Commentaries and Rejoinder to "Does Quality Win? Network Effects Versus Quality in High-Tech Markets"

Quality May Not Win

*BRIAN T. RATCHFORD**

A substantial body of theoretical literature indicates that network effects may hinder the entry of higher-quality products into markets in which network effects are important. However, Tellis, Yin, and Niraj (2009) provide compelling evidence that, in general, higher-quality offerings win out in software markets after a short time lag. Because software markets are commonly believed to be susceptible to network effects, this finding provides important empirical evidence against the hypothesis that network effects impede entry. Because Tellis, Yin, and Niraj obtain their results across a large number of product categories and because their analysis holds up across various methods, their evidence that high quality trumps network effects is impressive.

However, in the final section of the article, Tellis, Yin, and Niraj are careful to provide a set of limitations for their research. Because I believe that their results must be qualified in the light of these limitations, I elaborate on some of these in my comment. Because the authors have gone about as far as possible with the data at their disposal, this comment is intended to stimulate further research on the topic of network effects and quality.

Consistent with Tellis, Yin, and Niraj's research objectives, their conceptual model focuses on the demand side and factors that might affect consumer response, but the supply side is also important. In particular, it is not known whether the firms refrained from developing or marketing products because they judged that network effects were too difficult to overcome. Thus, the results are subject to a sample selection problem, in which only products that suppliers believed to be worthy of introduction on the market were selected. Because the sample is limited to cases in which suppliers believed that introducing the product on the market was justified in the face of any network effects, this creates an unknown bias toward showing that quality can overcome network effects.

A related consideration is that suppliers have ways of dealing with network effects or even using them to their advantage. One is to make the higher-quality product compatible with its predecessor, such as making Excel compatible with Lotus. Another is to arrange to have software bundled with the sale of new computers, thus forcing its acceptance in the market. For example, a current buyer of a Windows computer must either accept Vista or have someone uninstall this software in favor of an older version, a time-consuming and expensive process. This bundling may have facilitated the adoption of Windows, Word, Excel, Internet Explorer, PowerPoint, AOL, and possibly other software types. Supplier actions to mitigate network effects, such as compatibility and bundling, do not invalidate the general findings in Tellis, Yin, and Niraj's article. Rather, they may help explain how and why network effects can be overcome.

However, consistent with the sample selection issue outlined previously, the inability to overcome constraints imposed by the need for compatibility or the need to bundle software with new computers to foster acceptance may preclude higher-quality offerings from reaching the market. For example, potential suppliers may refrain from innovating because they know that computer manufacturers are unlikely to install their innovative software on new computers.

There may be reason to question the nature and extent of the network effects present in the markets Tellis, Yin, and Niraj studied. They define these effects as "the increase in a consumer's utility from a product when the number of other users of that product increases" (p. 135). Consistent with this definition, they find that market shares tend to increase with the cumulative share of a given product, a measure of network size. Although this finding is consistent with utility increasing with network size, other explanations for the finding are also plausible. For example, early in the life cycle, a positive relationship between demand and network size would result from diffusion due to word-of-mouth communication (Bass 1969). The positive relationship between demand and network size could also be due in part to bundling of software into new computers, which creates passive acceptance of the high-quality item. In summary, there are alternative explanations for the evidence about network effects that Tellis, Yin, and Niraj present.

It would be useful to have more direct evidence for these effects—for example, how the utility of a piece of software increases with the installed base. Although the ability to exchange files or programs might be one possibility, differ-

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ent generations of software are usually compatible so that files created by one type can be read by another, at least by users of the same operating system. Thus, the benefits of file or program exchange would seem to be limited in most cases. There may be benefits to standardizing software within an organization that would create network effects, at least for those in the organization. Another potential source of network effects is that developers may not develop programs to use with a given piece of software without a critical mass of users of that software.

The latter effect would seem to be strongest for operating systems, and problems due to incompatibility of programs and file exchange would also seem to be most prevalent for operating systems. Thus, network effects might be expected to be strongest for operating systems. Among operating systems, Windows and its predecessor, DOS, have been dominant for more than 25 years, despite not being demonstrably superior to alternatives, such and Macintosh and Linux. It is likely that availability of programs that run on Windows and compatibility with other Windows computers has played a role in this. This might be regarded as a counterexample to Tellis, Yin, and Niraj's finding that quality dominates network effects.

In summary, although Tellis, Yin, and Niraj present strong evidence that quality wins in markets that are subject to network effects and though they also maximize the information that can be obtained from their data, the evidence that network effects do not create a lasting impediment to higher-quality products is still incomplete. The data include only products that were placed in the market, which means that the sample is skewed toward cases in which managers believed that network effects could be overcome. There could be cases in which managers shied away from developing products because they believed that network effects were insurmountable or that they could not engage in actions needed to overcome them, such as getting their software included in original equipment. Moreover, the degree to which the markets in question are actually subject to network effects is unclear. Of the markets studied, operating systems seem most likely to be a source of network effects. The continued dominance of Windows over alternatives in the operating system market may be a counterexample to the results.

Further research might examine the process by which software is adopted to better understand the role of bundling of software with new computers and the role of corporate policies of adopting uniform types of software. There is also a need to examine further whether network effects really are important for the different types of software. In cases in which the network effects are important, there is a need to understand whether these create a barrier to developing new products that may preclude some suppliers from competing in these markets. The study of such issues will likely require survey data that go beyond the data employed in the current study.

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Ignore Successful Followers—Entry Is Still Urgent

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WHAT SHOULD FIRMS DO?

Network effects seem undeniable (Katz and Shapiro 1994; Lieberman and Montgomery 1998). Certainly, enjoying a telephone is difficult when there is no one to call. The user network gives the telephone its utility. Moreover, network effects could ensure future compatible spare parts, ancillary products, and upgrades.

Tellis, Yin, and Niraj (2009) survey the literature and find controversy. Their empirical analysis finds that network advantages for early market entrants are weaker than previously believed and that sufficient quality can overcome these advantages. This important finding complements previous findings (e.g., Golder and Tellis 1993) and provides many useful implications. However, it is misleading, and possibly disastrous, to conclude that a speedy market entry is unimportant. Trying to be first is not only worthwhile but also sometimes essential for some, if not most, organizations.

Even if public policy makers and frustrated maladroit competitors seeking to capture the gains of so-called lucky early entrants, as well as incumbents trying to discourage competitive entry, all vastly exaggerate the incumbency advantage in free markets (which I believe happens), striving for early entry is still important. The question facing a firm is not whether early entrants have a persistent advantage or whether early entrants earn a greater profit (which is unlikely) than later entrants, but rather whether the firm should strive for an early launch. The firm's choice is not between early entry and a more profitable later entry, but rather between allocating more or less resources to immediate new product development efforts. Front-loading resources might enable an early launch but does not necessarily guarantee it. Delaying resource allocation might increase the likelihood of facing formidable later entrants, entirely forgoing market entry, squandering the firm's unique resources, and never launching successive extensions.

WHY ENTERING EARLY IS CRITICAL DESPITE NO APPARENT ADVANTAGE

Few Olympic athletes run races with the explicit goal of finishing second and taking the silver medal. Such complacency would likely result in no medal at all. The athlete who actually finishes second might have expended as much effort as or more effort than the athlete who finished first. Finishing second is an outcome, not the decision itself. Similarly, entering the market earlier with a new product is not the decision itself, but rather a possible outcome of decisions made much earlier.

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In free markets, firms strive to enter markets with new products that at least some consumers prefer over extant alternatives. Such preferences can be interpreted as the consequence of quality, quality per dollar (Hauser and Shugan 1983; Shugan 1987), features, value, reliability, durability, versatility, and so on. However, free markets evolve with advancing technology (Shugan 2004), improving productivity, accelerating rates of innovation, increasing knowledge, new capabilities, and ever-better communications. Each successful new entrant exploits this evolution to achieve still higher levels of consumer satisfaction than the last entrant. Consequently, almost by definition, the required threshold for success steadily increases. It becomes increasingly more difficult to surpass the last entrant because each entrant raises the threshold for success. The longer an organization waits to enter a market, the higher is the threshold of customer satisfaction required for successful entry. For example, few users of today's telephones would be happy with the telephones of a decade ago. This is the implication of the quality argument in Tellis, Yin, and Niraj. Higher quality overcomes network advantages, but it also raises the thresholds for future entrants.

If the threshold for success is always increasing, delay can be deadly. The difference between the market share or profits of an early and a successful late entrant is an inadequate, a misleading, and an inappropriate measure of the advantage of being earlier. The successful later entrant must have achieved a higher threshold of satisfaction to be successful. The appropriate comparison for an organization making a launching decision is whether to proceed at an economical pace or to devote more resources toward expediting the development process. There is no guarantee that the leisurely economical pace will yield the often-observed successful late entries. A lack of urgency might result in falling far short of the required threshold for a viable entry. The observed successful late entrant might be the fruits of a highly resource-intensive development effort by organizations that might have failed at being first but still stressed urgency. There are many examples of deadly delays resulting in disaster despite many successful late entrants. For example, the San Jose–based 3dfx company, the former king of the video card market, declared bankruptcy after being several months late to market with its next-generation cards. Subsequent leadership among the surviving firms, ATI and NVIDIA, alternated depending on which firm was first to market with each successive generation. The 3dfx company quietly left the market with many new products still on the drawing boards. These never-launched potential late entrants are not observed in failure rates.

Thus, just because a successful second entrant has much greater sales, it is unwise for a company to conclude that it would be such a second entrant if it waits. There is no guarantee that a leisurely effort will spawn such a propitious outcome. Observed late entrants might not be typical. Remember, all the would-be second entrants that are never able to develop viable new products that achieve the required threshold are not observed. Likewise, all the would-be second entrants that abandon all efforts and never try to develop a superior offering are not observed. Development in a rapidly evolving economy is a matter of urgency.

Finally, if the threshold for entry is unrelentingly increasing, waiting longer raises the required threshold, thus shortening the expected life cycle for any given level of quality. Optimal entry depends on whether development efforts are advancing at a much more rapid pace than the pace at which improved products are entering the market (Shugan 2004). Moreover, the option of an immediate launch must also be considered, while leaving open the possibility of future product improvements (Chandy and Tellis 1998, 2000) against the option of having no presence in the market at all.

Thus, less enduring advantages for the incumbent (network effects, location effects, preference effects) imply shorter life cycles and an ever-increasing threshold that successful new products must achieve. The faster a firm can get its products to market, the lower is the threshold the products need to achieve and the longer is their effective life cycles before the market catches up.

DIFFERENT FIRMS HAVE DIFFERENT BEST STRATEGIES

Marketing scholars are far too quick to dismiss marketing projects as merely simple financial investments. Financial investments might be interchangeable. For example, investors might be indifferent between investing in projects with the same distribution of payoffs and expenditures. A security enjoys the same rate of return regardless of who buys it. New product projects lack this property. They are not fungible financial securities. The outcome of new product projects is often dependent on who executes the project.

For example, a liquor company might expect greater payoffs than an entertainment firm might from launching the same new beverage. Firms have different resources, expertise, employees, knowledge, and experience. Firms are not interchangeable. At a minimum, firms start with different brand reputations (Aaker 1997) and different histories. These differences change the distribution of outcomes for the same projects. Even two motion picture production companies might have different degrees of success with exactly the same script because each has access to different directors, contractual agreements with other talent, and distribution opportunities.

Empirical data are consistent with this conclusion in the area of publication. Academic authors who are the first to study an important research problem face a different peer-review process than scholars who publish in well-established areas. It is not necessarily easier to do either. Each requires different skills, resources, publication strategies, and (possibly) reputations. In any case, the opportunity cost of publishing an article is not the average difference between these two outcomes (which might be zero). It is the difference between trying to publish the first article in a new area and trying to improve an existing area.

Similarly, some nimble organizations might be good at developing new markets with pioneering technology. Other established organizations might have an advantage at understanding market segmentation and differentiating products on the basis of design rather than cutting-edge technology. Still other streamlined organizations might have the advantage of securing lower costs for a mass market. Each organization might have different innovative expertise in different areas that gives them different out-

comes for the same project. Some might be better pioneers, and some might be better late entrants. Moreover, a great pioneer might be an incompetent late entrant.

LATE ENTRY MIGHT BE MORE RISKY THAN EARLY ENTRY

Still another reason many firms should continue to strive for early entry despite little apparent advantage is that for many firms, early entry is far less risky than a later entry. Remember that most empirical studies have inferred risk by examining failure rates. Failure rates reflect risk when other factors are constant, but other factors—in particular, the accuracy of market research—vary over time. Perhaps market research is more difficult for early entrants because potential buyers are unable to envision very new concepts. Perhaps forecasting production costs are more difficult for early entrants because of unforeseen production problems. Perhaps predicting distribution, advertising effectiveness, and buyer responsiveness to different prices are all more difficult. These difficulties might produce more incorrect early launch decisions, thus resulting in failure. However, these difficulties can also make failure rates a poor measure of risk because these difficulties diminish over time.

Later entrants enjoy better predictions and thus might be less likely to make launching mistakes. As a result, later entrants might avoid observed market failures by suspending development efforts. However, this fails to imply that later entry is easier or less risky. Later entrants must still expend considerable resources to exceed the threshold established by the incumbents. Moreover, later entrants face a moving target as the threshold continues to increase as still more firms enter the market. Thus, would-be later entrants might never enter the market. After expending considerable resources, these firms might never achieve a viable product.

Empirical observation might exclude many of these firms that eventually abandon their efforts to launch new products after never succeeding in being first. For example, academics in the business of producing research abandon research topics after painfully observing the unintentionally preemptive research presentations and the recent publications of others. Sometimes, the peer-review process at scholarly journals performs that dismal function. When a firm is beat to market by a competitor, it can either abandon its efforts or attempt further improvement to meet the higher threshold created by the recent new entrant. Although prior efforts are sunk costs at this point, subsequent efforts to meet the higher threshold incur additional costs that might diminish the eventual profitability of the entire effort. Even when the firm subsequently launches and achieves the same market share as the first entrant, the eventual costs might exceed the costs of the first entrant.

In summary, although Tellis, Yin, and Niraj compellingly demonstrate that the first mover remains vulnerable to a second entrant, this important finding does not necessarily imply that a strategy of being second is any less difficult than being first. It is unclear whether in a free market the risks of early entry exceed those of later entry. At least for some, the risks of waiting might be greater. Going back to the Olympic athlete example, the athlete who achieves second place may have expended more effort than or the same effort as the athlete who achieved first place. The correct comparison is between the option of initially spending more resources in an attempt to be first and the option of spending fewer resources with a lower probability of being first. Remember, a complacent organization can easily find itself in the position of having only obsolete products and insufficient new products in the pipeline to survive. The organization quietly disappears without influencing failure rates.

Finally, empirical observation appears to reveal more new product launches in the early stages of a market's development, implying that fewer organizations tried to launch during the market's decline. Thus, failure rates are unable to capture completely the concept of risk.

CONCLUSION

Great care must be exercised when inferring theories from observation (Shugan 2007). Early entry might still be the best goal for many organizations, even if late entrants earn much greater profits (which is unlikely). There are several reasons for this conclusion:

1. Firms that enter later might have invested as much as or more than early entrants. Higher thresholds are required for a later entry. Moreover, late entrants might have tried and failed to be first.
2. Because it is easier to make predictions in more mature markets, many firms might have abandoned their efforts to enter mature markets, resulting in deceptively low failure rates for mature markets.
3. Given many mature markets with unobserved failures (i.e., projects never consummated with observed entry), the rate of return for late entry might be less than that for early entry, despite many observed early failures or a lack of an enduring advantage for early entrants.
4. Many organizations that failed to achieve the higher thresholds required for a later entry might not be observed.
5. Early entrants enjoy a monopoly period during which they face less competition than late entrants.
6. Some firms are more capable of executing successful early entries, while other firms are more capable at entering more mature markets.

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A Broader Perspective of Network Effects

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Tellis, Yin, and Niraj (2009) address the topic of whether network effects can overwhelm quality, thus enabling companies with strong network effects to dominate markets. This is important because it would imply that being first to market enables a firm to develop a strong network, thus making it difficult for competitors, even those with superior products, to penetrate the market. The network effects would be a strong competitive barrier to entry. However, Tellis, Yin, and Niraj find that this is not the case. Product quality appears to dominate, and networks are not sufficient barriers to prevent competitors with superior-quality products from rising to the surface. The authors have done a good job of addressing an important issue.

However, there are a few other factors that should be taken into consideration in this discourse. In particular, it is important to begin by delineating what is meant by "network effects" and which effects are being captured in this study. Furthermore, the other factors that play a role in determining market share shifts must be recognized, and how they might be directly or indirectly influenced by product quality and network effects should be examined. Next, product quality and network effects are treated as two separate, independent variables, but I believe that they are not independent. This may cloud the conclusions. In addition, I explore the strategic inferences of the finding that product quality dominates network effects. Finally, I consider the conditions under which the "quality prevails" conclusion are expected to be true.

DECOMPOSING THE NETWORK EFFECT

Given that the central focus of their study is on product quality and network effects, Tellis, Yin, and Niraj view market share shifts as a function of these two variables. However, they never conceptually define network effects, though they operationalize it in accordance with standard economics.

Conceptually, network effects occur when the size of the network, or the number of users, makes the product more valuable for subsequent users. The network serves as a product enhancement. Yet the network of customers can affect subsequent sales beyond just product enhancement. In this brief comment, I try to expand the understanding of network effects. Network effects could be actualized through product enhancement, communications, or distribution and perhaps even in price, thus rounding out the complete marketing mix.

There are several different meanings of network effects; in other words, the effects could be manifested in different ways:

1. Network effects are often viewed as the interdependence one user has on another, given that they are both users. For example, early applications of network effects began with telegraphs and the telephone company, and the value of a product was in the network that was connected. Thus, the more people who are part of the network, the more valuable the product is.
2. In today's world, accelerated by the ease of communicating through the Internet, there is rapid diffusion from one user to the next of information about the product, or contagion. This communication from one customer to the next, whether explicit or implicit, in which one customer views another customer using the product, can influence adoption by others. In a controlled experiment on "hit" music, Salganik, Dodds, and Watts (2006) find that the awareness of what other customers were selecting had a significant impact on preferred choices.
3. Beyond product enhancement from a broad customer network is the distribution effect. As demand swells, so does distribution. Network effects could be construed as capturing the network of distributors. More distributors will enhance the availability of the product for the customer and will increase some interdistributor competition to sell the product, often driving prices down.
4. As the network grows, the total volume grows, resulting in experience curve effects that are often passed through in the form of lower prices for the customer.

Each of these elements might influence the intertemporal effect of market share on the next period. As network effects are traditionally operationalized, and as used by Tellis, Yin, and Niraj, all four elements, and perhaps more, are subsumed under the term "network effects." Which of these elements the authors are referring to is never addressed. I suspect that there are different network effects, and it would be worthwhile to determine where the effects are occurring—that is, how much is driven by the traditional concept of network effects, and how much is driven by other factors. Undoubtedly, some effects create a greater competitive barrier than others. For example, if there were a category in which the product is enhanced by the size of the network, this would make it more difficult for competitive entry. Similarly, if it was a price or distribution effect, competitors would be at a disadvantage, regardless of product quality. Conversely, if there were a product in which the network effects are those of communications, the network effects could work favorably for the new product with superior quality. Indeed, Tellis, Yin, and Niraj demonstrate the positive influence that results from this communication or is signaled by the quality-sensitive segment.

The measure of network effects in Tellis, Yin, and Niraj's study is the "accumulated market share of a brand in the last three years" (p. 139). This is a common economist operationalization but a relatively narrow view of network effects. This is not Tellis, Yin, and Niraj's fault, but unfortu-

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nately it make it difficult to determine how the network effects might be manifested.

DRIVERS OF MARKET SHARE

In essence, Tellis, Yin, and Niraj's model, though not specified specifically as such, suggests that current market share is a function of product quality and lagged market share (over the last three periods), as shown in Equation 4 of their article.

$$Sh_{i,t} = \alpha + \beta_1 N_{i,t} + \beta_2 Q_{i,t} + \sum_{m \in M} \beta_m Cov_m + \varepsilon_{i,t},$$

where Sh is the product's market share, N is the product's network effects, Cov is a couple of possible covariates, and ε is an error term. There are several other variables that affect market share beyond the simple covariates they were able to include in the model (i.e., price and growth). Thus, it is a misspecified model. It is not too problematic if the missing variables are uncorrelated with the included variables, but if they are correlated, the parameter estimates will be biased.

Another variable that would affect market share is distribution. The question is whether distribution is unrelated to product quality and network effects. Yet it is known that market share affects distribution; that is, distributors are more likely to carry products that have a higher market share (Reibstein and Farris 1995). The first brand in a market is more likely to have a higher market share and, thus, distribution than followers, even if the followers have a superior product quality—at least initially. The high distribution for the brand with the largest historical market share would retard how quickly the superior-quality product captures market share. This could be mistaken as a network effect when it is something totally different—namely, a distribution effect. Distribution is directly correlated with market share. Dislodging the owner of the leading market share is difficult because of the entrenched distribution it holds. This is some of the network effect described previously. This could be referred to as a network effect or as a distribution effect caused by the network, but it is not considered product enhancement in the traditional meaning of network effects. If it is a direct distribution effect, it is a missing variable that is highly correlated with the variables in the model. The good news is that the degree to which distribution plays a role and is influenced by previous market share—in this case, network effects—would retard the impact of product quality. If the distribution effect were included as a function of cumulative market share, the argument for the impact of network effects would be even stronger.

Gaining distribution could also be a function of product quality. Distributors would want to carry the best products. Is the quality effect in Tellis, Yin, and Niraj's study really just a distribution effect in its reduced form, thus eliminating the intervening distribution effect? This should be more fully explored in subsequent studies.

Tellis, Yin, and Niraj mention the example of Coke, which has held onto its dominant share for more than 100 years. It is implied that Coke has done so because of its superior quality. However, it would be difficult to argue that Coke truly has an objectively higher quality. It would appear that it is much more of a distribution effect.

The Coke example raises two other points: objective quality and brand. Although not explicitly stated as such, Tellis, Yin, and Nira's contention is that objective quality prevails; otherwise, the measure of quality should not be external experts but rather perceptions of potential customers. If the measure were customer perception, this could be merely communication network effects shaping others' perceptions. This leads to another missing variable in the market share model—namely, advertising. Advertising should be used to create awareness and quality perceptions. Unfortunately, advertising is not independent of product quality either. Firms are much more likely to advertise when they have a product with superior features. Again, it is not possible to determine how much of the observed effect is the result of product quality and how much should be attributable to advertising.

Because Tellis, Yin, and Niraj refer to the actual product quality and not perceptions, they are also excluding from the model any brand effects. Brand would be another variable in the market share model, just as much as product quality, because brand influences perceptions of product quality. Part of what has contributed to the brand's impact is the advertising behind the brand. With stronger brands, a premium price could be charged, thus resulting in higher margins for advertising and distribution spending.

The point of this section is that there may be many other factors that contribute to the overall market share that are correlated with the quality or the network effect measures, thus biasing the weight placed on the quality dimension. In some cases, it might be biasing quality upward, and in other cases, it might be biasing quality downward. It would be worthwhile to examine further what the true impact of product quality is.

INDEPENDENCE OF PRODUCT QUALITY AND NETWORK EFFECTS

Tellis, Yin, and Niraj treat product quality and network effects as two independent variables. According to their model, the product quality in period t leads to market share in period t, which is part of network effects in period t + 1. In accordance with a Koyck model, the equation could be rewritten as follows:

$$Sh_{i,t} = \alpha + \beta_2 Q_{i,t} + \sum_{j = 1,n} \beta_m Q_{i,t-j} + \varepsilon_{i,t},$$

which is a mere rewriting of the original equation such that the network effects are expressed in terms of lagged cumulative market share operationalization. It is not surprising that product quality plays a dominant role. Furthermore, because product quality has more of an impact, it leads to stronger subsequent network effects. This is mathematically true and logical as well.

Thus, the major inference is that contemporaneous product quality has a greater impact on market share than lagged quality, or $\beta_2 > \beta_m$. This should not be a surprising result.

STRATEGIC IMPLICATIONS

The strong conclusion in Tellis, Yin, and Niraj's article is that "firms may need to put a premium on quality rather than on speed to market" (p. 147). This conclusion has also been drawn in other studies and is demonstrated in the work of Tellis and Golder (1996, 2001). This implies that there are minimal first-mover advantages. Before such a conclusion could be drawn from these studies, it would be necessary to know whether there are other first-mover advantages beyond network effects. Are there premiums to be had by being a monopolist in the early stages and dealing with the early adopters, who tend to be less price sensitive? Is there a distribution effect beyond the network effect that works to the advantage of the first mover and makes it difficult to dislodge the incumbent? Does being an innovator lead to the perception in the marketplace of being an innovator and make it easier to continue to bring new products to market cheaper?

Generalizability of the Results

Tellis, Yin, and Niraj's results are noteworthy. The question is, What are the conditions that lead to these findings? The authors purposely chose high-tech, frequently changing products for which, "because of frequent repurchases and upgrades, the brands actually in use are those bought relatively recently (e.g., in the last three years)" (p. 139). Would the same results hold when customers are less prone to constant upgrading? By definition, "upgrading" implies that these categories were chosen explicitly to seek conditions in which quality should matter. That is not to question the findings but rather to ask how pervasive the conclusions would be in other categories in which purchases are frequent or switching occurs more for variety seeking than for purposes of upgrading.

Would the results be the same if the network was meaningful as part of the product or, as referred to previously, played a product enhancement role, as the term "network effects" was originally intended to mean? Would the same results have occurred if product quality were more perceptual rather than rated by experts in the public domain?

In the categories studied, many products were purchased electronically or directly from the manufacturer. Thus, there would be less of a distribution effect. Would the same results prevail if distribution played a greater role, such as in the Coke example, or in consumer packaged goods? Even for business-to-business products that are sold through brokers, in which brokers are major influencers, the distribution effect might retard the penetration of the superior product. As an example of needing to go through an intermediary, consider Viagra. Most medical experts would rate Cialis and Levitra as superior products, but Viagra continues to have the dominant market share, as a result of "owning" the intermediary in the purchase process.

Further Research

It would be worthwhile to continue this type of work and to develop a taxonomy of when quality is more likely to prevail and network effects to take a back seat and the conditions that lead to this. As noted previously, it would also be useful to decompose network effects so it could be more clearly understood which specific network effects are significant and which are less vulnerable to product quality or, in other words, which effects erect a more sustainable barrier to entry. With better data, it may be possible to tease out the product-quality effects from the other possible determinants of market share. Nonetheless, Tellis, Yin, and Niraj's research provides a fruitful path that should lead to many other studies in this area. However, it is premature to abandon the notion that there may be first-mover advantages under certain circumstances and beyond network effects. So, don't slow down the product development just yet.

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Both Network Effects and Quality Are Important

*PETER E. ROSSI**

Tellis, Yin, and Niraj (2009) argue that on the basis of the time-series relationship between quality measures and market share, quality differences between products dominate network effects in terms of predicting future market shares. The straw man in this discussion is a view that the economics literature emphasizes network effects at the expense of quality differences or that the economics literature suggests that network effects are larger than quality effects. For example, some people believe that Microsoft has taken advantage of direct and indirect network effects to dominate the market with products that are inferior to those offered by Apple.

The tradition in the economics literature (at least in the theoretical literature) is to isolate the effects of a particular phenomenon by abstracting from other issues. Much of the network effects literature (e.g., the survey by Farrell and Klemperer 2007) uses only one product or competing firms with products of the same quality. In these models, the only difference between products across time or markets is the size of the installed base that affects the utility flow derived from the adoption of a product. This literature completely abstracts from quality differences as determinants of eventual market dominance. A key question in this literature is the efficiency of the market outcomes. Efficiency is defined by comparison of the market outcomes with those attainable by a social planner who explicitly maximizes total

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welfare. With a single product, efficiency boils down to the extent of adoption. That is, the social planner would have all consumers whose marginal value of the product exceeds marginal cost adopt the product.

There is also a policy-oriented literature, which considers quality differences and market efficiency, but mostly through a more informal case-by-case analysis. For example, there are discussions of whether the highest-quality keyboard organization system was adopted. Many believe that the current QWERTY system was adopted merely as a result of a few random events and the superiority of the system, which few dispute could be improved on *ex post*. Others are skeptical of the value of incompatible products and view this as anticompetitive. This argument has been trotted out in many forms as a criticism of Microsoft; that is, Microsoft went out of its way to create incompatibilities that are socially inefficient. The argument against this point of view is that Microsoft could create higher-quality products because incompatible standards enabled the firm to capture rents.

In short, I believe that Tellis, Yin, and Niraj have created a straw man that is true to the informal policy beliefs of some prominent contributors to the network literature but is not supported by the rigorous work in economic theory. However, this does not make the question of the importance of quality versus network effects any less important or interesting. Herein lies the real contribution of the article. Tellis, Yin, and Niraj have gone to considerable lengths to obtain time series of quality measures of software products to supplement more readily available market share data. They follow the finest tradition of marketing; specifically, they obtain direct measures of important constructs rather than rely, as so many do in economics, on various exogenous or "instrumental" variables that only indirectly reveal important effects.

Inspection of the quality data is surprising. Quality measurements vary a lot over time, and products frequently change in their quality rankings. Already, we are beyond the standard models in the theoretical and empirical network literature. In this literature, products have a fixed quality that does not vary over time. The utility from adoption changes only because of the size of the installed base. At first, there may be skepticism regarding these measures. How can the quality of various software products vary so much over time? Is this just measurement error, or are the features of these products changing rapidly? In the case of software, it is reasonable to assume that quality is, indeed, changing over time. New versions of software products are constantly being released. What would be a problem is if the ratings of software are based, in part, on the utility of the software derived from current or anticipated network effects.

If network effects are present, what should be expected in share data? Because compatibility between most of the products studied is limited, convergence to a single dominant product should be expected. The data do not show this happening. Thus, even without consideration of quality differences, there should be skepticism that network effects are the only factors that influence market share outcomes.

A tough competitor for a model with only network effects is a model in which consumers learn about products through their experience with the products and communicate this learning with other potential adopters. This could give rise to the same sort of patterns of shares. At first, shares of products are more equal, but eventually, one product dominates. However, Tellis, Yin, and Niraj's quality measures document that product quality is already changing over time, so there should not be a dominant product. Specifying a structural model with time-varying quality, learning, and network effects would be a monumental task and require a great deal of data to estimate with any degree of reliability.

Instead, Tellis, Yin, and Niraj perform various descriptive analyses in which they investigate the relationship between current market shares and lagged measures of quality and market size/installed base. Table 7, Panel B, presents the regression results that are most immune to various standard econometric criticisms (note that Tellis, Yin, and Niraj should have used heteroskedastic and autocorrelation consistent standard errors, not ordinary lease squares standard errors). The right-most half of Table 7, Panel B, is the bottom line. Here, changes in market share are regressed on lagged changes in market share (what the authors use to measure network effects) and lagged changes in quality. Both quality and market share are important, suggesting the presence of both network effects and quality as drivers of demand.

Both variables (N and Q) are significant at any conventional level of significance. Tellis, Yin, and Niraj note that one is "more significant" than the other. The *p*-value for the network variable is 3.58×10^{-6}, while the *p*-value for the quality variable is 2.00×10^{-15}. It is a stretch to argue that this implies, even from a statistical point of view, that network effects are less important than quality.

What is most relevant for marketing policy is the size of the effects. If both variables (quality change and network change) were on the same scale, it would be necessary to conclude that network effects were more important than quality effects because the network variable coefficient is almost two times the magnitude of the coefficient on changes in quality. It is difficult to say, but it appears that this regression could easily be interpreted as implying the opposite of Tellis, Yin, and Niraj's conclusion.

They then go on to perform a Granger test of "causality." These tests do not test causality per se but simply document various aspects of the time-series relationship between these variables. I do not believe that causality can be tested in the complete absence of a theoretical framework. To understand that this is true, consider a simple example: Suppose Y represents a daily temperature variable. Let X_t represent a forecast at time t of the temperature at time t + 1. If the weather forecaster has access to some other source of information (e.g., variable Z) that is useful to forecast the future, there are circumstances in which it would be concluded that X Granger-causes Y (in the sense that in the regression of Y_t on Y_{t-1}, X_{t-1} has a significant coefficient on X_{t-1}, but in the regression of X_t on X_{t-1}, Y_{t-1} does not have a significant coefficient on Y_{t-1}). No one would seriously argue that a weather forecast "causes" future weather patterns.

In summary, Tellis, Yin, and Niraj should be congratulated on a stimulating article that contributes the first hard evidence (that I am aware of) regarding the relative importance of network effects and quality. I do not believe that

the analysis supports the conclusion that network effects are less important than quality differences, but this work certainly underscores the importance of quality differences.

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Moving Targets: Price, Quality, and Platform Competition

*JENNIFER BROWN and JOHN MORGAN**

Suppose that two people are choosing between platforms of differing quality. Platform A is low quality, and Platform B is high quality. Players make their choices at the same time and face the following payoffs:

	A	B
A	2,2	0,1
B	1,0	4,4

What will the players choose? To a lay person, the answer is obvious—both players should choose the high-quality platform and, consequently, enjoy higher payoffs. To a game theorist, however, the answer is far from clear. If one player expects that the other will choose A, choosing A and enjoying a payoff of two is better than choosing the high-quality platform and enjoying a payoff of only one. Both (A, A) and (B, B) are equilibria to the game: The network effects associated with coordinating on Platform A dominate the benefits of being the sole user of the high-quality platform. There is no a priori reason to believe that users will not get stuck using the low-quality platform. In other words, quality ≠ size.

Although the (A, A) equilibrium follows from the logic of the game, what about its relevance? In their intriguing article, Tellis, Yin, and Niraj (2009) examine how often users get stuck in this equilibrium. They conclude that as a practical matter, the market manages to solve this problem and coordinate on the high-quality platform. Indeed, the main message of the analysis is that in real-world markets, quality → size.

We offer two caveats to this conclusion. First, in most markets, few consumers can afford to consider product quality in isolation. Instead, consumers are likely to consider the surplus they receive from each platform. This depends on network effects and inherent platform quality. However, it also depends on prices. These do not simply fall from the sky; they are strategic decisions on the part of platform operators. In other words, price is a moving target. Firms can continuously change their prices and, consequently, adjust the relative surplus consumers earn from each platform.

Second, although fixed in the short run, platform quality is also a moving target in the long run. To understand why this matters, consider the following variant of the simple game described previously. Suppose now that consumers make platform choices in each of two periods, which may be viewed as the short run and the long run. After the first period, the winning platform, as a result of its financial success, invests in quality, which increases consumer surplus by three units. Thus, if Platform A wins in the short run, payoffs in the second period are as follows:

	A	B
A	5,5	3,1
B	1,3	4,4

If the players coordinate on Platform A in the second period, a long-term analysis will suggest that the high-quality platform prevailed. This was not the case from the start. If quality is a moving target, it is important to distinguish between the short run and the long run.

SURPLUS

Although the first caveat suggests that it is important to consider surplus (quality and price) rather than quality alone, it is extremely difficult to measure surplus in field data. However, controlled laboratory experiments of platform competition can distinguish between surplus and pure quality effects. Hossain, Minor, and Morgan (2008) report results of experiments in which respondents participated in more complicated versions of the games described previously. They varied both a platform's quality and its price. Respondents played four sets, each consisting of 15 periods of the game. At the start of a set, respondents were assigned randomly to a market that consisted of four players. They were told about prices and how their payoffs would vary with the number of other players who chose the same platform. After each period, respondents learned about market outcomes.

Figure 1 illustrates the pattern of platform choices in one of Hossain, Minor, and Morgan's (2008) treatments. In light of Tellis, Yin, and Niraj's findings, their result is reassuring. Although coordinating on the low-quality platform is an equilibrium, the market quickly solves the coordination problem and converges to the high-quality platform. However, changing the payoff parameters produces a troubling result, as Figure 2 illustrates. Here, respondents get stuck with the low-quality platform—the opposite of Tellis, Yin, and Niraj's findings. What could be happening?

To reconcile the apparent contradiction, it is essential to account for the prices charged by each platform. In Figure 1, the high-quality platform charges a low enough price that consumers prefer to coordinate on it rather than the low-quality platform. In contrast, in Figure 2, the high-quality platform is too expensive to be attractive to consumers.

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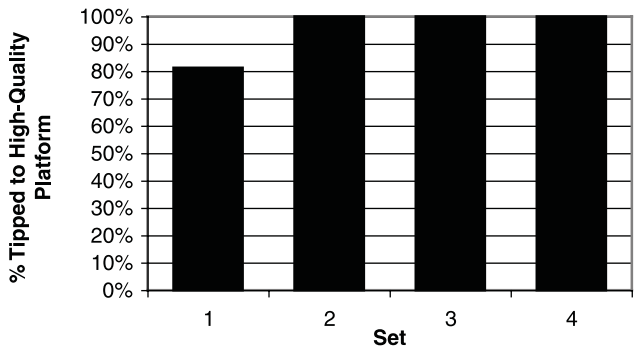
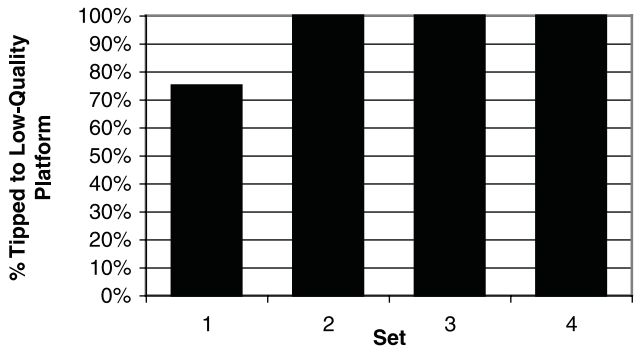
Figure 1
PERCENTAGE OF MARKETS TIPPED TO HIGH-QUALITY PLATFORM



Source: Figure 4 in Hossain, Minor, and Morgan (2008).

Figure 2
PERCENTAGE OF MARKETS TIPPED TO LOW-QUALITY PLATFORM



Source: Figure 6 in Hossain, Minor, and Morgan (2008).

That is, consumers enjoy higher surplus from coordinating on the low-quality platform than from coordinating on the high-quality platform. When the platform prices are accounted for, both figures are consistent with consumers coordinating on the high-surplus platform rather than getting stuck on the low-surplus platform. That is, surplus → size.

While prices were parameters in the experimental setting, real-world prices are chosen strategically. There is reason to believe that the prices charged in Figure 1 are closer to practice than those in Figure 2. After all, the high-quality platform can afford to cut its prices and offer higher surplus than the low-quality platform.

QUALITY

Success in one dimension can often lead to success in another. For example, Liebowitz and Margolis (1995) show that success on the quality dimension leads to market share success.

Yet, in principle, the reverse could be true. For example, Psion leveraged the success of its personal digital assistant platform (a low-quality alternative to Palm) to transform its EPOC operating system into Symbian—one of the dominant (and high-quality) operating systems used in mobile handsets. Microsoft famously uses cash generated from its operations to turn low-quality platforms, such as its first-generation Web browser, into high-quality platforms. These examples support our second caveat: When quality is a moving target, it is necessary to consider the possibility that, in the long run, size → quality.

The Granger-causality tests in Tellis, Yin, and Niraj address this issue in the short run. They shows that market dominance in the previous period does not lead to more favorable product-quality reviews. Nonetheless, given the significant time and investment needed for substantial quality improvements, it is difficult to rule out a long-term effect.

CONCLUSIONS

Tellis, Yin, and Niraj's findings reveal a key limitation of existing theoretical models of platform competition. While the models are mostly static, platform competition is dynamic in practice; that is, prices, quality, networks, and total market size may change over time. Moreover, while theory suggests that mutually held beliefs among sophisticated players can lead low-quality platforms to dominate, both in practice and in the lab, this lock-in belief rarely seems to arise.

If theoretical predictions of network growth and market efficiency fail to describe reality, perhaps it is time to enrich the theory with dynamic modeling and to relax the assumption that players are hyperrational. A potentially fruitful direction, as suggested by Friedman (1998), is to use evolutionary models to understand how these markets evolve. For example, in studying platform competition in U.S. online auctions, Brown and Morgan (2008) show how an evolutionary model can successfully rationalize several apparent anomalies between their field experiments and standard theory. Their model also describes the dynamics leading to the eventual tipping that took place in this market—the closure of Yahoo's auction site.

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Why and How Quality Wins over Network Effects and What It Means

*GERARD J. TELLIS, EDEN YIN, and RAKESH NIRAJ**

Contemporary high-tech markets are marked by two key characteristics: the presence of network effects and a tendency for one brand to dominate with a high market share. Casual observers have inferred causality from these two factors, suggesting that network effects lead to high market share. Furthermore, popular anecdotes of supposedly inferior VHS dominating Beta or supposedly inferior QWERTY dominating Dvorak have led others to conclude that network effects lead to perverse markets in which inferior brands dominate superior ones. Some economists have gone on to develop formal models to show how such perverse equilibria happen. In our lead article (Tellis, Yin, and Niraj 2009), we provide what we believe is compelling evidence to refute this conclusion. We find that, in general, quality wins, despite network effects. Moreover, networks can enhance the beneficial effect of quality. Commentaries by Ratchford (2009), Shugan (2009), Reibstein (2009), Rossi (2009), and Brown and Morgan (2009) raise a variety of questions and implications about this finding. Their insightful comments can be grouped into issues of why and how quality wins and what that victory means.

WHY QUALITY WINS

The classic argument in economics for why a low-quality brand might win is based on network effects. Brown and Morgan lucidly present this argument in a game-theoretic example. The essence of their argument is that any one consumer could get more benefit from using a low-quality brand when others also do so than from using a high-quality brand given the risk that others might not do so. Moreover, they argue that such a perverse equilibrium could persist for one of two reasons: (1) The low-quality firm could drop price relative to the high-quality product to provide consumers with greater surplus, or (2) the low-quality product might generate so much profit initially that it is able to increase quality. Brown and Morgan agree that our results do not support the prevalence of such perverse equilibria. They speculate that the reason might be that the high-quality product could match any price drop by the low-quality product.

Ratchford provides two reasons quality might only seem to win. First, low-quality brands may not enter the market. We believe that this is not widespread because (1) we present evidence of some brands entering with quality below that of their rivals and several brands increasing quality since their entry and (2) Tellis and Wernerfelt (1987) show that contemporary markets have a wide variation in the quality of products, even in markets that seem to be mature. Second, Ratchford notes that new brands with superior quality may win in the market either by bundling their products with established ones (e.g., Internet Explorer bundled with new computers) or by making them compatible with established products (e.g., Word having WordPerfect-compatible functions). We concur but believe that it is not the primary reason.

We suggest that the primary reason quality wins is the following: Contemporary markets, especially for high-tech products, have a small segment of consumers who are keenly informed about new products and their quality. These consumers are independent minded, eager to test out new products even if others do not, and actively share their findings with others online, in print, or in real life. We call these consumers "market mavens" (Feick and Price 1987). Such consumers seed the network for new, high-quality products. Support for this hypothesis arises from our finding that in so many markets, the winning product is new and has no market share to begin with but has or builds superior quality relative to its larger rivals that have early large networks.

Competing arguments about why quality wins may rely on segmentwise differences and dynamic behavior of firms and consumers. We concur with Brown and Morgan that future economic models need to account for such differences by segment and the dynamics of changing quality, network effects, and prices over time. We also concur with their and Ratchford's call for empirical research that tests our hypothesis directly with survey or market data.

EVIDENCE THAT QUALITY WINS

We ascertain the role of quality by assembling time-series data on quality and market share from a large number of markets and by analyzing these data in five different ways. Both Reibstein and Rossi applaud these efforts. However, they raise several valid issues about our data and analyses.

Rossi points out that in contrast to standard theoretical models that completely abstract from quality or consider quality fixed, in our data, quality varies a lot over time. He rightly suggests that future models need to account for this phenomenon, though specifying a structural model with time-varying quality would be a "monumental task."

Rossi suggests that the importance of a variable in an estimated model should be inferred from the size of its coefficients rather than from its level of significance. We agree but contend that the importance of variables may be estimated only from standardized coefficients, which we had not previously supplied. We reestimated the models to obtain standardized coefficients. In the all-category sample, for the log-log model (Equation 5), the standardized coefficient of quality is .394, much higher than that of the network at .028. For the first-differences model (Equation 6), the standardized coefficient of quality is .345, much larger than that of the network at .197. These results are consistent with our assertion that in both models, quality has a larger effect than the network of consumers on market share flows.

Rossi's greatest concern is that Granger causality may not imply genuine causality in the absence of theory. Although we agree with his technical point, our conclusion

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about quality is interpreted within a theory about the role of quality, albeit one that is not formally developed in this article. The essence of the theory is a view of the market as segmented. At least one segment of consumers is informed about quality and values it as much as or more than the network, while other segments are uninformed about quality or weight the network of users more importantly than they weight quality. Moreover, as we state in our lead article, the major reason for the Granger test is to refute the notion (also raised by Rossi) that market share might drive the rating of quality when raters are influenced by the rated brands' market share. The graphical and switching analyses also provide support for the notion that quality drives market share.

Reibstein points out that our estimates of the effects of quality and network may be biased because of excluded variables. Tellis (1988b) points out that the direction, though not size, of such biases can be identified. When the excluded variable is positively related to the included one and the dependent variable, the bias is positive; when the excluded variable is positively related to the included variable and negatively related to the dependent variable, the bias is negative. In line with this principle, the exclusion of brand, distribution, and advertising positively biases the effect of quality and network. The exclusion of price negatively biases the effect of quality and network. Because of data limitations, the precise, unbiased effect of quality and network effects must await further research. Because these biases apply to both quality and network effects, the data limitation does not undermine our main contribution that the effect of quality is strong relative to that of network effects. Moreover, advertising, price, and brand names may not be a major factor in these markets. Advertising's effects have been shown to be weak (Tellis 1988a, 2004; Tellis and Ambler 2007). For markets in which price is included, its effect is insignificant. As we also show, brand names do not seem to carry much weight in this market (there are some notable examples of the quick demise of brands with leading market share). Nevertheless, further research needs to confirm our finding after controlling for price, advertising, and brand names.

Estimates from the model aside, we believe that the graphical analysis of market share flows provides compelling evidence that improvement in quality drives improvement in market share. Similarly, our analysis of switches strongly suggests that switches in quality drive switches in market share. A strong case would have to be made that missing information on changes in advertising, distribution, or price—rather than the strong patterns for quality that we present—are responsible for these flows and switches in market share. Moreover, the famous cases of Beta versus VHS and QWERTY versus Dvorak may not truly contradict our findings that quality usually wins. Indeed, after an in-depth reexamination of the historical evidence, Margolis and Liebowitz (1999) conclude that the quality of the QWERTY typewriter was no worse than that of Dvorak and that the quality of VHS was superior to that of Beta, if recording time is taken into account.

THE SCOPE OF NETWORK EFFECTS

Our conceptual definition is the same as that in the economics literature: the increase in utility of a product for any one user from more users of the product. Examples are the increase in value of a cell phone, e-mail, or a multiperson game to one person as more people own these products. The utility here increases only because of additional users. However, Reibstein points out that utility could increase from three other sources of a network broadly construed: more users (leading to more word of mouth), higher sales (leading to lower costs), and more distributors (leading to greater availability). All these variables are related to, but distinct from, the network of users. Our operational measure of network effects includes effects due to all these sources. We also concur that the issue of the precise source of network effects is pertinent. Further research needs to explore the measurement and implications of the various meanings of network effects with precise operational measures.

Reibstein argues that a richer construct of network effects brings into question the generalizability of the effects across markets. Some markets reveal the dominance of one brand (e.g., Coke) despite apparently no change in quality or even no superiority in quality over rivals (e.g., Pepsi). We concur with him that further research could explore what markets typify those in which quality plays a major role and seems to trump network effects and others in which it seems unimportant.

SHOULD FIRMS BE RELUCTANT TO ENTER EARLY?

The traditional reasons for early entry are shaping of consumer preferences, signing up distributors, building brand reputations, preempting best locations in brand space, and exploiting economies of experience. Both Reibstein and Shugan argue strongly that despite our findings, firms need to enter these markets as early as possible for some additional reasons. Shugan argues for early entry because (1) firms motivate employees to beat rivals, (2) thresholds for success increase over time, and (3) the low failure rate of late entrants may be due to self-selection. Reibstein argues that the early entrant might be profitable in the early period even if it fails later. A late entrant might not have such a margin of safe profits.

However, although we do not dispute the merits of these reasons, we warn against a rush to market. Our article reinforces findings that pioneers often fail (Golder and Tellis 1993; Tellis and Golder 1996, 2001), while quality pays off with increases in market capitalization (Tellis and Johnson 2007). Based on all these findings, our recommendation is not to slow the product development process but rather to subordinate the timing of entry to achieve superior quality. Because a superior late entrant can overwhelm an inferior early entrant even if the early entrant enjoys network effects, we suggest that managers should "get it right" rather than "rush to market." In other words, the important strategic lesson is that "it is better to be better than to be first."

CONCLUSION

Networks are an important and interesting phenomenon, and they are becoming increasingly prevalent in contemporary high-tech markets. Much research in the literature has explored how network effects can lead to perverse markets. Contrary to this position, we suggest that quality is important even in the presence of network effects, which enhance

rather than overwhelm the role of quality. The insightful commentaries of Ratchford, Shugan, Reibstein, Rossi, and Brown and Morgan seem to concur that the evidence we present in our lead article is persuasive. However, the commentaries raise several important issues that enrich the meaning of the constructs and the interpretation of the findings. They provide a wealth of directions for further research.

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