A Vision of Theory, Research, and the Future of Business Schools

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The authors have distinguished between two broad types of research: studies dedicated to development and testing of theoretical explanations and studies seeking to generalize observed effects to settings of interest. Winer (1999 [this issue]) argues that the goal of making research conducted by marketing academics more relevant can be served by coupling theory studies with effects studies. Outcomes observed in the former type of study would gain external validity from the latter. While this notion has much intuitive appeal, the authors believe it is flawed. Generalizing the effects obtained in theory testing research by conducting additional effects studies using realistic data, such as scanner data, fails to take into account that theoretical explanation is not inherent in any set of data. An alternative view is offered in this article in which theoretical explanation serves as the basis for solving real-world problems and is the appropriate focus of business school education.

Many marketing academics and practitioners would echo Winer's (1999 [this issue]) call that "as we move into the twenty-first century, the current state of business schools in which marketing academics reside will require more and more that our research is not only of high quality but relevant" (p. 349). Most would agree because they see business schools all too often as ivory towers turning out research that appears out of touch with the real world of everyday business decisions. Winer seeks to be more specific about the problem and to offer concrete recommendations for making research more relevant.

Winer (1999) follows Wells (1993), Ferber (1977), and others in placing the problem squarely on the concern of some researchers for internal validity at the expense of external validity. In his view, research may be of high quality in that the outcomes are attributable to experimental variables, but relevance is in doubt because the research variables and context do not correspond to some real-world setting of interest. Winer, unlike others who have argued along these lines, does not claim that research that focuses on internal validity is without merit. His vision of twenty-first-century research is simply one that builds on such research to add external validity. He proposes that researchers concerned with internal validity be required to include a section in their papers indicating in specific terms the type of research needed to achieve external validity. Furthermore, researchers ought to be encouraged to work on such studies with marketing scientists using scanner panel data. The vision is one in which studies of "18- to 22-year-olds at large midwestern universities" are not left to languish in the journals. In the future, researchers should build on studies focusing on internal validity. They should extend their work by collaborating with marketing scientists to investigate the purchasing behavior of individuals in real environments. This would serve to demonstrate in realistic settings the robustness of findings observed under controlled, laboratory conditions and, thereby, result in enhanced external validity and relevance.

We find Winer's (1999) vision appealing in its emphasis on collaborative streams of research. In particular, the idea of consumer behavior researchers from an experimental tradition working more closely with marketing colleagues from a modeling tradition seems likely to bode

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well for the future. At the same time, we have reservations about Winer's view of the nature of this collaboration. His view is that collaboration entails combining studies in a compensatory fashion. High internal validity studies are bolstered by strong external validity studies, and, conversely, high external validity studies are supported by strong internal validity studies. The "next" study is always the one that compensates for the weakness of prior research. This results in programs of research that have both internal and external validity, the high quality *and* relevant research of the future. We first raise some concerns about the concept of the compensatory research program. Then we present an alternative vision of research that accords with a view of theory that goes beyond the issues of internal versus external validity.

WINER'S COMPENSATORY TA-EA RESEARCH PROGRAMS

In describing his vision of collaborative research in which studies high in internal validity are combined with studies high in external validity, so that an entire research program has both, Winer (1999) adopts terminology from Calder, Phillips, and Tybout (1981). We distinguished between two different approaches to application. With one approach, which we termed *effects application* (EA), studies are designed to be applied by mapping observed events directly into other settings. If the observed events are similar enough to the other settings, the research is thought to generalize to these settings. Thus, research that is intended to be applicable to people from a broad range of age groups should itself employ a similar range of age groups. And if research is to be applicable to consumers of a product category, then the research should use people with similar consumption experiences. EA studies are thus applied by virtue of their similarity to other settings.

Theory application (TA) studies, in distinction, are designed to be applied by using theory to explain events beyond the research setting. Effects observed in the research are not expected to generalize directly to other settings. Rather, effects are used to test the theory. And it is the theory that is expected to apply to other settings.

Our point in distinguishing the two approaches to application is that it is important not to confuse EA and TA studies. Each should be designed according to its particular approach to application. EA studies should not be designed like TA studies and vice versa. It may, for instance, be perfectly reasonable to use student research participants in a TA study if having a homogeneous sample leads to a more powerful test of theory. For an EA study, it might be appropriate to consider student participants to be a particularly lame attempt at a convenience sample. But to transfer this to the TA study is to confuse the different approaches to application.

Winer (1999) uses the distinction between TA and EA research in a different way. He identifies TA research with being concerned almost exclusively with internal validity. EA research, in contrast, is distinguished by a concern for external validity. He then uses Lynch's (1982) argument that even TA studies need external validity in that TA studies may otherwise be compromised by unknown background variable interactions. This leads to the conclusion that this weakness can be remedied by combining TA studies with EA studies in a research program.

Our concern is with Winer's characterization of TA studies. There is more to TA research than simply an emphasis on internal validity. TA research is about the testing of theoretical explanations. The design goal is not to mimic other settings. It is to establish experimental conditions that allow a theory to predict outcomes that would not be anticipated by available alternative explanations. Internal validity is necessary but by no means sufficient for this. The key to TA is that the research design exposes the theory so that the theory either fails or predicts results that cannot otherwise be explained. This focus on testing theoretical explanations dictates how TA research should be conducted (see Sternthal, Tybout, and Calder 1994 for a full treatment of these implications).

In our view, the main point about TA research is not that it is focused on internal validity rather than external validity. More important is that TA research is fundamentally about testing theoretical explanations. From this point of view, external validity is not just a matter of emphasis. The question is, do issues of external validity bear on the severity of the test of theory? And the answer is that issues grounded in a definition of external validity that is based on notions of statistical generalization, robustness, and realism (Winer 1999, p. 350) pale when the question is asked in this way. If student research participants do not compromise the severity of the test, and perhaps even enhance it, such sampling is acceptable and even desirable.

Now, Lynch (1982) does raise a sophisticated point in this connection. He argues that some background variables of the sort usually worried about in connection with external validity (e.g., age, income levels, and consumption experience) may indeed compromise the testing of theoretical explanations.[1] We do not doubt that this is a possibility. The problem is how to address it. Lynch argues for using such variables as blocking factors or at least replicating research in situations that maximize heterogeneity. Our argument against this (Calder, Phillips, and Tybout 1982, 1983) is that the number of possible background factors is too large to handle in this way. We contend that one must take a theoretical approach to deciding which background factors warrant attention. And doing so effectively transforms background factors into variables of theoretical interest. The only reason for focusing attention on one background factor versus another is that it fits into the

theoretical explanation and adds explanatory power. If so, then the factor becomes just another variable in the design requiring convergence procedures in an effort to triangulate on the underlying theoretical constructs (Sternthal et al. 1994). If a background factor is not linked to the theoretical explanation, there is no more reason for incorporating it into the research design than there would be for any of a host of other factors.

We would prefer to consider background factors in connection with the theoretical explanation being tested. Lynch would prefer a more ad hoc approach in an effort to increase confidence in the theoretical explanation. Regardless of how one feels about this issue, it seems to us that neither position argues that we should focus on the variables in a TA study apart from the theoretical explanation at issue. Thus, in our view, Winer goes well beyond Lynch in advocating that TA studies be concerned with external validity "however defined." We believe that TA studies should be concerned with *any* factors that compromise the theoretical explanation but not with demonstrations of robustness and realism per se.

Winer is, in effect, advocating that TA studies be replicated by mapping the TA study into a real setting. But it is unclear how this external validity study actually complements the original TA study. It is not designed to provide a stronger test of the theoretical explanation. It can only show that the effects obtained in the TA study generalize to another (more "real") setting. As such, the external validity study that Winer wishes to combine with the TA study represents a switch in the approach to application. It is really an EA study in the original sense of Calder et al. (1981). It represents a different approach to application—application not by theoretical explanation but by correspondence of settings. Rather than compensating for the weakness of the TA study, it changes approaches and runs the risk of confusing TA with EA research.

AN ILLUSTRATION

To illustrate the concept of a compensatory TA-EA research program, Winer (1999) offers an example from his own work that we find intriguing. We examine these studies here to highlight our concern that Winer's vision of research does not take into account the critical importance of testing theoretical explanations.

Winer suggests that Simonson (1990) is an example of a TA study and Simonson and Winer (1992) an example of an EA study that taken together illustrate the concept of a compensatory research program. The Simonson research is regarded as "more clearly in the TA camp, as the focus is more on the theoretical explanation for the finding rather than whether the particular effect found would replicate in the real world" (Winer 1999, p. 356). The basic hypothesis that is confirmed across three studies is that consumers' choices of brands for sequential consumption reflect greater variety when these choices are made simultaneously rather than sequentially. This hypothesis was "motivated" (Winer 1999, p. 353) by previous studies showing variety seeking and the notion that this would be more likely when consumers are uncertain about future preferences (as with simultaneous purchases for future consumption). And variety simplifies the greater difficulty of a simultaneous purchase.

What we find intriguing about this example is that in our opinion, Simonson (1990) is a treasure trove of interesting theoretical explanations. The most obvious thing about the research, however, is that none of these explanations is tested. All are loosely consistent with the hypothesis about purchase quantity when choices are simultaneous versus sequential. None is really favored by the findings. The detailed analyses provided are similarly interesting but also indecisive. Simonson finds, for instance, in examining decision-making protocols that subjects who found it harder to simultaneously choose for future periods (indicating uncertainty) were more likely to show variety. And subjects were often observed to simplify by first eliminating alternatives and then choosing all the noneliminated alternatives. But no favored theoretical explanation emerges. Theories of variety seeking as a hedge against uncertainty and of employing a simplification decision heuristic or combinations of the two remain equally plausible. Moreover, other theoretical ideas abound. Although no subjects mentioned expected satiation, plausible lines of theorizing could be developed around the mere act of choosing an item from a set and then having to choose from the set again.

For Winer (1999) it is sufficient for the Simonson (1990) research to be an example of TA that the hypothesis linking variety to simultaneous choice is theoretically motivated. He is more concerned with internal and external validity. For him, the study has high internal validity but lacks external validity. The studies used undergraduates. Subjects never spent their own money. A limited number of products were used. So what is needed is another study to compensate for these weaknesses—a study that translates the variables of Simonson's hypothesis into a more realistic context. Simonson and Winer (1992) do this by using scanner data. They look at whether consumers buy more unusual yogurt flavors, given their purchasing history, when they are buying more yogurt. The study uses real consumers spending their own money. This is the sense in which it is an EA study that supplies the external validity missing from Simonson's original TA study.

Our point, of course, is that all of this ignores the real purpose of TA research, which is to test theoretical explanations and to approach application through theory and not effects generalization. Simonson and Winer (1992) do not add to the theory testing power of the original study.

Their findings too are only loosely consistent with all of the theoretical explanations discussed. They do not favor any particular line of theorizing. The study is useful, and intriguing, in the same way as the original study from the standpoint of theory testing. But it does not represent theoretical progress.

What this really points up is that Winer is not primarily concerned with testing theoretical explanations. Ultimately, his research program vision has more to do with the generalization of effects through the use of realistic research settings than it does theory testing. As such, it does not so much compensate for the weaknesses of TA research but switch to a different approach to application.

THE CALDER-TYBOUT VISION

Our vision of research in the future is this: researchers pursue either theoretical explanation (a more descriptive term than TA) or EA. There may be synergies between the two approaches, but they should not be confused. Theoretical explanation research is devoted to theory testing. The best research design is the design that allows the most unambiguous assessment of theoretical mechanisms. Internal validity is important but by no means sufficient for testing theory (see note 1). Observed findings do not prove theoretical explanations. Findings are only evidence for or against the theory. Nor are hypotheses about findings the same as the theory. Theories are composed of constructs that are not the same as observed variables. Theories explain the effects obtained in studies, but explanation resides in the theory, not in the effects.

EA research does not seek to go beyond observed effects. The question is whether observed effects are obtained in a research setting that corresponds to other settings of interest. The best design is one that represents most fully the settings of interest in the research settings.

Obviously, it makes sense to consider the type of external validity Winer (1999) discusses in EA research. This research is all about mapping external variables into the research design. But robustness and realism should not be the focus of theory explanation research. It does not matter if variables are artificial if they enhance the power of the theory test. There may be a need per Lynch's (1982) argument to consider exogenous variables that may be relevant to the test of theory, although we see this as a part of theory formulation rather than external validity. In any case, there is not a need to include variables merely because they occur in other settings of interest when trying to test a theoretical explanation.

In our vision, researchers would be free to pursue the type of research they choose rather than having to append sections to their reports or even conduct studies to satisfy colleagues who are more attuned to the other type of research. Theory explanation research would be evaluated according to the severity of the theory test and the adequacy of available alternative explanations. EA research would be evaluated in terms of external validity—how well the research setting maps into real settings of interest.

In the future, assuming that the goal is to test theory, as Winer (1999) has argued was the case in Simonson's 1990 article, our recommendation would be *not* to pursue the robustness and realism of the effect. Instead, a next study would focus on obtaining evidence for or against some of the possible theoretical mechanisms (i.e., a desire for variety and change due to uncertainty, simplification of the decision task, risk reduction, or the role of prior behavioral choice in satiation). It is not clear whether any or all of these processes are operative. Thus, a study might be conducted in which research participants are primed with alternative simplifying heuristics (choose the cheapest brand, choose the leading brand) to see if these reduced variety seeking with simultaneous choices. Such a result would favor a task simplification explanation. This work would not be burdened with theoretically irrelevant requirements such as that the findings must also be obtained for people paying with their own money. One would only be interested in whether people actually purchased the product if this variable were tied to one of the theoretical explanations under investigation.

If the Simonson and Winer (1992) article were evaluated as EA research, which is how we see it, the recommendation would be that subsequent research pursue even greater external validity. Would the observed effects be obtained with scanner data for products other than yogurt or for single-person households composed of people who are less likely to buy products for guests? This work would not be burdened with objections that the explanation for the observed effects remains ambiguous.

Our vision is therefore one of diversity in which different approaches to research coexist. Certainly, there is always the possibility of synergy. Theory testers may well get ideas from effect studies. And effect application researchers may well be motivated by theoretical discussions in selecting effects to generalize. But each type of research is evaluated according to its own merits.

BUT WHICH APPROACH IS BEST?

It is natural that researchers harbor the view that the approach to research that they favor is the best and that it can be used to improve the other type of research. Someone who favors EA studies might wonder, "Why don't theoretical explanation researchers wake up and realize just how slow and difficult it is to pin down theoretical mechanisms and how little success can be pointed to in the consumer behavior literature?" While a proponent of TA studies might grumble, "Why don't EA researchers see that they are trying to bail out the ocean with a spoon and

that there will always be one more issue of external validity (similarity) that makes their work questionable?" Hence, when speaking of TA researchers, Winer (1999) allows that "scholars from every discipline normally develop a research 'routine' in which they tend not to stray too far from what has made them successful . . . there is considerable amount of inertia in research programs pursued by academics" (p. 352). And although in previous articles we have never taken a stand for or against theory explanation versus EA research, it will surprise no one familiar with our work as to where our sympathies lie.

Frankly, we do not foresee that either approach will clearly win out in anything like the near future. Certainly, the dawn of the millennium will not bring about any abrupt shift. Our hope, however, is that in the years to come researchers, regardless of which approach they favor, will stop scratching their heads over the inability of the other side to see the error of their ways. As to what might produce this no doubt optimistic vision, we can offer little in the way of concrete suggestion. But it does occur to us that one thing stands in the way. It is the perception by some researchers that theoretical explanation researchers really do not care about application. The perception is, as Winer (1999) and Wells (1993) have put it, that theory testers are only too willing to make application someone else's problem. Although we can see why this view is tempting from an effects research perspective, it is simply not the case. We wish to assert emphatically that theory testing does not imply any shirking of responsibility for relevance.

We thus offer as part of our vision an explanation of how theory is applied. In our original article outlining the distinction between EA and TA (Calder et al. 1981), we indicated that TA involves two steps. In the first, theory-testing step, the focus centers on arriving at the most compelling explanation for the phenomenon of interest. This step seems to be what Winer has in mind when he refers to TA, and it has been the focus of our discussion to this point. However, to apply theory, there is an important second step. This second step is necessary because theoretical explanations may not predict specific outcomes in situations in which key variables are uncontrolled. This second step entails using theory as a basis for developing a strategy or intervention and then subjecting that intervention to testing. Intervention testing is best conducted in an environment in which variables that are not of theoretical interest, but that might overwhelm the effect of the invention, are allowed to vary or are systematically manipulated. The goal is not to test the theory—it has already been tested—but rather to determine whether the theory-based intervention is strong enough to have its intended impact when nontheoretical factors are allowed to vary.

This notion of a two-stage process to TA becomes clearer if we borrow another example used by Winer (1999). We see this research as at least in the spirit of intervention testing. LeClerc and Little (1997) draw on well-tested theoretical models regarding the relationship between motivation and persuasion (Eagly and Chaiken 1993; Petty and Cacioppo 1986) in designing strategies for making free-standing inserts (FSIs) effective for different segments of consumers. They apply the elaboration likelihood model (ELM) to arrive at the prediction that brand-loyal consumers will have a lower motivation than brand switchers to scrutinize advertising copy associated with an FSI. Thus, FSIs featuring a picture (easy to process) and no detailed copy are expected to be more persuasive for loyal consumers, whereas FSIs featuring details about brand (more difficult to process) are anticipated to be more persuasive for switchers. Both laboratory experiments and an analysis of the scanner data yielded findings suggesting that the FSI interventions operated as intended for the different segments of consumers.

LeClerc and Little's (1997) focus on the second stage of TA research is appropriate because they draw on a theoretical explanation that has survived numerous tests over many years. Of course, Stage 2 TA studies will not always yield predicted results. It might have been the case that the anticipated results were obtained for some product categories but not others or for low value FSI but not for high value ones. Such variation might occur because these other factors also affect motivation to process the FSI. If so, such findings would not be at odds with the theory, but they might provide future insight into the actual circumstances, that is, whether the particular intervention would be effective. (Failures that could not be accommodated by the theory might, of course, lead to new hypotheses to examine from a theory testing [Stage 1] perspective.)

The point is that far from ignoring application, good theory is a gold mine for potential applications. At the same time, theory-testing studies will be insufficient to ensure that any particular intervention designed with the theory in mind will have the intended effect. Thus, when the costs of failure are judged to be high, it is prudent to support the TA with intervention testing (see Calder et al. 1981 for more detail regarding intervention-testing studies and how they differ from both effects studies and theory-testing studies). Suffice it to note here that intervention testing is tended to be just that, a test of the particular intervention and not a generalization of an effect to many other situations.

THE FUTURE OF BUSINESS SCHOOLS

To many practitioners and academics, debates about theory and effects and internal and external validity may seem to be only a matter of rather internecine quarreling. Winer (1999), however, does a service in relating these issues directly to ones of more widespread concern. What should marketers be teaching executives and future

executives? What is the relation of marketing to other disciplines? And what should a business school look like in the future?

The issues raised through debates about the theory explanation and effects generalization approaches thus extend beyond questions of how studies should be conducted. Winer's (1999) vision is one in which pressures for business schools to become more "relevant" require them to place more emphasis on the external validity of studies and therefore on effects generalization. Joint ventures with marketing science researchers, using realistic settings of the sort provided by scanner data, include a role for those less immediately concerned with relevance. However, "there is a big distinction between being a social or cognitive psychologist in a psychology department versus being a member of a marketing department or group in a business school" (p. 352). Business school faculty members must "teach students who are more interested in the real world than the laboratory world" and must themselves be interested in "thinking about our research in the same way" (p. 352).

Few would dispute the premise that the pressure on business schools for relevance will increase. But does this mean that faculty must think of their research as the application of findings of effects and train their students accordingly? We think not. In our view, the path to greater relevance lies in the appreciation of the power of theory.

We realize that the vision of business schools as applied and, therefore, effects driven does not mean that theory is accorded no role. Winer (1999) certainly indicates that theory can motivate research. This vision does miss, however, a very critical aspect of theory. What it misses is that theory is not simply something that leads to findings of effects. Observed effects are not complete in themselves. The reason is that linking A to B, even with scanner data, does not in itself explain the effect. Finding that variety increases with the simultaneous choice of a number of items does not explain why this effect is obtained. Explanation for this or any other effect necessarily resides in theory (see Sutton and Staw 1995 for a good discussion specific to this point).

Theory is separate from observed effects. As stated earlier, this is why theory testing is not to be confused with internal validity and why external validity is, as unintuitive as this seems, not a necessary condition for theory tests. Beyond this, it is why business schools should not be constituted to emphasize effects at the expense of providing theoretical explanations.

An illustration may help in appreciating the importance of regarding theory as something more than observed effects. It is said that Karl Popper used a thought experiment for illustrating this point to students (Watkins 1997). Suppose one had data of the sort that a man had survived all attempts to kill him, including having an atomic bomb exploded beneath him. The data are clear. The effects of the attempts to kill the man are that he is still very much alive. The explanation of the data, however, is not clear. What it is that would explain the man's apparent immortality is not *in* the data. It has to come from a theory. This theory can be tested with data designed for this purpose. But this is not the same as showing that one more attempt of a different kind generalizes the effects of previous attempts to kill the man. Nor is the theory somehow an obvious extension of the observations. Popper joked that you ask the man how he survives and he says, "Oh, it's easy; I'm immortal." Probably one should not try this humor on students today, but the moral is sound: theoretical explanations are never as easy as data.

An analogy may help in appreciating the broader importance of theory not only for research but also for the future of business schools. Apply the vision of relevance through effects generalization to another professional school, the medical school. Certainly, medical schools have the same research and teaching mission and the same pressures for relevance to settings of practice. And theory explanation and effects generalization are both possible in this context. Would one seriously argue that medical schools should be constituted to give more weight to effects generalization?

Take the case of a procedure such as electroconvulsive therapy (ECT). Findings indicate that a very large percentage of patients with severe depression improve after multiple ECT sessions. The reason for this effect is not clear, however. Should this be the prototype for successful relevance for the medical school? Note that this is not the same as asking, if this is the only information about ECT that we have, whether it should be used. Based on the observed effects, ECT can be recommended to medical practice. It seems apparent to us, though, that anyone would agree that theoretical work is needed and that medical students would benefit from a greater understanding of *why* ECT works.

ECT is an extreme example, but the point it makes is this. Much as the medical school is very much attuned to advising practitioners based on clinical trials of observed effects, it must also be committed to theory. The effects of antidepressant drugs like Prozac can certainly be tested for effects in clinical trials. But this does not take away from the importance of understanding how the drugs work based on explanations of the neurotransmission of chemical signals in specific areas of the brain such as the amygdala. Moreover, theories far removed from the levels of specific chemicals such as serotonin are needed. This theory will not come from effects research. It will come from research dedicated to theory. And there is *not* a big distinction to be made about whether this research comes from researchers in a biochemistry department or in the medical school, as long as it comes. Moreover, we doubt that

anyone would argue that medical students should be taught to think only in terms of the "real" world of dispensing pills versus having the best available understanding of what they are doing.

Why should business schools be any different from medical schools? There is clearly a place for effects generalization and a need for theory development and testing in both. It is not enough that effects research be motivated by theory. Theory does not come from considering effects. It comes from dedicated research. Researchers trained in theory testing might well work in a discipline-based department, but they are needed in the business school also to make sure that the work gets done. And being in the business school should facilitate the application of theory—but *theory as applied explanation* in the sense that we have advocated, perhaps coupled with intervention testing.

SO WHAT DO WE TEACH?

We conclude by asking the reader to imagine a business school class of the future. The class is discussing new retailing formats. The professor is prepared with rules from many different effects studies. Greater purchase variety has been observed with multiple simultaneous purchases of yogurt and, by this point, other scanner data product categories. And it has been shown that the effect holds for different age groups and some other demographic divisions.

A student asks whether it might be a good idea to use an in-store format emphasizing variety for, say, imported beers. The student has in mind a chain of party stores in which the average purchase size for beer is five units and single-unit sales are rare. The instructor replies that, of course, there are no studies that match this situation exactly, but some are close, and so by virtue of external validity, the answer is that variety should be featured.

Contrast this with the instructor who can turn to theory as well as effects, without confusing the two. "You see," the instructor says, "a number of laboratory studies with college students support the explanation that people process information in simultaneous choice situations in the following way. . . . "

NOTE

1. This concern stems from the relationship between internal validity, external validity, and a third type of validity, construct validity (see the Calder, Phillips, and Tybout 1982, 1983; and Lynch 1982, 1983 debate for details of these distinctions). Winer (1999) does not discuss construct validity, and, therefore, we will not introduce that terminology here. However, the notion of construct validity is reflected in the focus of theory application on ruling out alternative explanations to arrive at the single, best account for a phenomenon. We refer to this process as seeking "theoretical explanation."

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