduced from them. In this connection, Blaug notes that: 'direct evidence about assumptions is not necessarily more difficult to obtain than data about market behavior used to test predictions' (Blaug, 1992: 96). Apart from this testability issue, the current article also comments on other aspects of the approaches advocated by Tsang (2006), and put forward other ways of conducting process analysis.

## Process (mediation) analysis

In process (mediation) analysis, the researcher attempts to generate statistical evidence for the mediating process in which the behavioral assumptions are involved (Miller *et al.*, 2007; Wood *et al.*, 2008). Without loss of generality, I use an example with one mediator only to explain process analysis. A hypothesized effect of a predictor (X) on an outcome (Y) could be explained by delineating the process through which X affects Y. This delineation requires the researcher to explicitly put forward a variable that mediates the hypothesized effect, that is, a mediator (M), which is usually derived from the behavioral assumption being tested. For example, a mediator in the case of transaction costs theory would be a manager's perception about the likelihood that a supplier may engage in opportunistic behavior. In process analysis, the researcher attempts to provide empirical support for the causal chain  $X \rightarrow M \rightarrow Y$  through statistical analysis of relevant data, as Figure 1a indicates. Under the structural model approach that Tsang (2006) posits, the researcher would estimate the relationships between the predictor (X) and the mediator (M), and between the mediator (M) and the outcome (Y), simultaneously. This approach requires measurement of the mediator and thus

belongs to the *measurement-of-mediation* design for performing process analysis (Spencer, Zanna, and Fong, 2005).

Under the measurement-of-mediation design, the researcher would collect data about the variables involved in the mediation process (X, M, and Y), including measurement of the mediator (M). In addition to the causal chain,  $X \rightarrow M \rightarrow Y$ , which is known as the indirect effect of X on Y, the researcher would usually investigate the direct effect of the predictor (X) on the outcome (Y) as well (Figure 1a). Thus, the structural model approach is a special case of the measurement-of-mediation design as the former concerns the causal chain only. The direct effect represents an effect of X on Y *not captured* by the process being tested. It concerns another mechanism that also contributes to the effect of X on Y but is not implied by the behavioral assumption. Therefore, the design could provide more insights than the structural model approach.

Furthermore, the examination of the direct and the indirect effects can still serve a useful purpose even if the observed relationship between the predictor and the outcome is nonsignificant. Nonetheless, Tsang (2006: 1008) states that: 'If the relationship is as posited, then the assumption is examined and tested; if not, we can stop right

there.' The current article contends that the generating mechanism described by the theory may still exist even if no evidence is found for the relationship. Consider a hypothetical case illustrated by Figure 1b. Suppose a researcher proposes that the number of hotels in a market (the predictor) negatively affects a hotel operator's intention to enter the market (the outcome), because the operator may think that the more hotels there are, the more intense the competition for customers will be in the market. In this case, the predicted relationship is based on the assumption about competition. Next, suppose the researcher finds that the observed relationship between the number of hotels and the market entry intention is nonsignificant. According to Tsang (2006), this result suggests that the competition mechanism does not exist. However, by adopting the measurement-of-mediation design, the researcher gathers support for a negative indirect effect of the number of hotels on the entry intention, as Figure 1b indicates. Therefore, the mechanism is supported. In addition, the researcher also finds a positive direct effect of the number of hotels on the market entry intention. The indirect and direct effects cancel each other out, thus explaining the lack of support for the relationship between the predictor and the outcome. Furthermore, the identification of the positive direct effect leads the researcher to look for any agglomeration benefit in a market, such as heightened consumer demand (Kalnins and Chung, 2004). Thus, this example illustrates that the measurement-of-mediation design could yield counterintuitive insights.

Despite the foregoing merits, the measurement-of-mediation design has some limitations because it requires measurement of the variables involved in the mediation. As Tsang (2006) notes, often the mediator concerns a person's internal state, including perceptions, attitudes, and beliefs. As the internal state cannot be observed directly, researchers must rely on respondents' self-report of it (Wood *et al.*, 2008). Self-report measures of the internal state (such as attitudes) are susceptible to various kinds of biases (Schwarz, Groves, and Schuman, 1998). For example, transaction costs theory assumes that individuals consider some people to be opportunistic. Respondents of a survey may be concerned about social desirability and evaluation apprehension when verbalizing such a thought because they may not want to speak ill of other people. Furthermore, many studies in management

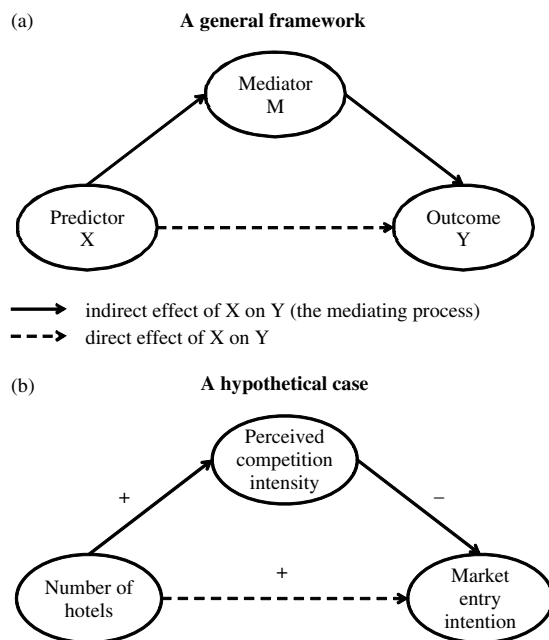


Figure 1. Illustrations of process analysis

research use survey data that are all collected from a single source at the same time (Wood *et al.*, 2008). Common method bias is an issue for these research studies and the inclusion of the mediator's measure in a survey aggravates this bias. Many researchers have discussed how such bias could arise (e.g., Feldman and Lynch, 1988; Podsakoff *et al.*, 2003). For example, the measurement itself may interfere with a respondent's thought, thus causing the respondent's answer to one question to exert undue influence on his/her response to a subsequent question. To address the foregoing limitations, Spencer *et al.* (2005) propose two alternatives of conducting process analysis, namely, the *experimental-causal-chain* and the *moderator-of-process* designs.

Under the experimental-causal-chain design, the researcher would attempt to investigate the two parts of the focal process ( $X \rightarrow M$  and  $M \rightarrow Y$ ) in two studies, respectively. This also means that the researcher would examine the two parts by using two different samples from the same target population. The confirmation of the underlying process ( $X \rightarrow M \rightarrow Y$ ) is based on the combined evidence collected from the two studies. Although experimental designs have been under-represented in strategy research, Croson, Anand, and Agarwal (2007) highlight its great potential in advancing the strategy field. By examining the two parts of the underpinning process separately, the experimental-causal-chain design makes it possible for the researcher to manipulate the predictor in one study and the mediator in another. The researcher could then treat the predictor and the mediator as manipulated variables (i.e., categorical variables representing the manipulations) in the data analysis. Since measurement of the predictor and the mediator is not necessary under this circumstance, common method bias is not an issue. If the research setting does not permit the researcher to manipulate the predictor and the mediator, the researcher could still consider examining the two parts of the underlying process separately. Such separate investigations could still reduce the threat of common method bias.

Under the moderator-of-process design, the researcher attempts to identify or create a special situation in which the focal process pertaining to the behavioral assumption could be forestalled. The researcher expects that if the assumption really accounts for the relationship between the predictor and the outcome, the relationship could be

observed in normal circumstances but not under the special situation. This contrast in the observed relationship serves as evidence for the assumption. The moderator-of-process design thus has an advantage in that it does not require the inclusion of the mediator in the data analysis, and therefore is not inflicted by the problems associated with the mediator's measurement. This advantage is particularly valuable when the mediator, such as opportunism, is characterized by both measurement and state unobservability (Godfrey and Hill, 1995).

Consider an example from transaction costs economics research. Zaheer, McEvily, and Perrone (1998: 144) state that: 'Trust reduces the inclination to guard against opportunistic behavior.' Therefore, a high level of trust could forestall or divert thoughts about opportunistic behavior. To test the behavioral assumption about opportunism, the researcher may carry out an experiment involving a governance choice task. Following the moderator-of-process design, the researcher may create two settings and randomly assign participants to each of them. The first setting would simulate a common business environment, where opportunistic behavior is possible. The second setting would be a special environment that promotes trust between participants and/or has a strong sanction against opportunistic behavior. In the latter environment, individuals are unlikely to think about other participants' likelihood to engage in opportunistic behaviors. In each setting, the researcher manipulates the level of asset specificity (the predictor) and observes the choice of governance mode (the outcome) made by the participants. If the effect of opportunism operates as transaction costs theory proposes, the relationship between asset specificity and governance choice should be more in line with the transaction costs theory prediction in the first than in the second setting. Although it would be difficult to conduct a field study of this kind, it is not impossible. For instance, Dyer and Chu's (2003) study of supplier-automaker exchange relationships found that supplier trust was significantly higher in Japan than in Korea or the United States. With sufficient diligence, a quasi-experiment can be designed based on this difference.

In sum, while the measurement-of-mediation design dominates process analysis in strategic management research (Miller *et al.*, 2007), management researchers may consider the experi-

mental-causal-chain and the moderator-of-process designs as alternatives that could address the measurement problems associated with the former design. Nonetheless, the measurement-of-mediation design will be the only viable alternative when the research setting does not allow the researcher to manipulate the predictor and the mediator and to collect data about the variables concerned separately from different samples. Furthermore, this design allows the researcher to check the data for the possibility of any unexpected effect of the predictor on the outcome. Therefore, the choice of the designs is also affected by the constraints of the research setting and the research objectives, and management researchers could strengthen the support for their assumptions by gathering convergent evidence from different designs. While these designs utilize statistical results as evidence for the underpinning process, Tsang (2006) also suggests direct inquiry into managers' rationale of their decisions for generating qualitative evidence for the process. However, findings from the psychology and human decision making literature cast doubt on the reliability of these subjective reports.

### Direct inquiry

Many research studies in psychology and human decision making suggest that people may not have direct introspective access to their cognitive processes, such as the processes involved in judgment, problem solving, and the initiation of behavior (e.g., Chartrand and Bargh, 2002; Latané and Darley, 1970). Some of these studies—for example, Nisbett and Bellows's (1977) study on hiring decisions—pertain to people's decision making in the workplace. Nisbett and Wilson (1977) review these research studies and conclude that people often cannot report accurately on the effects of particular stimuli on their responses.

The foregoing evidence raises concern over the reliability of managers' reports on their decisions rationale. Although managers can sometimes provide assertions about the rationale, they may not do so on the basis of accurate introspection. Instead, as Nisbett and Wilson (1977) propose, they may base their reports on *a priori*, implicit causal theories, or judgments about the extent to which particular stimuli in their environment are plausible causes of a given response. In contrast, White (1980) attributes the inaccuracy of these

reports to respondents' inadequate attention to the cognitive processes or the difficulty of recalling them. For some routine or very simple judgment tasks, the cognitive processes leading to the judgment could be so short that they escape people's notice; whereas for very complex tasks, information present in the task environment captures so much attention that little attention could be paid to internal events in people's minds. Furthermore, people may find it difficult to recall the processes if they are complicated, or the lapse between processes and people's reporting of them is long. Accordingly, White (1980) predicts that accuracy of subjective reports about the processes could be improved if the judgment task is neither too simple nor complex, and the lapse is shortened. This prediction was supported by an experiment that White (1980) conducted.

To summarize, the researcher needs to take circumstances into account when using the direct inquiry reports as evidence in testing behavioral assumptions. The accuracy of the reports would vary with the characteristics of the decisions and the time when the reports are made. Also, the accuracy may improve if managers are encouraged to pay attention to their cognitive processes by telling them in advance that they will be required to report on their decisions rationale.

### CONCLUSION

Friedman's (1953) essay sparked a series of debates about assumptions in economics during subsequent decades. Although the arguments in his essay are complex and sometimes ambiguous, there is a general consensus that his position is a kind of methodological instrumentalism (Caldwell, 1980). Despite the fact that many of the comments on Friedman's (1953) essay within the extant research literature have been negative, some scholars did support it (Boland, 1979; Machlup, 1955). After more than half a century, the debate has not died down. Most recently, Shugan's editorial essay within *Marketing Science* reinstates the position of methodological instrumentalism and restates the 'F-twist' that unrealistic assumptions 'breed great new theories' (2007: 449). Tsang (2006) brings this important methodological issue to the attention of strategy researchers.

Strategy research would benefit from good (methodological, philosophical, and theoretical)

conversation (Mahoney, 1993). The current article's commentary represents a contribution to this conversation by supplementing Tsang's (2006) paper in at least two respects. First, it clarifies the meaning of 'realistic' and elaborates on different kinds of assumptions, some of which Tsang (2006) has not discussed. The current article contends that certain assumptions are intended to be unrealistic so as to enable the researcher to formulate and articulate the basic mechanism of a theory, pave the way for theory development, and test the theory in its early stage of development. Thus, researchers should not be criticized for making them. However, consistent with Tsang (2006), the current article holds that behavioral assumptions must be realistic because they govern the basic mechanism described by a theory and unrealistic behavioral assumptions could lead to faulty implications for practice. Second, the current article maintains that testing behavioral assumptions can be done in several ways. This article suggests a broader range of testing approaches than does Tsang (2006), and discusses circumstances in which they are applicable. This analysis could enable a better choice between them in strategic management research.

## ACKNOWLEDGEMENTS

I thank Joseph Mahoney (associate editor) and two anonymous reviewers for their valuable comments.

## REFERENCES

- Argyris C. 1990. *Overcoming Organizational Defenses: Facilitating Organizational Learning*. Simon & Schuster: Needham Heights, MA.
- Blaug M. 1992. *The Methodology of Economics: Or How Economists Explain* (2nd edn). Cambridge University Press: Cambridge, UK.
- Boland LA. 1979. A critique of Friedman's critics. *Journal of Economic Literature* **17**(2): 503–522.
- Caldwell BJ. 1980. A critique of Friedman's methodological instrumentalism. *Southern Economic Journal* **47**(2): 366–374.
- Chartrand TL, Bargh JA. 2002. Nonconscious motivations: their activation, operation, and consequences. In *Self and Motivation: Emerging Psychological Perspectives*, Tesser A, Stapel DA, Wood JV (eds). American Psychological Association: Washington, DC; 13–41.
- Croson R, Anand J, Agarwal R. 2007. Using experiments in corporate strategy research. *European Management Review* **4**(3): 173–181.
- Dyer JH, Chu W. 2003. The role of trustworthiness in reducing transaction costs and improving performance: empirical evidence from the United States, Japan, and Korea. *Organization Science* **14**(1): 57–68.
- Feldman JM, Lynch JG Jr. 1988. Self-generated validity and other effects of measurement on belief, attitude, intention, and behavior. *Journal of Applied Psychology* **73**(3): 421–435.
- Friedman M. 1953. *Essays in Positive Economics*. University of Chicago Press: Chicago, IL.
- Godfrey PC, Hill CWL. 1995. The problem of unobservables in strategic management research. *Strategic Management Journal* **16**(7): 519–533.
- Hindriks F. 2008. False models as explanatory engines. *Philosophy of the Social Sciences* **38**(3): 334–360.
- Hutchison TW. 1956. Professor Machlup on verification in economics. *Southern Economic Journal* **22**(4): 476–483.
- Kalnins A, Chung W. 2004. Resource-seeking agglomeration: a study of market entry in the lodging industry. *Strategic Management Journal* **25**(7): 689–699.
- Latané B, Darley JM. 1970. *The Unresponsive Bystander: Why Doesn't He Help?* Appleton-Century-Crofts: New York.
- Machlup F. 1955. The problem of verification in economics. *Southern Economic Journal* **22**(1): 1–21.
- Machlup F. 1956. Rejoinder to a reluctant ultra-empiricist. *Southern Economic Journal* **22**(4): 483–493.
- Mahoney JT. 1993. Strategic management and determinism: sustaining the conversation. *Journal of Management Studies* **30**(1): 173–191.
- Mäki U. 2000. Kinds of assumptions and their truth: shaking an untwisted F-twist. *Kyklos* **53**(3): 317–336.
- Miller TL, del Carmen Triana M, Reutzel CR, Certo ST. 2007. Mediation in strategic management research: conceptual beginning, current application, and future recommendations. *Research Methodology in Strategy and Management* **4**: 295–318.
- Modigliani F, Miller M. 1958. The cost of capital, corporation finance, and the theory of investment. *American Economic Review* **48**(3): 261–297.
- Musgrave A. 1981. 'Unreal assumptions' in economic theory: the F-twist untwisted. *Kyklos* **34**(3): 377–387.
- Nagel E. 1963. Assumptions in economic theory. *American Economic Review* **53**(2): 211–219.
- Nisbett RE, Bellows N. 1977. Verbal reports about causal influences on social judgments: private access versus public theories. *Journal of Personality and Social Psychology* **35**(9): 613–624.
- Nisbett RE, Wilson TD. 1977. Telling more than we can know: verbal reports on mental processes. *Psychological Review* **84**(3): 231–259.
- Podsakoff PM, MacKenzie SB, Lee J-Y, Podsakoff NP. 2003. Common method biases in behavioral research: a critical review of the literature and recommended remedies. *Journal of Applied Psychology* **88**(5): 879–903.
- Samuelson PA. 1963. Discussion. *American Economic Review* **53**(2): 231–236.
- Schwarz N, Groves RM, Schuman H. 1998. Survey methods. In *The Handbook of Social Psychology*



- (Vol. 1) (4th edn)., Gilbert DT, Fiske ST, Lindzey G (eds). McGraw-Hill: Boston, MA; 143–179.
- Shugan SM. 2007. It's the findings, stupid, not the assumptions. *Marketing Science* **26**(4): 449–459.
- Spencer SJ, Zanna MP, Fong GT. 2005. Establishing a causal chain: why experiments are often more effective than mediational analyses in examining psychological processes. *Journal of Personality and Social Psychology* **89**(6): 845–851.
- Tirole J. 2006. *The Theory of Corporate Financing*. Princeton University Press: Princeton, NJ.
- Tsang EWK. 2006. Behavioral assumptions and theory development: the case of transaction cost economics. *Strategic Management Journal* **27**(11): 999–1011.
- White P. 1980. Limitations on verbal reports of internal events: a refutation of Nisbett and Wilson and of Bem. *Psychological Review* **87**(1): 105–112.
- Williamson OE. 1975. *Markets and Hierarchies: Analysis and Antitrust Implications*. Free Press: New York.
- Williamson OE. 1991. Comparative economic organization: the analysis of discrete structural alternatives. *Administrative Science Quarterly* **36**(2): 269–296.
- Wood RE, Goodman JS, Beckmann N, Cook A. 2008. Mediation testing in management research. *Organizational Research Methods* **11**(2): 270–295.
- Zaheer A, McEvily B, Perrone V. 1998. Does trust matter? Exploring the effects of interorganizational and interpersonal trust on performance. *Organization Science* **9**(2): 141–159.