

Disruptive Technology or Visionary Leadership?*

Gerard J. Tellis

Christensen's (1997) thesis of disruptive technology has been highly praised and popular with managers. Two of its premises are important and insightful. These deal with the performance path of a disruptive technology and its impact on dominant incumbents who ignore it in favor of listening to their current consumers. However, Christensen's thesis also suffers from limitations, two of which are troubling: ambiguity in the definition of *disruptive technology* and the logic of the sampling to test its validity. Several studies my colleagues and I have conducted over the years suggest that the disruption of incumbents—if and when it occurs—is due not to technological innovation per se but rather to incumbents' lack of vision of the mass market and an unwillingness to cannibalize assets to serve that market. We have developed metrics to test these concepts. Models to predict the outcomes and financial value of strategic changes that firms can make to avoid these problems.

With his concept of disruptive technology, Christensen (1997) made a highly praised and critical contribution to the strategy literature. The impact of his thesis has been enormous both in the business and academic communities. For example, a search of the term *disruptive innovation* yields over 150,000 thousand entries on Google, much higher than for rival terms such as radical innovation (58,000) or architectural innovation (16,000) or competence-destroying innovation (55). Danneels (2004) reported that Christensen's book has sold over 200,000 copies. These are impressive statistics for any theory or concept in strategy. Thus, it is with some hesitation that I attempt to comment on this thesis or to suggest an alternative.

Address correspondence to: Gerard Tellis, Marshall School of Business, University of Southern California, Los Angeles, CA 40089-1421. E-mail: tellis@usc.edu.

*The author thanks Rajesh Chandy, Erwin Danneels, Peter Golder, Ashish Sood, and Jaideep Prabhu for valuable comments on earlier versions of this paper.

This article summarizes Christensen's (1997) thesis in five important premises, many of which can be further divided into subpremises, shown as letters in the following points:

- (1) A new disruptive technology initially underperforms the dominant one along the dimensions mainstream customers in major markets have historically valued.
- (2) But the disruptive technology (a) has other features a few fringe (and generally new) customers value. Products based on disruptive technologies are typically (b) cheaper, (c) simpler, (d) smaller, or (e) more convenient than those established on the dominant technology.
- (3) (a) The leading firms' most profitable customers generally do not want and indeed initially cannot use products based on disruptive technologies. So (b) disruptive technologies are first commercialized in emerging or insignificant markets. (c) Incumbents conclude that investing in disruptive technologies is not a rational financial decision for them.
- (4) The new disruptive technology (a) steadily improves in performance until (b) it meets the standards of performance demanded by the mainstream market.
- (5) At that point, (a) the new (disruptive) technology displaces the dominant one and (b) the new entrant displaces the dominant incumbent(s) in the mainstream market.

One valuable insight in Christensen's (1997) thesis is the observation that the disruptive technology initially underperforms the dominant one on dimensions the mainstream market demands, but with steady improvements it meets or exceeds those demands. A second valuable insight is that dominant incumbents are displaced even though they did what generations of strategists and the basic philosophy in marketing says

they should: listen to their (mainstream) customers. Even though these two insights contradict received theory and common intuition, Christensen (1997) cited persuasive examples in support of them.

Though the thesis of disruptive technology is interesting, has broad appeal, and persuasive examples in support, it seems to suffer from some weaknesses. The importance of the thesis dictates that researchers study it carefully. Danneels (2004) provided a relatively comprehensive list of the limitations and a critical review of the literature it spawned. This article briefly discusses two of these limitations that have troubled me: the definition of the term *disruptive technology* and the sampling for its empirical validation.

Consider first the definition. Danneels (2004) suggests that several authors seem to think that Christensen (1997) did not provide a precise and consistent definition of the term *disruptive technology*. At the narrow end of the spectrum, the concept could be defined in terms of only the first of the aforementioned premises: that the new technology underperforms the dominant one. But then the issue is whether there is sufficient information to distinguish the disruptive technology from the many other underperforming technologies that small and large competitors in the market are constantly introducing. What makes any one of these underperforming technologies disruptive? How can we pick up a priori the underperforming technology that will end up being disruptive? At its broadest, the definition could encompass all of the five premises given. That is, a disruptive technology is characterized by all five premises. In that case, the definition itself encompasses the two most interesting testable premises: that the disruptive technology meets the standard of performance demanded by the mainstream market (4b); and the new entrants displace the dominant incumbent (5b). As such, the term is well defined but has little predictive value.

A key issue is whether the definition includes the fourth or the fifth premises. If it includes the fourth premise, a technology would be disruptive only if and when it comes from behind to meet or surpass the existing technology in performance. If so, then the concept begs the question, What are characteristics of such disruptive technologies so that managers can identify them ahead of time and can plan accordingly? The problem in the definition lies in the term *disruption*, which is at once a characteristic of the innovation and its most interesting and valuable prediction. The confusion that ambiguity in definition causes may be appreciated when reading Henderson's (this issue)

analysis of the energy bar market. Henderson suggests that the energy bar (targeted at athletes) is an innovation relative to the candy (targeted to the mass market). She concludes, "It's probably too early to tell if energy bars are 'disruptive innovations' in Christensen's sense." But if one must wait till the disruption has occurred, then what predictive value is there in the concept?

Next consider the issue of sampling. In his 1997 work, Christensen (1997) includes an in-depth analysis of the disk-drive industry for internal validity and examples from many other markets for external validity. The key issue here is whether these examples are for inductive purposes (to build the theory) or for deductive purposes (to test the theory). If it is the former, then the logic of sampling is not critical. However, if it is for the latter, then the researcher needs to justify how and why the examples were chosen. More generally, a deductive test should include the logic for the sampling of markets and the logic for the sampling of innovations within markets. The purpose of spelling out the logic of the sampling is to establish that the empirical test is not biased in favor of the proposed theory. While I find the empirical examples in Christensen (1997) quite persuasive, I cannot say the same for the logic of his sampling.

Formal Test of S-Curve of Technological Evolution

Christensen's theory is related and bears some similarity in implicit propositions with another concept or thesis espoused by other researchers of strategy: Technological evolution follows a distinct pattern (e.g., Foster, 1986; Utterback, 1974).

According to their views, when performances of successive technologies are plotted over time or effort, they appear as successive S-curves, each new one starting below but ending above the level of the prior technology and crossing the prior technology just once. This theory also seems to suffer from the problems of definitions and sampling discussed already. Ashish Sood and I (Sood and Tellis, 2005a, 2005b) designed a study that attempted to test the thesis of S-curves while minimizing the previously mentioned two problems. I provide synopsis of this study, because the results have implications for the concept of disruptive technology.

To begin with, we defined innovations and the ensuing technologies strictly in terms of their

characteristics and not their effects and then evaluated their performance on a variety of dimensions. First, we defined platform, design, and component innovations as follows. *Platform innovation* is the emergence of an entirely new scientific principle to solve a problem, which is distinctly different from the current scientific principle used to solve the same problem. For example, the CD used a new scientific principle—laser optics—to write and read data where the prior products (VHS) used magnetism. We reserve the term *technology* for a means of solving a problem based on a distinct platform or scientific principle. We subsequently use the term *technology* synonymously with technological platform or platform. *Component innovation* is the use of new parts or materials within the same technological platform. For example, magnetic tape, floppy disk, and zip disk differ by use of components or materials, although all are based on the platform or technology of magnetic recording. *Design innovation* is a reconfiguration of the linkages and layout of components within the same technology. For example, floppy disks decreased from 14 to 8 inches in 1978, to 5.25 inches in 1980, to 3.5 inches in 1985, and to 2.5 inches in 1989, although each was based on the technology of magnetic recording. By these definitions, performance improvements within a technology come from design or component innovations.

Second, we identified and analyzed the performance of every technology in six markets. We chose these markets to ensure a mix of previously studied and entirely new categories. We did so to ensure each had a sufficiently large number of innovations so as to study the pattern of changes: external lighting, data transfer, computer memory, desktop printers, display monitors, and analgesics. Within these six markets, we identified 23 technologies and tracked their performance on key dimension plus several alternate dimensions. With regard to the thesis of S-curves, the key results are the following (see Sood and Tellis, 2005a, 2005b for details):

- Technological performance generally does not follow a single S-curve. Rather, it seems to follow a multiple step function, with big random improvements frequently following long periods of dormancy. We found only six technologies followed a single S-curve.
- The path of technological change seems essentially random. This result applies especially to the start of new technologies, to the height of their jumps, and the timing of their jumps.

- A new technology does not always start below or end above the prior technology.
- Even after a new technology crosses a prior technology in performance, the prior technology could recross the new one and end up higher. For example, in the desktop memory storage market, on the primary dimension of recording density, the new technology of optical storage started above magnetic storage, but the latter soon caught up with it. Then optical storage went ahead, but now magnetic storage is way ahead.

The critical importance of these findings is that the S-curve is not a predictive theory and thus not a good basis for strategy. For example, a manager seeing a plateau in performance may wrongly assume that the technology has matured and so abandon it. We found that huge performance jumps often follow such plateaus.

Thus, overall, we found little support for the thesis of the S-curve of technological evolution. Our study also has the following implications for the thesis of disruptive technology, though uncertainty about its definition has prevented us from carrying out a formal test of the thesis.

- It is very difficult to predict the path of technological change. We do not see the straight linear patterns with constant slope that one sees in examples of disruptive technology.
- In about two-thirds of the cases, the new technology starts below the prior one in performance on the primary dimension but is superior on a secondary one. (Perhaps these characteristics may meet two of the criteria of what Christensen [1997] calls disruptive technologies). Some of these technologies then proceed to surpass the existing technology in performance, even on the primary dimension. This finding provides some support to Christensen's fourth premise.
- Where new technologies are superior to the existing one, the dimension of superiority may not be price, size, convenience, or simplicity. Indeed, in the vast majority of cases, the secondary dimension is some other one than these four. This finding does not support Christensen's second premise.
- Contrary to popular beliefs, small new entrants are not the only ones that introduce new technologies. Both small and large firms and incumbents and new entrants come out with new technologies.

A little less than half of the new technologies are from large incumbents, whereas the remaining technologies are equally divided between small new entrants and large new entrants.

Thus, contrary to a widely held belief that new technologies come from outsiders, we (Sood and Tellis, 2005a, 2005b) found cases of large incumbents championing new technologies and cases of large incumbents being displaced by small firms and new entrants who championed new technologies. In another study of over 90 innovations, both radical and incremental, Rajesh, Chandy and I (Chandy and Tellis, 2000) found a similar pattern of results about incumbency, size, and radical innovation.

Computing such results for the thesis of disruptive technology would depend on how one defines the term. Assuming one adopts a nontautological definition encompassing just two or at most three of the premises at the outset of this article, then support for the thesis would not be strong.

Technologies versus Visionaries

The key issue is, why do some incumbents thrive on technological change while others succumb to it? I posit that the answer lies in visionary leadership and the will of the leader to execute that vision. This is a theme Peter Golder and I (Tellis and Golder, 1996) first espoused in 1996 and then articulated in a book with extensive examples (Tellis and Golder, 2001). Our sample covered over 66 consumer markets, some going back to over 100 years. Our method was historical or archival analysis of markets as they evolved (see Golder and Tellis, 1993; Tellis and Golder, 2001 for details). The essence of our findings is that some firms last for decades and even a century. Other highly successful and established incumbents self-destruct or decline into oblivion. The reason is not order of entry or technological change or brand loyalty; it is primarily the leadership within those firms. Long-term market leaders focus intently on future emerging mass markets. They innovate relentlessly to cater to that emerging market (e.g., Procter & Gamble) and are paranoid about competitors getting there first (e.g., Intel, Microsoft). Most importantly, they are willing to cannibalize their current assets to realize that future potential (e.g., Gillette in the wet shaving market or Procter & Gamble in detergents). In contrast, dominant incumbents that succumb to technological

change are content with past successes, are disdainful of new entrants, focus on current products or current customers, and are highly unwilling to cannibalize current assets and products to build future markets (e.g., Xerox in the 1970s and 1980s).

A critic may ask whether our thesis of visionary leadership is subject to the criticism of hindsight bias. Can it be operationalized by objective measures and tested formally? In another study, Chandy and I (Chandy and Tellis, 1998) indeed did so. We used a multi-item scale to measure key constructs such as future market orientation (a version of *vision*) and willingness-to-cannibalize (a version of *will*). Our goal was to examine the effect of these constructs on the level of radical innovation in a large sample of over 300 firms. The results show that will and vision need not be circular concepts used in vacuous claims. Rather, firms' future market orientation positively drives their willingness-to-cannibalize assets, which in turn drives radical innovation. Other explanatory variables that also turn out to be important are the level of risk seeking, the presence of product champions, and internal competition. In the study, we refer to these variables jointly as the *internal culture* of the firm. In an extensive replication of this design to over 700 firms in 17 countries of the globe, we found that such internal cultural characteristics of firms do indeed trump current metrics for explaining radical innovation, such as research and development (R&D), patents, or the external culture of countries in which they operate (Tellis, Chandy, and Prabhu, 2005).

A critic may also ask whether our thesis of internal culture has any predictive validity. A fair prediction of a model requires estimation of the model on one sample and prediction for a holdout sample. Such predictive analysis should show the hit rates, derived from how well the predicted values match or fail to match with the actuals. When dealing with large cross-sectional samples, we developed a type of jack-knifing approach to prediction, where we estimated the model on the whole sample except for a target firm and then predicted the performance of that firm using the parameters of the model and the firm's own independent variables (see, e.g., Golder and Tellis, 1997). Using this approach, an ordered logit model, and the internal cultural characteristics of firms, we were able to predict the level of radical innovation accurately for about 34% of the sample. With an error rate of + or – 1 in the level of predication of radical innovation, our cumulative hit rate jumped to about 58% (Tellis, Chandy, and Prabhu, 2005). These are not

outstanding hit rates, but they are a start and better than not having any scientific means of prediction at all.

Finally, a critic might ask what the practical implications are of adopting changes suggested by our model. Jaideep Prabhu, Chandy, and I (Tellis, Chandy, and Prabhu, 2005) this path to radical innovation can indeed lead to superior financial performance for the firms that adopt it, in terms of superior market-to-book ratio. Using this model and the data on the 700 firms, we can now value changes a firm might make in internal culture by benefits derived in better market-to-book ratio.

Conclusion

Our findings on technological change challenge unqualified faith in law-like generalizations such as the S-curve of technological evolution. In contrast to that theory, we found that technologies do not evolve along S-curves, do not cross in performance only once, and do not always start below and end above the prior technology's level of performance. Rather, performance paths of rival technologies follow irregular step functions, may never intersect, or may intersect multiple times. All these results have important implications for strategy.

None of these results detract from two insightful contributions of Christensen's (1992) work. In particular, the danger of focusing too tightly on current customers and the risk of ignoring technologies that currently appear inferior are important lessons he has demonstrated with persuasive examples. With our data, Christensen's thesis could be formally tested once a precise definition of disruptive technology is adopted. We have not yet done so. However, our study of 23 technologies across six markets leads us to doubt his thesis. For example, contrary to premises of his thesis, we find that the performance path of most of the technologies in our sample is neither linear nor easily predictable. Rather, it is punctuated by irregular jumps in performance. Also, contrary to some premises of the thesis, the secondary dimension is not always price, size, convenience, or simplicity. Most importantly, technological transitions per se do not seem to drive the success or survival of firms. Large incumbents were almost as innovative as new entrants in terms of introducing new technologies.

In addition to disconfirming some widely held beliefs, our work emphasizes that technological and

market evolution are important forces managers must confront and analyze. However, the research my colleagues and I have done suggests that success and failure are unlikely to be deterministic outcomes of inanimate technologies, whether they are radical, revolutionary, or disruptive. Rather success and failure are probably the result of internal cultural aspects of the firm. Important among these is visionary leadership that embraces change and is willing to cannibalize existing assets to serve customers with new technologies. It seems that several of the other articles in this special issue echo this theme. Thus, the concepts of organizational competence and cognitive framing (Henderson, this issue), strategic orientation (Slater and Mohr, this issue), and customer orientation and willingness to cannibalize (Govindarajan and Kopalle, this issue) all speak about the internal mindset or culture of the firm rather than of external technological forces as drivers of success or failure.

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