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The Ongoing Process of Building a Theory of Disruption

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The easiest way to respond to the critiques and complements the other authors in this issue have written about the model of disruption would simply be to address them head on—to accept some as useful additions or corrections and to suggest that others are ill-founded. Because this special issue of *JPIM* represents a unique opportunity to examine the process of theory-building as it unfolds, however, this article is structured in a way that addresses the other scholars' suggestions in the context of a model of the process by which theory is built and improved. My hope in doing so is that this issue might not just be an examination of this particular theory of disruptive innovation but that it might also constitute a case study about theory-building itself—a study that can help scholars of management in different fields to conceptualize how the theory-building process is or is not at work in their domain—and how they might help the process work better.

A Model of the Theory-Building Process

Some years ago in a doctoral seminar my students and I examined how communities of researchers in a variety of disciplines had cumulatively built bodies of understanding. Seeing some stunning commonalities in the processes these scholars had followed, we synthesized a model of the process of theory building (for a summary, see Carlile and Christensen, 2005). My students and I found this model extremely useful as we designed our own research, positioned our work within streams of prior researchers' efforts, and evaluated the reliability and validity of various papers. The present article recounts the development of the theory of disruption within the context of this model of what theory is and how it is built. It also suggests how the comments of the other authors in the current issue of *JPIM* might contribute to the improvement of this body of theory. In this way, I hope that both the content of this theory and the process by which it is being built might become clearer.

Our model asserts that theory is built in two major stages: the descriptive stage and the normative stage. Within each of these stages, theory builders proceed through three steps. The theory-building process iterates through these three steps again and again. In the past, management researchers have quite carelessly applied the term *theory* to research activities pertaining to only one of these steps. Terms such as *utility theory* in economics and *contingency theory* in organization design, for example, actually refer only to an individual step in the theory-building process in their respective fields. It is more useful to think of the term *theory* as a body of understanding researchers build cumulatively as they iterate through each of the three steps in the descriptive and normative stages. This should be abundantly clear as we examine the theory of disruption. It already has evolved considerably as a growing group of scholars, including those whose work is published herein, have worked to refine it. Among the most notable improvements to date have been Adner and Zemsky (2003), Adner (2002), Gilbert (2001), Christensen and Raynor (2003), and Christensen, Anthony, and Roth (2004).

Building Descriptive Theory

The descriptive stage of theory building is a preliminary stage because researchers generally must pass through it before developing normative theory. The three steps researchers use to build descriptive theory are observation, categorization, and association.

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Step 1: Observation

In the first step researchers observe phenomena and carefully describe and measure what they see. This stage of research is depicted in Figure 1 as the base of a pyramid. Unless researchers lay a foundation of careful observation, documentation, and measurement of the phenomena in words and numbers, subsequent researchers will have difficulty improving the theory because they will not be able to agree on what the phenomena are.

Researchers in this step often develop *constructs*, which are abstractions that help us rise above the messy detail to understand the essence of what the phenomena are and how they operate. Bower's (1970) *Managing the Resource Allocation Process* is an example of this. His constructs of *impetus* and *context*, explaining how momentum builds behind certain investment proposals and fails to coalesce behind others, have helped a generation of strategy and innovation researchers understand how strategic investment decisions are made.

My initial research on the history of the disk-drive industry comprised this phase in the building of disruption theory. It entailed building a database of all components and technologies in every disk-drive model ever announced by any company in the world between 1976 and 1992; of the revenue histories of every disk-drive company; and of the market shares of each competitor by product segment. This data were a complete census, not a statistical sample—Christensen (1992) first compiled the empirical evidence.

The constructs developed in this stage are the two intersecting trajectories of performance improvement depicted in Figure 2. The dotted one charts the rate of improvement in performance that customers can utilize, and the other maps the pace of improvement that innovating companies provide. I observed that the

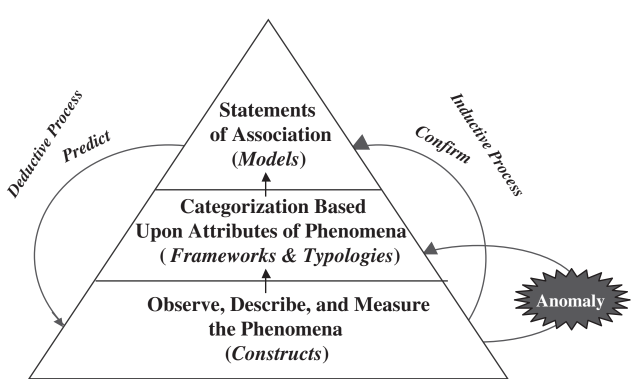

Figure 1. The Process of Building Theory

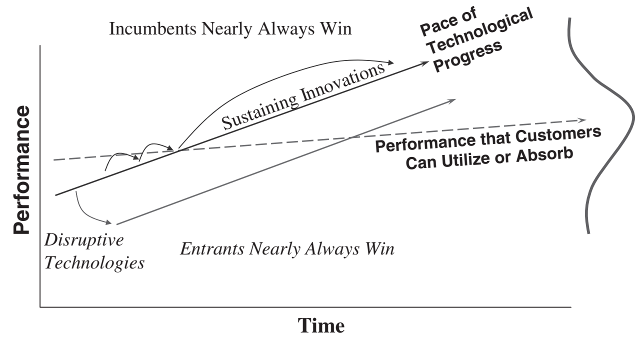

Figure 2. The Constructs of Performance Trajectories and the Classification of Sustaining and Disruptive Innovations

trajectory of technological progress outstripped the ability of customers in any given tier of the market to utilize that improvement.

Step 2: Classification

With the phenomena described, researchers in the second stage of the theory-building pyramid then classify the phenomena into categories. In the descriptive stage of theory building, the classification schemes scholars propose typically are defined by the attributes of the phenomena. Categorization simplifies and organizes the world in ways that highlight possibly consequential relationships between the phenomena and the outcomes of interest. These descriptive categorization schemes are often referred to as *frameworks* or *typologies*. The study of strategy, for example, contains categories such as diversified versus focused firms and vertically integrated versus specialist firms. Slater and Mohr (this issue) categorize firms as analyzers, defenders, prospectors, and pioneers (originally proposed in Miles and Snow, 1978)—a descriptive categorization scheme. The classification of the myriad technologies in the history of the disk-drive industry into sustaining and disruptive categories emerged from this stage of my work.

Step 3: Defining Relationships

In the third step, researchers explore the association between the category-defining attributes of the phenomena and the outcomes observed. They make explicit what differences in attributes and differences in the magnitude of those attributes correlate most strongly with the patterns in the outcomes of interest. Techniques such as regression analysis often are useful in defining these correlations in the stage of descriptive theory building. The output of studies at this step are referred to as *models*.

My finding that the industry's leading firms almost always triumphed in battles of sustaining innovation and that entrant firms typically beat the incumbent leaders when disruptive innovations emerged was the conclusion of this stage in the process of building the theory of disruption; at this point in the research, this was a statement of correlation.

How Theory Is Improved in the Descriptive Stage

When researchers provide constructs, frameworks, and models by climbing the pyramid in these three steps—observation, categorization, and association—they have followed the inductive portion of the theory building process. Researchers then begin to improve the theory by cycling from the top down to the bottom of this pyramid in the deductive portion of the cycle: testing the hypotheses that were inductively formulated. This most often is done by exploring whether the same correlations exist between attributes and outcomes in a different set of data than from which the hypothesized relationships were induced. When scholars test a theory on a new data set they sometimes find that the attributes of the phenomena in the new data do indeed correlate with the outcomes as predicted. Such tests confirm that the theory is useful under the observed circumstances observed. However, researchers who stop at this point simply return the model to its place atop the descriptive pyramid, tested but unimproved.

It is only when an anomaly—an outcome for which the theory cannot account—is identified that an opportunity to improve theory occurs. A theory that can be falsified is a statement capable of yielding anomalies. As Figure 1 depicts, anomalies give researchers the opportunity to revisit the foundation layers in the theory pyramid—to define and measure the phenomena more precisely and less ambiguously or to categorize the data better—so the anomaly and the prior associations of attributes and outcomes can all be explained.

In the study of how technological innovation affects the fortunes of leading firms, for example, radical versus incremental innovation was an early attribute-based categorization scheme. Statements of association built on this scheme were that established firms tend to do well when faced with incremental innovation but that they stumble in the face of radical change. However, established firms that succeeded with radical technology change were an anomaly to this generalization. To account for these anomalies, Tushman and Anderson (1986) offered a different categorization scheme: competency-enhancing versus competency-destroying technological changes. This scheme resolved many of the anomalies to the prior scheme, but subsequent researchers uncovered new ones for which the Tushman–Anderson scheme could not account. Most notably, Henderson and Clark's (1990) categories of modular versus architectural innovations were a response to these anomalies. My initial work on disruption, in turn, was an attempt to resolve anomalies I had observed in the disk-drive industry for which Henderson and Clark's work could not fully account. Descriptive theory is often characterized by a plethora of categorization schemes because the phenomena generally have many different attributes. Often in this phase, no model is irrefutably superior: Each seems able to explain anomalies to other models but suffers from anomalies to its own. Kuhn (1962) observed that a very similar condition characterized the period prior to the emergence of a paradigm in the various fields of scientific inquiry whose histories he chronicled.

Every complete lap around the theory-building pyramid consists of an inductive side and a deductive side. We noted that all observations are shaped, consciously or unconsciously, by cognitive structures, previous experience, or some theory in use. Although it is true that individual researchers might start their work at the top of the pyramid, generally the hypotheses deductive theorists test have been derived consciously or unconsciously, by themselves or others, from an inductive source. Few blue-sky hypotheses are conceived *de novo* at the top of the pyramid in the complete absence of observation. Danneels (2004) observed that the model of disruption was derived only from historical data, and he is correct: it was inductively derived, and data exists only about the past. It is not a weakness of the model; it is simply a fact of inductive theory building. Danneels and Tellis (this issue) are absolutely incorrect, however, in any assertion that disruptiveness is defined post hoc: They seem to have concluded, that if the leader was dethroned or missed the technology, it was disruptive. I am not aware of a single instance where I have done this. The model was derived from histories, but the definition of disruptiveness (restated following) exists independent of the outcome (Christensen and Bower, 1996). The theory therefore is capable of yielding anomalies, some of which are enumerated later in this article.

To clarify this, consider different technologies that bloodied the American integrated steel companies in the 1960s and 1970s. American mills were very slow in adopting the technologies of continuous casting and basic oxygen furnaces. Japanese competitors adopted them much more aggressively, and the American steelmakers lost significant money and market share. These technologies were not disruptive, however; they were sustaining innovations. Christensen (2002) explains why the American mills found it difficult to adopt them—not because the technologies were disruptive. The American companies' sales were not increasing, so they were not adding capacity, whereas Japanese steelmakers were building new mills year after year. Minimills were indeed disruptive, inflicting equally brutal damage. Someone engaged in post hoc definition would label these all as disruption. But the mechanism of paralysis to the leader in the case of the first two technologies was fundamentally different from minimills.

I have heard many people make the mistake of post hoc definition of *disruptiveness*, and I correct them whenever I hear it. If Danneels (2004) or Tellis (this issue) have ever read about or have heard me commit this error, I ask them to point out specifically where I have been so sloppy, and I will issue a letter of apology and retraction. The term *disruptive* has many prior connotations in the English language, such as "failure" and "radical," in addition to the phenomenon to which I applied it. I fear this is why we see so much post hoc definition by the uninformed. As noted following, Grove (1998) proposed that the phenomenon should be labeled the "Christensen Effect" to eliminate this source of misunderstanding. Possibly we should have taken his advice.

The Transition from Descriptive to Normative Theory

The confusion of competing categorization schemes that often accompanies descriptive theory is resolved when researchers, through careful observation, move beyond statements of correlation to define what causes the outcome of interest. As depicted in Figure 3, they leap across to the top of the pyramid of normative theory, whose capstone is a statement of what causes the outcome of interest, not just what is correlated with it. Their understanding of causality enables researchers to assert what actions managers ought to take to get the results they need. For reasons noted following, normative theory has much greater predictive power than descriptive theory does. As preliminary versions of this article have been presented in various faculty seminars, my students and I have frequently found ourselves engaged in esoteric discussions about whether absolute truth exists, let alone whether we can ever discover what it is. We concluded from these discussions that we cannot judge the value of a theory by whether it is true. The best we can hope for is a body of

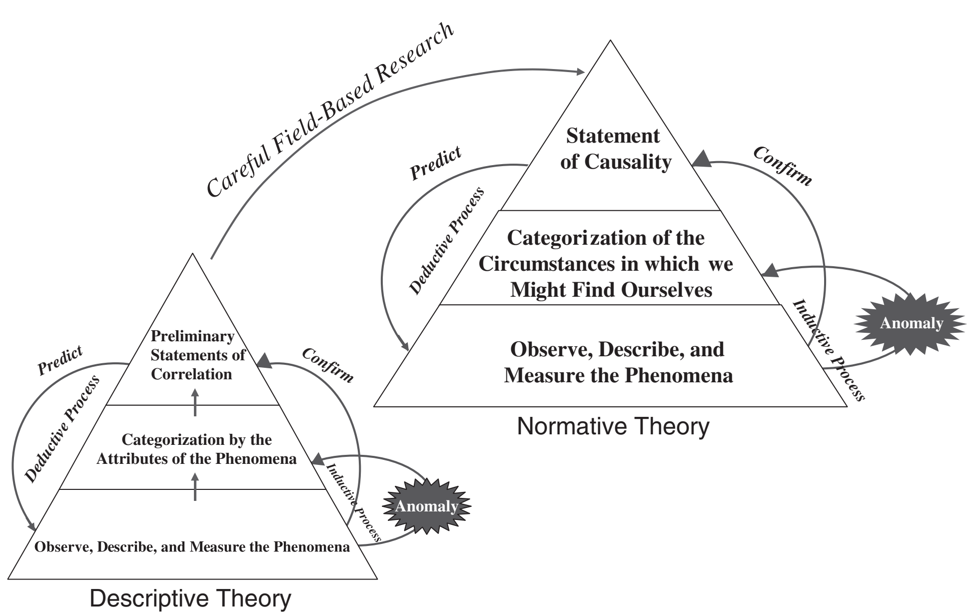

Figure 3. The Transition from Descriptive Theory of Normative Theory

understanding that asymptotically approaches truth. Hence, the value of a theory is assessed by its predictive power, which is why this article asserts that normative theory is more advanced, and more useful, than descriptive theory.

Normative theory, like its descriptive predecessor, still needs to be improved, and researchers do this by following the same steps used in the descriptive stage. Hypothesizing that their statement of causality is correct, they cycle deductively to the bottom of the pyramid to test the causal statement: If we observe these actions being taken, these should be the outcomes we observe. When they encounter an anomaly, they then delve back into the lower levels of the pyramid. Sometimes they can resolve anomalies by developing more accurate, less ambiguous ways to define and measure the phenomena. Often they account for anomalies by revisiting the categorization stage. Rather than using schemes based on attributes of the phenomena, however, researchers building normative theory categorize different situations or circumstances in which managers might find themselves. They do this by asking, when they encounter an anomaly, "What was it about the situation in which those managers found themselves that caused the causal mechanism to yield an unexpected result?"

By asking this question as they cycle up and down the pyramid of normative theory, anomaly-seeking researchers will ultimately define a relatively complete set of the situations or circumstances in which managers might find themselves when pursuing the outcomes of interest—whether or not this set can ever be defined in permanent, unambiguous ways is addressed later in this article. Asking this question allows researchers to make contingent statements of causality—to show how and why the casual mechanism results in a different outcome in different situations. A normative theory built on well-researched categories of circumstances can help managers, given their circumstances, predict accurately what actions will and will not lead to the desired result. In other words, the circumstance-contingent predictive power of normative theory enables managers to know what they ought to do given their circumstances. Bazerman (2005) noted that one reason why the work of social science researchers generally has had little influence on management is that most choose not to be prescriptive. In fact, a culture of sorts has emerged among many social science researchers that descriptive theory is as far as they should go. Bazerman shows that normative theory is not only possible to develop in the social sciences; it also is desirable.

Disruption theory began the transition from descriptive to normative theory in my own mind in about 1996, as I interacted with Stanford professor Robert Burgelman in a project at Intel Corporation. It became clear that the causal mechanism of the outcomes we observed—the incumbent leaders excelled at sustaining innovation but rarely succeeded at disruption—was resource dependence as manifested in the resource-allocation process. We observed that managers must prioritize investments that help them garner resources from customers and investors in the way their firms are structured to generate profit, or they cannot survive.

As Burgelman and I used this theory to explain the success and failure of various companies in the semiconductor and computer industries, we encountered anomalies: incumbent leaders in their industries that had succeeded at disruption. We observed, however, that in each of these anomalous instances, the leader had maintained its industry-leading position by setting up an autonomous business unit and by giving it unfettered freedom to forge a very different business model appropriate to the situation. In other words, it was not a technology problem; it was a business model problem. I made a mistake when I labeled the phenomenon as a disruptive technology; the disruptive business model in which the technology is deployed paralyzes the incumbent leader.

The resolution of these anomalies helped us define two categories or situations in which managers might find themselves when they confront a new technology or product/market opportunity: (1) when the innovation appears to be financially attractive relative to the organization's revenue and cost structure and its profit model; and (2) when it is not financially attreactive. We were then able to articulate a circumstance-contingent statement of what organizational structure managers needed to create to succeed in the face of different types of innovations, building it directly on Henderson and Clark's (1990) analysis of architectural change. This had not been clear in my mind when the first edition of *The Innovator's Dilemma* was published. A chapter was inserted in the second edition to introduce the *resources, processes, and values* framework, which can help managers see what type of team structure is required for different types of innovations and whether or not they need to create an autonomous business unit. This model was refined further in chapter 7 of *The Innovator's Solution*. With this publication, I now feel quite comfortable with our articulation of when and why autonomous units should and should not be created. I fear that much of the confusion about spin-offs has arisen because I only understood this after *The Innovator's Dilemma* first was published.

Several prominent scholars have examined the improvement in predictability that accompanies the transition from the attribute-based categorization of descriptive theory to the circumstance-based categorization of normative theory. Consider, for example, the term *contingency theory*, a concept born of Lawrence and Lorsch's (1967) seminal work. They showed that the best way to organize a company depended on the circumstances in which the company was operating. In the parlance of this model of theory building, contingency is not a theory per se; rather, it is the categorization scheme. Contingency is a crucial element of every normative theory. Rarely are there one-size-fits-all answers to every company's problem.

Similarly, Glaser and Strauss's (1967) treatise on "grounded theory" actually is a book about categorization. Their term *substantive theory* corresponds to the attribute-bounded categories of descriptive theory. And their concept of *formal theory* matches our definition of normative theory, which employs categories of circumstance.

On the Value of Multiple Methods in Theory Building

Just as inductive and deductive research designs play crucial roles in a complete theory-building cycle, field-based data and large-sample data, and empirical work and mathematical modeling play critical roles. As a general rule, field-based observation is most valuable in the inductive portion of the cycle, in understanding causality, and in defining the categories of circumstance undergirding normative theory. When field research has made progress on these fronts, then large-sample statistical analysis and mathematical modeling can add value to the building of better theory. When researchers use these latter methods in isolation, their work can yield incomplete and even misleading results.

Consider, by illustration, King and Tucci's (2002) study of the disk-drive industry. They analyzed the same disk-drive industry data I used, but with more sophisticated methods of large-sample data analysis than I would be capable of hefting. However, they only used that one method, and they did not consider the circumstance-based categorization scheme I described previously. Rather, they went back to ground zero on the descriptive side of the theory-building process, started at the top of the pyramid with some hypotheses derived from others' work, and deductively tested those hypotheses. One of the correlations they observed was that disk-drive manufacturers' volume in one generation of disk-drive products was strongly and positively correlated with their volume in the next generation of disk-drive products, which is seemingly contradictory to my findings that leaders in one generation generally got disrupted by entrants in the next generation.

What King and Tucci (2002) did not see, because their methods could not give them visibility, is that the dominant vertically integrated disk-drive manufacturers (i.e., IBM, Control Data, and Japanese manufacturers) set up autonomous business units to manufacture the smaller form-factor drives (Chesbrough, 1999; Christensen, 1992, 1997). During most of the years in King and Tucci's study these manufacturers accounted for well over 60% of the volume in the market. Established companies that did not create autonomous organizations got disrupted at these generational form-factor transitions. Although King and Tucci concluded that their study possibly disconfirmed some of my findings, they unwittingly confirmed them. Their work actually is what Yin (1984) calls a theoretical replication of the model of disruption. The fact that they chose recording density as the measure of performance rather than capacity per drive also rendered their study not to be a useful deductive test of the theory, and that is too bad. If somehow I had succeeded in inviting King and Tucci to view me not as a rival researcher but as a slow, tired runner on these laps up and down the theory pyramid who is quite happy to hand the baton off to someone much more gifted in the techniques of large-sample data analysis, they might have been able to find true anomalies to the model, thereby improving it. As it stands, their work became a regression back to the descriptive side of the theory-building process, measuring the magnitude and statistical significance of what are actually spurious correlations.

In contrast to King and Tucci's (2002) seeming determination to start from ground zero rather than to consider the categories of circumstance I proposed, Insead's Ron Adner (2002) used his prowess in mathematical modeling to build on and to clarify the theory. One very important insight his work has given us is the notion of asymmetric motivation: a much clearer definition of the causal mechanism underlying the disruption process.

Deductive Tests of the Model of Disruption

The deductive portion of a complete theory-building cycle can be completed by using the model to predict ex post what will be seen in other sets of historical data or to predict what will happen in the future. The primary purpose of the deductive half of the theory-building cycle is to seek anomalies, not to avoid them. This is how theory is improved. Some have claimed that I have tautologically labeled as an instance of disruption any situation where the incumbent leader was toppled by an entrant and that I have labeled as a sustaining innovation every instance where the incumbent leaders beat the entrants. As such, they assert that the theory cannot be falsified. I suspect those who raise this criticism have not read Christensen and Bower (1996) or *The Innovator's Dilemma* (Christensen, 1997, p. 130), which array the anomalies in tabular form.

True anomalies to the model would be where (1) the incumbent leaders lose their positions of industry leadership to an entrant when confronted with a sustaining innovation that would help them sell a better product for more attractive profit margins to their best customers; or (2) an incumbent successfully builds an industry-leading business in a disruptive innovation by developing and commercializing the disruption within the same business unit that is also responsible for sustaining the profitable growth of the original, healthy, core business.

Historical Search for Anomalies

In the search for anomalies, my colleagues and I deductively tested the model and published paper- or chapter-length reports on the following industries: mechanical excavators, steel production, retailing, computers, printing, motorcycles, automobiles, the economy of Japan, semiconductors, cardiovascular surgery, management education, financial services, management consulting, telecommunications, computer-aided design software, metropolitan newspapers, carbeurators, and airlines. (Some of these are published; some are not. Each is available from the author on request.) These comprise as diverse a collection of data sets as we could find. Some anomalies related to (1) in the previous paragraph have emerged from this work (described in the following section). As yet no anomalies to the second proposition have been uncovered, though I am still holding out hope.

Use of the Theory to Predict the Future

The second way my colleagues, students, and I have deductively sought anomalies is to use the model to predict what will be seen in the future: to predict ex ante whether an innovation will disrupt the leaders. Although Danneels (2004) and others express concern that the model does not provide the ability to predict what will happen, their fear is unfounded. It is true that one cannot think a thought before it has been thought. All that must be asked of a theory, however, is that it help to evaluate a technology after it has been conceived or to evaluate a business venture after it has been proposed or launched. The theory must provide the ability to predict what will happen to the incumbents and entrants in the future if they take different actions relative to the innovation. The earlier we these predictions can be made after conception, of course, the better.

This article provides here four publicly documented examples of how the model was used to predict the impact of technologies across the spectrum of maturity: (1) a technological concept around which a product had not yet been developed; (2) a nascent technology that was just beginning to be manufactured and marketed; (3) an early-stage threat that had taken root but had not yet affected the health of the industry leader; and (4) the future viability of a leading company's strategy of responding to a disruption after the disruption already was well under way. In each case, the prediction was made to help the incumbent leader see the threat of disruption and to address it appropriately before it was too late to take action. I made some of these predictions, and company executives made others without my assistance, after reading a paper or book on the topic.

First, in 1996 Alex d'Arbeloff, the chair of Teradyne, a leading manufacturer of semiconductor test equipment, read an early article about disruption. He concluded that a technological concept discussed in an industry meeting—the potential for making a PC-sized tester using complementary metal-oxide-semiconductor (CMOS) circuitry—was a disruptive technology. He responded by assigning a team to create an autonomous business unit to develop and commercialize the technology. The unit's products took root initially at the low end but have improved their performance at a stunning rate. The technology now accounts for well over $1 billion of the corporation's revenues. D'Arbeloff's ex ante identification of this technology is chronicled in two Harvard Business School case studies, which were written while the technology was being developed and before the products were launched (d'Arbeloff, 1996).

Second, when writing *The Innovator's Dilemma* in 1995, I stated that flash memory was poised as a disruptive technology to hard disk drives (Christensen, 1997, pp. 46–53). I did not predict before flash memory was conceived that it indeed would be, but that is not necessary. All that is required for a theory to be useful is to be able to interpret the meaning and future potential of a phenomenon when it is observed. Flash memory is substituting for the Toshiba Microdrive in the lower-end models of the Apple iPod. Flash is not yet used as the storage device in high-end servers, but disruption is a process, not an event—and the predicted process is well under way. I suspect that most of those scholars who have worried that the model cannot yield predictions because it was inductively derived from historical data now own mass storage memory sticks. They have just forgotten that a decade ago the model allowed us to see this coming.

The third is an early-stage business example. In his August 1998 Academy of Management keynote address, Intel chair Andy Grove showed on the projection screen the diagram of disruption to the assemblage of business school professors. Grove labeled it the "Christensen Effect" because he felt the term *disruption* had too many confusing connotations in the English language (he was right, as noted previously). He predicted that the performance of Intel's processors was going to overshoot the speed mainstream customers could utilize and further predicted that cheaper processors sold by AMD and Cyrix that already had taken root in entry-level computer systems would improve at such a rapid rate they would invade the core of Intel's market. Perhaps Danneels and Tellis were not at that meeting, but hundreds of others saw Grove's chart and heard the prediction. In response to Grove's insight, Intel set up a separate organization in Israel and gave it responsibility to launch what is now known as the Celeron processor. The Celeron is now Intel's highest-volume processor, used squarely in mainstream personal computers. Intel generates billions of dollars in revenues and profits because Grove was able to use the model of disruption to predict the importance of this phenomenon. Indeed, Grove learned of the model of disruption from a much earlier prediction. In late 1994 Carmen Egito, an engineer in Intel's architecture laboratories, received a photocopy from someone of a draft paper on which I was working, which subsequently was published in *Research Policy* (Christensen and Rosenbloom, 1995). She predicted that low-end processors would emerge to disrupt Intel and took her hypothesis to senior management. I was not aware of this until several years later.

Fourth, Kodak's chief executive officer Dan Carp and vice president Willy Shih, on learning of the disruption model in 1999, declared that Kodak had spent $2 billion trying to force digital photography to compete on a "sustaining innovation" basis against film—and against powerful electronics giants such as Sony and Canon as well. They changed Kodak's strategy to disrupt the market with a simple, convenient, low-priced camera called *Easy Share*. Within two years, Kodak went from being a minor participant in this market to having the largest (and still increasing) U.S. market share in digital cameras. In this case, the model did not help Kodak predict the concept of digital photography, but it did help the company predict, mid-stream, that its strategy needed to change. The management team then followed the prescriptions of the model. Kodak's digital photography business now generates $1.5 billion in profitable revenues.

None of these instances is an ex post rationalization that these companies' actions were disruptive. Rather, these were ex ante predictions based on the model that these companies could use disruption to achieve growth and market leadership rather than get killed by it.

A vexing characteristic of the world as God created it, of course, is that when a company chooses a particular course of action we cannot know for certain what would have happened to that same company if it had not taken that course of action. But any assertion that the model has not or cannot be used to predict outcomes simply does not square with historical fact.

The Value of Anomalies in Building Better Theory

As indicated before, when researchers in both the descriptive and normative stages cycle down from the top of the pyramid using statements of association or causality to predict what they will see at the foundation, they often observe something the theory did not lead them to expect, thus identifying an anomaly—something the theory could not explain. Anomalies are valuable in theory building because the discovery of an anomaly is the enabling step to less ambiguous description and measurement and to identifying and improving the categorization scheme in a body of theory. These are the keys to being able to apply the theory with predictable results.

Yin (1984) distinguished between literal and theoretical replications of a theory. A literal replication occurs when the predicted outcome is observed. A theoretical replication occurs when an unusual outcome occurs, but for reasons that can be explained by the model. This means that we must dive much more deeply before an exception to a theory's predictions should be labeled as an anomaly. For example, the observation that airplanes fly is an exception to the general assertion that the earth's mass draws things down toward its core. Does this exception disprove the theory of gravity? Of course not. While falling apples and flaming meteors are literal replications of the theory, manned flight is a theoretical replication. It is a different outcome than is normally expected, but Bernoulli's Principle explains why it can occur. An anomaly is an outcome that is neither a literal nor theoretical replication of a theory.

The Value of Seeking Anomalies

Because the discovery of an anomaly is what triggers a cycle of improvement, it is important to design anomaly-seeking research rather than research that avoids anomalies. I am fortunate to have joined a group of researchers—starting with Dan Schendel, Bill Abernathy and Jim Utterback, Mike Tushman and Philip Anderson, and Rebecca Henderson and Kim Clark before me and including Hank Chesbrough, Ron Adner, Clark Gilbert, and Chris Musso afterward—whose research has focused on uncovering and resolving anomalies in prior scholars work and who have welcomed the discovery of anomalies to their own theories. Precisely because these scholars have followed this process of building sound theory, ours has been a productive stream of research that seems truly to have influenced the way academics and practitioners think about this class of problems.

Tellis (this issue) seems suspicious that I may have been selective in the data sets on which we have tried to test the model in the deductive portion of the cycle, worrying that I have only drawn on historical examples that support the theory while sweeping anomalies under the rug. This is not true. Because he seems not to have read it, I'll reproduce here a paragraph from *The Innovator's Solution* (Christensen and Raynor, 2003, p. 69):

> *We would be foolish to claim that it is impossible to create new-growth companies with a sustaining, leap-beyond-the-competition strategy. It is more accurate to say that the odds of success are very, very low. But some sustaining entrants have succeeded. For example, EMC Corporation took the high-end data storage business away from IBM in the 1990s with a different product architecture than IBM's. But as best we can tell, EMC's products were* better *than IBM's in the very applications that IBM served. Hewlett-Packard's laser jet printer business was a sustaining technology relative to the dot-matrix printer, a market dominated by Epson. Yet Epson missed it. The jet engine was a radical but sustaining innovation relative to the piston aircraft engine. Two of the piston engine manufacturers, Rolls-Royce and Pratt & Whitney, navigated the transition to jets successfully. Others, such as Ford, did not. General Electric was an entrant in the jet revolution, and became very successful. These are anomalies that the theory of disruption cannot explain.*

I am not aware, from the earliest design of the disk-drive research (Christensen and Bower, 1996), that I have been guilty of avoiding anomalies. Rather, I have sought them as a means of improving the theory. As noted previously, these improvements are made by sharpening the definitions and measures of the phenomena, by defining better categorization schemes, and by refining our understanding of the mechanism of causality.

Tellis (this issue) uses his article to advance an alternative theory of his own. His might be better, but how are we to know? My colleagues and I have published several articles and an entire book, *Seeing What's Next* (Christensen, Scott, and Roth, 2004), that predict whether, how, and when certain technologies are likely to prove disruptive (or sustaining) in a range of industries in the future. For further evidence of the usefulness of the theory in prediction in other portions of the semiconductor industry, see Bass and Christensen (2002), Verlinden, King, and Christensen (2002), Milunovich (2002). These typically were written two to three years prior to publication; and many of their predictions already have materialized. For evidence in how the models are being used to look into the future of health care, see, for example, Clayton, Bohmer, and Kenagy (2000) and Shah, Brennan, and Christensen (2003).

It would be helpful if Tellis would publish an article predicting which of our predictions will prove false and which will be borne out, based upon which firms he judges to be guided by leaders who possess the requisite vision and which are not. I extend this invitation to him in an honest and sincere way. As his theory is framed presently, his definitions of self-cannibalization and radical innovation may or may not map onto the constructs of sustaining and disruptive innovation; we cannot tell. Tellis could identify specific anomalies and then could teach us how a better categorization scheme and a different statement of causality can account for all I have accounted for but also can resolve the anomalies. This would constitute a welcome contribution that would benefit all of us.

Resolving Anomalies through Clearer Definitions

This section recounts how the community of researchers on this topic has identified anomalies and has then improved the theory by making definitions and measures more precise, by refining the categories, and by clarifying the mechanism of causality. These have improved the predictive power of the theory.

New market versus low-end disruptions. Rarely do early researchers succeed in describing the phenomena in unambiguous ways. Generally the clarification is achieved by subsequent researchers, who find that by stating more precisely what the phenomena are and are not, they can account for phenomena that otherwise appeared to be anomalous. Several of the authors in this issue accurately note that my early publications about disruptions certainly suffered from this shortcoming. For example, in about 2000 I realized that the phenomenon I previously had characterized simply as *disruptive technology* actually was comprised of two fundamentally different phenomena, which I characterized as low-end and new-market disruptions (described in Christensen and Raynor, 2003).

The article by Govindarajan and Kopalle (this issue) does us a great service through their efforts to define the phenomenon even more precisely. Markides (this issue) also attempts to do this, though the same phenomenon I have called *new-market disruption* Markides terms *strategic disruptive innovation.* Unfortunately, the term *strategic* has many other prior connotations in the English language, too—so that misunderstanding and ambiguity about what the phenomena are still will persist, I fear, even with Markides's suggestion. I hope future scholars will be able to resolve this through even clearer definition.

Disruption is a relative phenomenon. Another improvement in definition of the phenomena has been in understanding that disruptiveness is not an absolute phenomenon but can only be measured relative to the business model of another firm. In other words, an innovation that is disruptive relative to the business model of one firm can be sustaining relative to the business model of another. On-line stock brokerage, for example, is a sustaining innovation (financially attractive) relative to the business models of discount brokers such as Schwab and Ameritrade, because it helps them discount even better. The same innovation is disruptive (financially unattractive) relative to the business model of Merrill Lynch.

Relativity is a crucial concept in the theory of disruption, because only if we understand this can we accurately say which phenomena are literal replications, theoretical replications, and anomalies. Some have worried that I have clouded things rather than clarified them by stating that disruption can only be measured in relative terms. But management literature is replete with relativity. The same innovation can be competency enhancing relative to one company and competency destroying relative to another. The same new business can be distant from the core of one company and close to the core of another, and so on.

This refinement of relativity has helped me make sense of a portion of Markides's (this issue) article. He quite rightly notes that there are "new to the world" innovations—a classification I have not considered as carefully as I should. However, many of the innovations Markides cites as new to the world really were not: Before personal computers there were minicomputers; before VCRs were Ampex's recording machines; before wireless phones were two-radios, cordless and wireline phones; and so on. The concept of relativity should help us: Where an innovation cannot be described relative to a preexisting product or technology, we can say it indeed was new to the world. There was a first wheel, a first photograph, a first boat. But most innovations can be expressed relative to a preceeding form of the same. Personal computers were disruptive relative to minicomputers; Eastman's Kodak Brownie camera was disruptive relative to the prior camera technology; CDs were sustaining relative to cassette tapes.

It is a business model problem, not a technology problem. As mentioned already, my original publications labeled the phenomenon as disruptive technology. In 1997 just after *The Innovator's Dilemma* was published, in a personal conversation Andy Grove surfaced an anomaly that helped me see I had defined it wrong, as he recounted how Digital Equipment Corporation (DEC) was disrupted by makers of microprocessor-based computers. He said, "It wasn't a technology problem. Digital's engineers could design a PC with their eyes shut. It was a business model problem, and that's what made the PC so difficult for DEC." He noted that in the early 1980s proposals to make PCs promised 40% gross margins on machines that could be sold for $2,000. What is more, none of DEC's customers could use them. These proposals were competing for resources against proposals to make more powerful computers than DEC had ever made before. These promised gross margins of 60% on machines that could sell for $500,000. It was the attractiveness of the opportunity relative to the company's business model that made the sustaining path attractive and the disruptive path unattractive.

To see why expressing it in terms of disruptive business models is an important improvement to the theory, consider the comment in Govindarajan and Kopalle (this issue) that wireless telephony is a "high-end" disruption. From a technology point of view, cellular telephones fit the mold of new-market disruption perfectly. Early car phones were clunky and unreliable and were only used where the competition was no telephone at all. Then little by little the technology became better, less expensive, and more reliable—to the point that today many people have cut the wireline connection, opting only to use wireless phones. From a relative business model point of view, however, wireless telephony was a sustaining innovation relative to the business model of wireline telephone companies. The wireless phone providers billed customers at a higher per-minute rate than wireline long-distance rates. The cellular network was built out along the freeways and commuting routes of the most attractive, least price-sensitive wireline customers. Nothing inherent in the technology forced it to be deployed within a business model that was attractive to the wireline companies; the wireless companies simply chose to do so.

After wireless telephony had grown to a point, it was attractive to the wireline companies such as SBC and Verizon simply to acquire wireless companies. The customers, margins, and revenues were all attractive, relative to their business models. Hence, although wireless technology is disrupting wireline technology, the profit model was not disruptive. The leading wireline telcos co-opted wireless telephony through acquisition rather than getting disrupted by it. This is not a literal replication of the model because the outcome of the leaders remaining atop the industry was not what we had generally expected. But it is a theoretical replication, because the model of disruption explains why the leading established firms now find themselves atop wireless telephony.

Because of instances such as this, I decided that labeling the phenomenon as disruptive technology was inaccurate. The technology did not make incumbent response difficult. The disruptive innovation in business models made it vexing, and I have subsequently sought to use the term *disruptive innovation*. Some authors in this issue are bothered by this, for reasons I do not understand. Refining the definition in this way has resolved several anomalies, such as the previous one. A disruptive innovation is financially unattractive for the leading incumbent to pursue, relative to its profit model and relative to other investments that are competing for the organization's resources.

The objective function is as yet unclear. Markides (this issue) cites my statement that disruption is a process, not an event, and he then asserts that quite possibly the best response of an incumbent is not to respond at all because there is no reason to expect the disruption to capture the entire market. Whether he is right depends upon the time horizon and objective function. Evidence he musters to support this assertion includes the fact that on-line brokerage has existed for 10 years, yet the full-service brokerages still have most of the market, as evidence that quite possibly the full-service folks need not worry about the disruption. Maybe this is true. But if Markides listed all the brokerage firms that populated financial centers in 1960, he would see that less than 20% of them have survived. The others were wiped out by the first disruptive discount brokerage, Merrill Lynch. Charles Merrill started his firm in 1912 at the bottom of the market (low net-worth individuals). Merrill grew among customers that were unattractive to the full-service brokers. As Merrill Lynch began reaching for higher net-worth investors, the incumbents happily retreated up-market until they found that there were not enough super-high-net-worth investors up there to sustain them all. One by one they disappeared. So yes, 10 years of discount Internet brokerage already have gone by—a lifetime for some people. And Merrill Lynch survives—because, although it disrupted most of the others, it has not yet been disrupted. It indeed is a process, not a cataclysmic event. This is an important improvement in definition.

Markides (this issue) similarly notes that low-fare airlines have been flying for more than a decade yet have only 20% of the market, implying that quite possibly, the right course of action for full-service airlines might be inaction. What he highlights is the need to make the objective function in this theory explicit. To illustrate, the market capitalization of Southwest Airlines (disruptor) exceeds the combined market capitalization of the largest seven U.S.-based full-service airlines (disruptees). Yet the majors have survived to this point. Minimill Nucor Steel's market capitalization exceeds that of US Steel, and though the other integrated mills all have gone bankrupt, US Steel has survived. The market capitalization of Dell (disruptor) now dwarfs the combined market capitalization of Digital Equipment, a disruptee that was swallowed by Compaq, a disruptee that was swallowed by Hewlett Packard, a disruptee that has not yet been acquired. Of the 316 full-service department store companies that existed in 1960, only a dozen have survived, though undeniably they have survived. Dayton Hudson is the only department store that launched a discount retailer (named Target) in a separate business unit. Markides might want to calculate the rate of return Dayton Hudson shareholders earned compared to those of the other department store companies. Toyota's annual profit now exceeds the combined profits of all other auto companies in the world, yet General Motors has survived.

Markides helped me realize I had simply assumed that the objective function of management should be to maximize shareholder value. If survival is instead the objective function, then quite possibly inaction is a good course of action.

Mistaken and Assumed Definitions

The academic literature has long framed questions of innovation in technological terms: Will this technology become better than that technology? As a result, a lot of people have framed disruption along these old lines, without noticing that disruption entails a very different question. Disruptive innovations do not necessarily improve to surpass the performance of the prior technology. They generally do not, and need not, as illustrated in the diagram on p. 16 of *The Innovator's Dilemma*. The trajectories of technological progress are parallel. They do not intersect. The salient question is whether the disruptive technology will improve to the point that it becomes good enough to be used in a given tier of the market. Analysis of tools like technology S-curves is interesting in its own right, because it can help us visualize the battles among alternative technologies as companies strive to move up the sustaining trajectory. But these constitute a fundamentally different phenomenon than disruption.

Resolving Anomalies through Improved Categorization

The second method for resolving anomalies is improved categorization. I quoted previously a paragraph from *The Innovator's Solution* that listed several anomalies for which the theory of disruption cannot account. We could add to this list of anomalies the fact that most (but not all) makers of carbeurators failed at the transition to fuel injection. None of the cotton spinners made the transition to artificial fibers; no slide rule maker made the transition to electronic calculators; Whole Foods seems to prosper at the high end of grocery retailing, unfazed by Kroger—even though in these cases the innovations have brought better products that could be sold for more attractive profit margins to the best customers of the incumbent leaders. Some have suggested that these are instances of high-end disruption. I resist labeling these phenomena as disruptions, because I am trying to give specific meaning to the term, independent of the outcome. Another mechanism of action causes the leaders to have missed these high-end innovations, and we should find another name for it. Finding an unambiguous name for this category is actually very important. If we find the right word for this different mechanism of action, it will help scholars such as Tellis (this issue) who have worried that disruption is defined post hoc to realize that their concern is unfounded. If we label the high-end phenomenon as disruption as well, people will make the post hoc mistake.

Though their work is not yet in working paper form, Brigham Young University professors Jeff Dyer and Hal Gregerson and Harvard Business School professor Dan Snow have independently concluded that the weight of these high-end anomalies is so heavy that another category of innovations must be out there. These are not low-end or new-market disruptions, as I have defined the terms, yet they seem to have had the similar effect of leaving the leader flat-footed, unable to respond effectively. This is how these scholars are presently framing the revision to the categorization scheme, though it surely must evolve as the concepts work their way toward publication. They see three categories:

(1) Innovations that are financially unattractive to the leading incumbents. These essentially comprise the low-end and new-market disruptions I have defined in my research.
(2) Innovations that are financially attractive to the leading incumbents. These comprise the innovations I have categorized as sustaining.
(3) Innovations that are unattainable to the incumbent leaders, because the technology or capital requirements are simply beyond the reach of the incumbent leaders.

I am quite hopeful that the work of Dyer, Gregerson, and Snow will constitute a substantive improvement in the theory, because they are using categorization to resolve anomalies. They will, of course, then need to manage the transition to normative theory by ensuring that we understand the mechanism causing incumbents to be unable to respond to this third category of innovations.

At least three of the articles in this issue propose different categorization schemes from the sustaining-disruptive dichotomy I used when the disruption theory was in the descriptive phase and the *financially attractive or unattractive relative to the business model* categorization undergirding the evolved theory today. Two of these are Slater and Mohr's analyzer–defender–prospector–pioneer categorization and Tellis's visionary/not-visionary typology. Though these might hold promise, we cannot judge whether they are superior to those I have proposed. Indeed, these authors are seeking to take us back to the descriptive theory stage, with categories defined by the attributes of the phenomena. I would love to see these authors, in their subsequent work on this topic, not just assert that their categories are improvements but cite specific anomalies for which the theory of disruption cannot account—and then show how their improved categorization explains all that the present theory can do but resolves the anomalies as well.

Markides (this issue) also proposes a different, more refined categorization scheme, some of which makes good sense to me. His notion that start-ups should create and that established firms should consolidate and grow is itself a circumstance-contingent normative theory; there is a lot of data to support it. This, for example, is precisely how Johnson & Johnson has grown over the last 15 years. But there are some quite apparent anomalies to this theory. Start-ups Ford, Compaq, Kodak, and Intuit created and scaled their disruptions to massive size. Established firms Teradyne and Hewlett Packard created disruptions and scaled to massive size as well.

Improved Explanation of the Causal Mechanism

By exploring whether the mechanism of failure is having the wrong information, the wrong customers, the wrong channels, or the wrong profit-model, Henderson (this issue) leaves us with a much-improved theory. I thank her for this article.

Upon reflection, the way I originally characterized the role of the customers of established leaders in disruption was a mistake. Resource dependence as it is enacted through the resource allocation process is the causal mechanism that makes it difficult for the leading incumbents to address a disruption. I have said that it was because the leaders listened to their best customers that they were not led to the disruptive technologies. Hearing and reading this, many people then asked, "So are you saying you should not listen to your customers? And what about von Hippel's (1988) discoveries about the role of lead users? Are you saying that lead users are misleading?" A more accurate prescriptive statement is that managers always must listen to customers. They simply must be aware of the direction in which different customers will lead them. A customer will rarely lead its supplier to develop products that the customer cannot use. The right lead customers for sustaining innovations are different from those for disruptive innovations. And the lead users for new-market innovations may not yet be users.

Do Revisions Discredit a Theory?

Some critics of my work seem to view the sorts of adjustments to the theory I have described here to account for anomalies as a weakness in the research. Those who see this as a weakness, however, do not understand the theory-building process. Modifying crudely articulated definitions of phenomena, categorization schemes, and causal mechanisms to account for anomalies is part and parcel to theory building—Kuhn's (1962) *The Structure of Scientific Revolutions* articulates this point well. The concepts that (1) there are two types of disruption; (2) disruption is a relative phenomenon; and (3) for predictive purposes disruptiveness should be measured relative to the business model rather than the technology cannot be found in *The Innovator's Dilemma*. They are improvements to the theory's explanatory power that have emerged as we have wrestled to account for anomalies.

Establishing the Validity and Reliability of Theory

A primary concern of every consumer of management theory is to understand where it applies and does not apply. Yin (1984) helps us with these concerns by defining two types of validity for a theory—internal and external validity—that help us gauge whether and when we can trust it. In this section I discuss how these concepts relate to our model of theory building and use them to assess the validity and reliability of the theory of disruption at this point.

Internal Validity

A theory's internal validity is the extent to which (1) its conclusions are unambiguously drawn from its premises; and (2) the researchers have ruled out all plausible alternative explanations that might link the phenomena with the outcomes of interest. The best way we know to establish the internal validity of a theory is to examine the phenomena through the lenses of as many disciplines and parts of the company as possible, because the plausible alternative explanations almost always are found by examining the workings of another part of the company, as seen through the lenses and tools of other academic disciplines.

When there is a possibility another researcher could say, "Wait a minute. There is a totally different explanation for why this happened," we cannot be assured of a theory's internal validity. Scholars who examine the phenomena and outcomes of interest through the lenses of all potentially relevant perspectives can either incorporate what they learn into their explanations of causality or can rule out other explanations so theirs is the only plausible one left standing.

I think this is a reason why my research was able to add value in the stream of research noted already. I examined the phenomena through the lenses of marketing and finance and not just the technological dimensions of the problem, which allowed me to see things that others had not seen before. Using this principle, Gilbert (2001, 2003, 2005) and Gilbert and Bower (2002) subsequently looked at the phenomena of disruption through the lenses of prospect theory and risk framing (Kahnemann and Tversky, 1984) and saw explanations of what had seemed to be anomalous outcomes in the online newspaper industry, for which the model of disruption at that point could not account. This has spawned a series of articles that have greatly improved our understanding of the causal mechanism and the precision of the categorization scheme. Similarly, Adner (2002) looked at this theory through the lenses of game theory and microeconomics and observed that when performance overshooting occurs, customers experience diminishing marginal utility. Using tools of mathematical modeling beyond my capacity, he framed the outcomes of the dilemma in terms of symmetric and asymmetric motivation. In clarifying how these trajectory constructs interact, Adner was able to resolve an anomaly for which I had not been able to account: Polaroid's success despite the fact that its product was highly attractive, relative to Kodak's profit model.

External Validity

The external validity of a theory is the extent to which a relationship observed between phenomena and outcomes in one context can be trusted to apply in different contexts as well. Many researchers believe a theory's external validity is established by testing it on different data sets. This can never conclusively establish external validity, however, for two reasons. First, researchers cannot test a theory on every conceivable data set. Second, data only exist about the past. How can we be sure a model applies in the present or future, before there is data on which to test it?

To illustrate this problem, let me recount my experience after publishing different versions of the theory of disruption in academic and practitioner journals. Those who read these early articles instinctively wondered, "Does this theory apply outside the disk-drive industry?" To address these concerns when writing *The Innovator's Dilemma*, I sought to establish the generalizability or external validity of the theory by testing it on data from as disparate a set of industries as possible—including hydraulic excavators, department stores, steel, computers, motorcycles, diabetes care, accounting software, motor controls, and electric vehicles. Despite the variety of industries in which the theory seemed to have explanatory power, executives from industries that had not yet been specifically studied kept asking, "Does it apply to health care? Education? Financial services?" After I had published additional articles that applied the model to these industries, the response was, "Yes, I see that. But does it apply to telecommunications? Database software? The German economy?" The killer question, from an engineer in the disk-drive industry, was, "It clearly applies to the history of the disk-drive industry. But does it apply to its future as well?" As these queries illustrate, it is simply impossible to establish the external validity of a theory by testing it on data. There will always be another set upon which it has not yet been tested, and the future will always lie just beyond the reach of data.

External validity can only be established through categorization. We can say that a normative theory is externally valid when the categories of circumstance are mutually exclusive and collectively exhaustive. Mutually exclusive categorization allows managers to say, "I am in this circumstance and not any of those others." And collectively exhaustive categorization would assure us that all situations in which managers might find themselves with respect to the phenomena and outcomes of interest are accounted for in the theory. No theory's categorization scheme is likely to achieve permanent status of mutually exclusive and collectively exhaustive, of course. But the refinements that come from cycles of anomaly-seeking research can asymptotically improve theory toward that goal.

Sample Size and Validity

Methods of measuring statistical significance show, of course, that the larger the sample size the more certain is the model's internal validity, because sample size affects measures of the statistical significance of coefficients in regression analysis. Some scholars also believe that a theory derived from a large data set representing an entire population of companies would have greater external validity than a theory derived from case studies of a limited number of situations within that population. But this is not true. When the unit of analysis is a population of companies, the researcher can be specific only about the entire population of companies. Some managers will find that following the formula that works best on average for the population also works best in their situation. However, sometimes the course of action that is optimal on average will not yield the best outcome in a specific situation. Hence, researchers who derive a theory from statistics about a population still need to establish external validity through circumstance-based categorization.

I found it interesting that Tellis (this issue), after disparaging my work, stated that he conducted a study of technology S-curves using a database of every technology in six industries. Although such an extensive database is laudable, it does not establish the external or internal validity of his research any more than if he had a census of data from one industry; internal validity comes from ruling out plausible alternative explanations, and external validity comes from getting the categories right. It is curious to me that Tellis would associate S-curves and disruption. Christensen (1992) showed that the S-curve construct cannot be used to describe disruption, because disruptions cannot be plotted on the same graph with the same metric of performance as the prior technology. Tellis seems unaware of this research, even though it is summarized in *The Innovator's Dilemma* and won the 1991 William Abernathy Award.

For these reasons, the fact that many dimensions of the theory of disruption are derived from case studies does not in any way detract from the theory's usefulness. These are, in fact, the source of its validity and strength.

Tellis (this issue) is critical of my sampling logic on the deductive side of the cycle. Sampling logic is very important in predicting the outcomes of things like elections. But urging the use of large-sample statistical methods to establish the external validity of this theory will not help this effort. The challenge is to get the categories right, and the method for doing it is simply to find anomalies and then to account for them.

Summary

At its foundation, this article is a description of what my students and I observed about how communities of scholars can build bodies of understanding that cumulatively improve. We have offered in our model of the theory-building process a set of constructs, labeled with terms such as *observation, categorization, association, anomaly, descriptive theory*, and *normative theory*. Their purpose is to abstract up from the detail of thousands of research projects—to offer a general description of the way productive research processes work.

Within this description of theory building, I also attempt to recount as a case-study illustration the process by which the theory of disruptive innovation has been built to date. It is quite apparent that this model has caused me to accept with gratitude some of the criticisms and suggestions authors of other articles in this issue have proferred and that it has caused me to dismiss, sometimes with impatient language, assertions that cannot plausibly lead to building better theory. Most importantly for the purposes of this issue, I hope this article shows that if a subsequent researcher uncovers an anomaly to a prior scholar's work, it represents triumph for both, because it will create the opportunity for them to improve the crispness of definitions, the salience of the categorization scheme, and the methods for measuring the phenomena and the outcomes of interest (Gilbert and Christensen, 2005). It will allow them to articulate better theory. When I have not accepted a criticism of one of these authors, it generally is because I have not been able to see a logical link to an anomaly; as such, it is impossible to tell whether they are offering a better theory. I would be honored to have them identify explicitly any anomalies the theory of disruption cannot yet account for and to suggest improvements, because I merely have hoped to set in place a solid enough foundation on which subsequent researchers can build.

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