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MARKETING RESEARCH AND THE
NEW PRODUCT FAILURE RATE

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FOR DISCUSSION PURPOSES ONLY

Across the panorama of marketing history, the 1950s and 1960s will probably be labeled the Golden Era of Research. Although formal marketing research finds its roots in the early 1900s, significant advancements awaited the prosperity, the development of research tools, and the expanding supply of trained personnel of the post World War II period. Virtually every facet of the field was blessed with usage growth, technical achievement, enhanced profitability, and a newly recognized position in the arena of research professionalism.

Consequently, American marketing today stands proudly preeminent in the development and exploitation of technology for solving problems in our field. However, as one whose professional career has spanned this period of research growth and whose activities have occasionally helped advance the research frontier, I find myself growing increasingly concerned. Evidence continues to accumulate in support of the belief that the output of this intensive period of development has been considerably less than successful in meeting one of its key assignments--that relating to new products.

The facts seem to be that (1) the overall rate of new product introduction continues to be high (with some cyclical fluctuations); (2) these new products continue to fail at an unacceptable rate; and (3) reasons offered for the failures predominately indict attitudes or decisions which a good marketing research program should avoid.

The first of these points is indisputable. New products continue to have a high priority in most marketing strategies; every distributive trade seems to lament the problems of handling such a flow, and company sales analyses almost inevitably confirm what its managers take pride in--a high percentage of current sales comes from products introduced within the past five (or ten) years.

The other two points are not so easily sustained. On the question of success (or failure) rate, a review of the literature turns up surprisingly little documentation for the frequent claim that 80 percent of all new products fail. Available references, cited in the Bibliography, offer the following:

1. New food and drug items:

   - Nielsen: 53 percent failed in 1971 vs. 46 percent in 1962 [16]
   - Business Week: 50-80 percent failed [4]
   - Rosen: Over 80 percent failed [18]
   - Dodd: Over 80 percent failed [7]
   - Helene Curtis: 43 percent failed [10]
   - United Kingdom: Over 40 percent failed [8]

2. New consumer goods (primarily packaged):

   - Angelus: Over 80 percent failed [2]
   - Booz, Allen & Hamilton (1968): 37 percent failed [3]
   - Conference Board: 40 percent failed [11]
   - Ross Federal Research Corp.: 80 percent failed [17]

3. New industrial goods:

   - Conference Board: 20 percent failed [11]
   - Booz, Allen & Hamilton (1968): 30-40 percent failed [3]

4. New "products":

   - Dept. of Commerce: 90 percent failed [20]

The biggest problem in interpreting these data is, of course, the definition of failure. One of the studies used "Went into test market but never went national." Another used "Disappearance from store shelves." The best approach, yet the most difficult to use consistently, is to pass the responsibility of a definition to the marketing firm, asking them to say whether a product failed to meet expectations. This definition makes failures of low-profit products even though they are kept in the line, and it is probably most desirable for this analysis because such low-profit products most likely would not have been marketed had the outcome been predictable (even though their low profits are adequate to keep them off the deletion list). Regardless of definition, all estimates would seem to indicate substantial room for improvement. It would be tough to argue that current failure rates are satisfactory, or that managements are happy with them.

Furthermore, the specter of failure haunts every firm. If any active new product marketer has avoided failure, he hasn't made his claim publicly. Look at this list of some well-publicized disappointments:

Ford's Edsel

DuPont's Corfam and Telar

General Foods' Gourmet Foods and Kool Shake

Block Drug's Sentrol and Palmsweet

Whitehall Lab's Chocolax and Deconjets

American Home Products' Pet Up and Go

Warner Lambert's Lip Quick and Reef

Dow's Dowguard

Union Carbide's Long Life Prestone

Brown & Forman's White Whiskey

Kaiser's foil

P&G's Teel, Cinch, and Hidden Magic

Bristol-Myers' Resolve, Fact, and Vote

Gillette's Nine Flags Cologne

Scott Paper's Babyscott diapers

Sylvania's Colorslide TV viewer

Lever's Vim tablet detergent

Convair 880 and 990 jets

Best Foods' Knorr Dry Soups

Standard's Golden Esso Extra gasoline

Hunt & Wesson's Supreme spaghetti sauce

Colegate's Cue Toothpaste

Campbells' Red Kettle soups

Rheingold's Gablinger beer

Dynamo detergent

Code 10 Hair Dressing

Easy-Off Household Cleaner

Cope sedative

These well-known failures are mostly consumer products. The industrial market can list many other classic flops, too, although such items are usually recognized only in specialized markets. To repeat, if any compnay has marketed new products without failure, its secret is well kept. Even the legendary Leonard Lavin of Alberto Culver was unable to keep his string of successes in tact.

All of this evidence, of course, is inadequate to prove that current failure rates are unacceptable to business in general. Any failure is unacceptable to its management, but even a significant overall failure rate may be acceptable on a macro (or public policy) basis. This would be based on the premise that new product development entails risk, nothing ventured nothing gained, successes more than carry the losses, and so on. Many new cancer drugs will fail before cancer is cured. Who could have said which office copying technology would win out, or that RCA's color TV system would prevail? There will be unprofitable small cars marketed over the next five years, but we all applaud the search for a solution to current economic/safety/environment dilemmas.

This essay, however, is addressed to the micro problem. The marketers of every product failure listed above were hoping for success. Every management (though not every person) expected success, and with sound reasons. Yet, every firm was surprised by something--a fickle consumer, a tough competitor, an overoptimistic R&D group. So, from the perspective of the individual management, it appears reasonable to accept the second premise: New products fail at an unacceptable rate. We would conclude otherwise only if the research necessary to avoid the failure was more expensive than the expected cost of the failure.

The third premise, that marketing research is significantly at fault, also requires explanation and comment. Some of the studies of new product success rates referred to earlier, also explored the causes of those failures. They looked at the reasons why selected items were withdrawn from the market or failed to meet profit goals.

Several other investigators have sought reasons for failures even when their research didn't attempt to assess failure rate.

Table 1 tallies the reasons these investigators have cited for new product failure. (To facilitate comparison I have taken some liberty with terminology, but, hopefully, not with meaning or intent.) As is generally suspected (though perhaps equally disappointing) all studies point to lack of meaningfully superior product uniqueness as the predominant reason for failure. High on the list, also, are the factors of poor planning, poor timing, and the tendency to let enthusiasm override a more becoming caution.

Comparing these reasons for failure with the claimed capability of marketing research, we can test the last of the three original premises--that the reasons offered for failure predominately indict attitudes or decisions which a good marketing research program should avoid.

One could probably argue that timing is not controllable. Ford Motor Co. could not stop the consumer's loss of interest in middle-sized cars during Edsel's last year of development. One firm rarely knows another's new product plans in any detail. And certainly the forecasting of major economic fluctuations defies expertise well beyond the capability of most firm's marketing research departments. The other three top reasons, however, cannot be excused. Technology presumably exists to measure product differences. We claim to be able to measure and validate the effectiveness of various marketing strategies and plans. All experienced corporate marketing researchers know it is predominately their assignment to see that enthusiasm doesn't outrun the known facts. Currently available marketing research capability therefore exists to avoid three of the four major reasons for new product failures.

Table 1

REASONS FOR NEW PRODUCT FAILURES

	Abrams (1)	Angelus (2)	Booz, Allen, Hamilton (3)	Constandse (5)	Diehl (6)	Hopkins & Bailey (11)	MacDonald (13)	Miles (15)	TOTAL
1. Lacked meaningful product uniqueness*	X	X	X	X	X	X	X	X	8
2. Poor planning**	X	X		X	X	X	X		6
3. Timing wrong	X	X	X	X		X			5
4. Enthusiasm crowded on facts				X	X	X	X	X	5
5. Product failed	X	X				X			3
6. Product lacked a champion					X				1
7. Company politics					X				1
8. Unexpected high product cost						X			1

* In some cases there was not, in fact, a difference, but in most cases the marketers overestimated the value of the difference to potential buyers.

** Includes poor positioning, poor segmentation, underbudgeting, poor overall themes, over pricing, and all other facets of a plan.

We have fifty years of technological developments, a growing body of psychological and mathematical hypotheses (if not theory or, in some cases, confirmed facts or laws), a reasonably complete literature, excellent journals, an eminently successful association (the American Marketing Association), a solidly established educational system, and a collection of practitioners which would compare favorably with that of any profession.

Why, then, do we have such a high rate of new product failures? If, as some of the research studies suggest, the problem is one of people not technology, what is wrong? Why do brand managers, product managers, and marketing managers ignore key data or refuse to finance research which would far more than recoup its costs? It would be ridiculous to suggest that they do so intentionally; the search for an answer had to lead elsewhere. Such is the point of departure for this investigation, which has tapped the thoughts and (sometimes very strong) opinions of highly experienced and knowledgeable people.

A careful review of the available literature--books, articles, and published speeches, plus the inevitable array of newspaper articles, company house organs, etc.--combined with my personal experience produced a series of first approximation hypotheses, which were sent to six experienced marketing researchers (some industry and some service firms, none academic).

Their response led to extensive revisions, additions, and deletions. It also led back to the literature for evidence to support this or refute that. The next version of the report was published as a working paper and sent to over thirty knowledgeable new product marketers or observers. The result of this final round of review is the present report. Unfortunately, there seems neither solid fact nor solid opinion in support of any one or a few hypotheses. It's apparently a matter of whom one talks to. Consequently, it was decided to make this a preliminary or interim report. The answers to the basic question are offered as hypotheses in the formal sense of the word. They are not yet theory, even though there is some empirical base for every one, and they most certainly are not fact. Perhaps future experimentation and empirical testing will provide a base for firm conclusion, perhaps not.

The order in which the hypotheses are listed is intended to be random; no significance should be attached to an item's placement in the array.

Hypothesis I: Product developers fail to define their new product development decision process concisely and completely.

An effective, and efficient, role for any business research requires a reasonably clear decision process on the part of persons using the research results. That is to say, the research should produce data which relate to specific critical decisions. Every decision which moves a new product closer to market should have a time designation and should be integrated into an overall flowing process. It should also have an understood importance designation, a mechanism for resolving the data into decision, and a clear indication of the risks (probabilities and costs) involved with various dimensions of error.

To clarify by example, this means that a decision on the trade incentive portion of a new product's marketing program is time-tagged: someone computes the ideal data and perhaps the latest time for this decision. The developer should also know how this decision relates to other decisions (e.g., pricing or channel choice) which precede or follow it. Channel should precede, whereas price (and budget) would follow. He should also be able to tell his researcher how critical this decision is (its importance), what criteria he plans to use (e.g., competitive margins, legal constraints, attitudes toward dealer premiums on this type of good), and some feeling as to the relative importance of these facts.

Finally, he should know what danger dimension he faces--that is, how serious is an error, how likely is an error, and how correctable would one be. Thus he might know that the dealer incentive program is essential to stocking and that his overall push strategy requires prior distribution of high quality and quantity. But perhaps his experience warns him that companies typically err on the side of too little incentive, and he knows that once the opening trade program misfires, recovery is almost impossible over the near-term.

From all this a qualified marketing researcher can craft a research program that will be both effective and efficient.

Short of having such a decision process, the researcher must shot-gun it--gather tons of data (or as much as he has budget for), not knowing which pieces are particularly relevant and useful to the decision maker. If he comes up with the needed data he's lucky, and he'll still be faulted for all the excess information compiled.

Evidence to support this hypothesis can be found easily although it is anecdotal. Take, for instance, the marketer who refuses to position his product early in its development. He prefers to keep all options open, at least as long as he can. The consequence is that when an early package size decision must be made, the entire range of options must be studied. The cost is prohibitive, or at least discouraging.

Another form of the concern comes from a David Ogilvy speech to a 4.A's meeting on May 18, 1974. He was citing the huge increase in creative research in recent years, and, although crediting creative research with an assist, he seriously questioned the need to create half a dozen new campaigns just for testing. This drains the well of creativity and the research budget, too, all needlessly. Researchers sometimes research most that which they can research best, not necessarily that which most needs researching.

Another flagrant example of suboptional research concerns remedial action plans, scheduled for implementation when and if troubles come after launch. If the product developer fails to anticipate the potential problems and establish appropriate action points for each remedial plan, he'll have researchers studying all facets of the market results rather than concentrating on the few pieces of data which are really key. Then when trouble does hit, panic actions will stem from the flood of disjointed research data.

The way to avoid this particular failing is to fully inform the marketing research team as to what decisions will be made, when and by what criteria as well as the likelihood of error and costs of error. If these decision parameters are known, a researcher can creatively and selectively manage the research technology to bring to bear on each decision the research (and only that research) which the decision warrants. Unfortunately, this first hypothesis holds that marketing planners and other product developers all too often lack this decision sophistication. They stand charicatured by the chap who, when asked why he was taking a given item into test market, replied, "Why, to see if it will sell, of course!"

Hypothesis II: Researchers have gotten carried away by the new stature (and riches) of their field.

Perhaps the most troublesome dilemma facing any research director (marketing, technical, or whatever) concerns the allocation of available funds between the three broad choices of (1) purely applied research,

(2) semi-basic research, and (3) pure basic research. Arguments for each are well known, and the typical firm ends up spending heavily on applied research, even though the research budget intended to concentrate on basic. Expenditures are divided this way, even if budgets aren't.

Some organizations, by deliberate decision, prefer to work with the bias, thus going to the basic end of the spectrum and seeking totally new technology in the case of Polaroid, for instance, or totally new research techniques in the case of most academic marketing research. I think that most observers applaud these decisions.

Such a policy is risky, however, especially for academia, and especially during good times when money is plentiful, when tangible results are not as eagerly sought, and when the research mix shifts too rapidly or too completely toward the basic. Research productivity per dollar expended may fall rapidly, and certainly will unless the long-shot discovery is found.

This essay is an improper forum for comment on the debate currently raging over the contributions of academic marketing research. (See particularly the Letters to the Editor, Marketing News, starting with the March 14, 1975 issue.) It can be granted that the practitioner engaged in researching his firm's new products both wants and gets at least some help from his academic counterparts.

This second hypothesis, however, poses the thought that practicing marketing researchers may have been too eager to fund and otherwise encourage academic research efforts. Although certainly tough to define, there must be a basic package of responsibilities that practitioners owe to their employers and customer departments. Among these responsibilities would seem to be obligation to critically appraise every new piece of research technology that comes out of academia and a willingness to make clear to their managements which of these developments are commercial (of proven value) and which are still highly experimental. Some of the best minds in industry and most of them in the universities are committed to highly experimental research, quite unproven and, to date, quite unproductive. Articles such as that by Paul Green on multidimensional scaling in the January 1975, issue of the Journal of Marketing, and books such as Ramond's The Art of Using Science in Marketing, testify to the doubts that must ultimately surface and be dealt with. Nevertheless, our journals and conferences often tell of great sums being spent on new product research techniques which haven't yielded anything, but which experimenters feel "certainly will over the next x years." If we only had a small portion of those expectations now, we might have a much lower new product failure rate.

This is not to indict experimental research per se; the hypothesis doesn't even imply this. But, while accepting the need for experimentation (and for failure) as a price of progress, it might still be possible to ask whether every company, every marketing research department, every new product budget, and every university should test every new technique. Such practice comes close to fadism, with all its economic inefficiencies.

One of the world's top marketing executives was asked a few years ago whether he supported the very expensive experimental program being conducted at that time by his marketing research director. He said he did not, and in fact he opposed it. But he then quickly pointed out that he would lose an outstanding research director if he did not, in effect, give him a percentage of his budget to use on "whatever folly he wished."

Such a liberal management policy is to be commended, but the research director must recognize his obligation in this situation. Although the firm marketed a line of successful consumer packaged goods, there had been new product failures. If research could have prevented even a single failure, the experimental program should bear the costs of that product failure and be judged accordingly.

Not that researchers are necessarily at fault. Managements sometimes create an environment or a reward system which literally forces research people to latch onto gimmicks or untested techniques in an effort to compete with other "technology" departments. If a marketing vice president marvels at a statistical technique the personnel director used on the board of directors, only a brave marketing research director will ignore the obvious. But nothing industry managements can do will rival the motivation system operating in our university marketing departments. The young professor who aspires to national recognition (to say nothing of promotion in one of the better schools) simply has no choice. He must corral some of the ever-flowing grant monies or divert on-going budget funds to work on experimental research. The more exotic and currently useless, the better--for publication, that is.

This second hypothesis suggests that marketing scientists may be succumbing to fads we really can't afford. The implication would be that we carefully reassess our mix of basic and applied research and seriously consider alternative approaches, such as strengthening the experimental research centers where concentrated effort could bring about the desired technological progress much more efficiently.

<u>Hypothesis III: New-product decision-makers really don't understand the proper role for marketing research.</u>

It is entirely possible that many managers of new product development do not understand what marketing research can do for them. If they lack the ability to use marketing research efficiently, the entire new product development function is hobbled; marketing research directors are forced to accept a role for their function substantially less than optimum and then must sell their service against an unfavorable institutionalized misconception.

Are there a priori reasons why new product managers might not understand marketing research? Indeed there are, and by far the most persuading is the non marketing background of most persons making the key intermediate new product decisions; they have never worked in a situation where an organized research function existed solely as a service to decision makers.

This fact is rarely noted, but it is real. Ultimately top marketing people are involved in a new product's development, but it is increasingly common to see organizational forms which put early and intermediate decision authority on people with backgrounds in technical research, engineering, manufacturing, corporate staff, etc. Such persons rarely are trained in the use of a decision-support function. Nor, incidently, are many of the marketing operations (decision making) people chosen to represent their function in interdivisional teams.

A tragic consequence is that both research and researchers are occasionally ignored. It is quite perplexing to college students, for example, to read in new product case studies that the results of product trials were rather poor, that advertising seemed confusing to potential customers, that test market results had to be rationalized, and yet the management concerned seemed genuinely shocked when the product ultimately failed. They find it almost impossible to believe that otherwise capable managers become so masochistic.

At this distance we cannot detail the precise difficulty. Perhaps in the words of a colleague, researchers are asked to gather facts rather than information. Perhaps they are accused of being negative if they become cautious or defend discouraging research results. Perhaps they have no effect mechanism for expressing their interpretations of the data. Perhaps they aren't even involved.

The role of a support function can be critical, and in this case probably is. Perhaps firms with recent product failures might ask their marketing research directors to submit, in writing, what they feel their role _should_ have been.

Hypothesis IV: Organizational rigidities are hindering the type of involvement essential to a successful marketing research program.

The third hypothesis (above) holds that marketing research is hindered from achieving a proper role because of the nonmarketing backgrounds of many of the key decision makers in new product development. There are other reasons for marketing research taking an excessively narrow role, and many of them cluster under the general heading of organizational rigidities. Four specific rigidities have been identified in this study; the _first_ is the enforcement of a distorted concept of loyalty.

Hardly a marketing research director around hasn't seen a young and impressionable analyst come under the (evil?) influence of a sales manager or product manager. The analyst is detected losing objectivity, and soon there is a flap over some research report which allegedly misinterprets a situation.

The outgrowth of such experiences is often the overt or covert building of organizational or procedural fences designed to protect the "integrity" of the research function. Now, research integrity is admittedly critical on some tasks, especially those involved in the control function, and its protection warrants strong action. But fences can be disastrous on new product development.

These so-called loyalty restraints are supposed to guarantee the objectivity of marketing researchers assigned to new product work. Researchers shouldn't get caught up in the enthusiasm of the development process, this line of reasoning insists, even if they as serving as the marketing department representative on committees, teams, or task forces.

We all must grant the attractiveness of research independence, research integrity, and objectivity, but we may also ask whether the price of achieving these ideals is sometimes the marketing research representatives entirely inadequate input into team deliberations? Though not researched, to my knowledge, it seems that successful product development requires intense personal involvement of the participants. Venture groups get this type of commitment. So do smaller companies and divisions. Shouldn't any product development team manager expect his marketing researcher to want team success? Isn't the absence of team loyalty actually a form of disloyalty? Is it a natural thing for a decision maker to give credence to the advice and counsel of persons who repeatedly proclaim their independence from the team?

If one accepts the premise that a major commitment to team success significantly enhances the likelihood of that success, then any organizational mandate which tends to isolate the marketing researcher from the rest of the new product team would seem to be unwise. Perhaps it happens too often.

Ironically, this concern can operate only if the marketing researcher is actually assigned to a team. He may not be, at least not in the beginning of the development process, and herein lies the second of the four possible rigidities. If a new product development system locks the marketing research function of the action, it obviously can play no substantive role.

Lest there be misunderstanding on this point let's note that there is now widespread agreement that the marketing function should be involved early in the new product process. Ideally, of course, most new products should start in the marketplace. But, involvement by those marketing strategists (product managers or marketing directors) who typically serve on overall new product committees is by no means assurance that marketing researchers are involved.

One of the consultants who reviewed this paper said:

> There is a major communications gap between marketing research management and product or marketing management. All too often the researcher is deemed an academic, unrealistic technician and not a marketing strategist. He is not involved in strategic thinking and he is not brought into the strategic picture seen by top management before the decisions are made.
>
> As a result, the researcher withdraws and views marketing management as pragmatic, opportunistic, and perhaps, not very bright. His defensive reaction forces him into becoming increasingly academic, increasingly technically oriented, and he therefore misses the boat when it comes to the real issue of getting involved with the decision process and a correct decision.
>
> The researcher's real value should be as a result of getting involved in the major decision process. Lack of involvement is partially his fault. The market

> researcher may contribute to the problem by avoiding decision responsibility. He may too often take refuge in the execution of his research assignment and avoid responsibilities for application of his results to the decision /that/ management faces.

These arguments support more marketing researcher involvement at the time early <u>decisions</u> are made, but I would also suggest the desirability of greater involvement in early product development operations. As just one example, it could be argued sensibly that all product testing, whether in shops, laboratories, hospitals, or wherever, should have the counsel of experienced marketing researchers. Another of the consultants who reviewed this article put it this way:

> An extremely large proportion of the money spent on new product marketing research is spent after the new items are well along in the development process. People, their jobs, and their reputations have been committed to them by that time. As a result, far too much research is devoted to trying to make silk purses out of sow's ears.

Organizational arrangements should encourage the most effective early involvement of the marketing research function. However, there is reason to think that many actually prevent it.

The <u>third</u> type of organizational rigidity operates entirely within the marketing department. It stems from an all-too-frequent conviction that product and brand people should not be permitted to participate in the execution of a research project once its purpose and method are decided. Such persons are sometimes not allowed to participate in group interviewing sessions, for instance. They may be discouraged from examining field interviewer reports in their original form.

Just as the first of these four rigidities showed the potential dangers in trying to keep the researcher himself from becoming contaminated, the point here is that some research departments go to great lengths to prevent the possibility of contamination of their research processes or their research reports.

Again, it can be granted that the scorekeeping activities of a research department justify such caution. But the marketing research in support of new product development serves quite a different function. Numbers are not as important as ideas. Nuances are more critical than conclusions.

Actually, this role for the marketing research function is more one of interface facilitation, helping to bring decision makers into close contact with the marketplace. Ultimately it all must come down to numbers in an economic summary, but that may be months after a researcher helps the key development people establish close and personal market contact.

The last of the four ogranizational rigidities concerns persons, not decisions, and may be unavoidable. It relates to the characteristics seemingly required of marketing researchers assigned to new product work:

- High risk acceptance,
- Ability to work with all types of company personnel and at all levels,
- Ability to act with little precedent,
- Acceptance of a high waste ratio (projects cancelled or research performed for products that are abandoned),
- Creativity in applying research techniques in new ways or to new markets,
- Ability to work on hectic scheduling and under great pressures,
- Understanding and acceptance of what one of this article's reviewers calls a basically irrational process. (He described new product development as essentially an art form, and proposed that the thought might serve as a separate hypothesis.)

Risk aversive, thorough, and orderly personnel are persona-non-grata in new product development. Yet most assignments in the marketing research department call for caution, order, patience, persistence. Thus we can speculate that many (perhaps most) researchers assigned to new product work are precisely the wrong people unless the department has been permitted to staff up especially for this purpose. It can't assign the ideal people to new product development if it doesn't have them.

Hypothesis V: The project system of marketing research department management works contrary to the needs of new product development.

The Golden Era of Marketing Research has produced (1) a cascading flow of new research techniques (linear programming, Markov processes, Bayesian analysis, multivariate analysis, MCA, AID, scaling, network analysis, and scores more), (2) a pleasing and opportunity-laden growth in research budgets (my rather large 1959 research department of twenty-three people operated on less than $500,000, including Nielsen on both food and drug), and (3) a professionalism reflecting the flow of college graduates (especially MBA's) into marketing research.

These three forces join to produce many effects, one of which is the research project system. The project is probably the most efficient mechanism for directing and controlling techno-bureaucratic operations. These larger, more professional, and technically complex marketing research departments no doubt need the project system.

But, again, there is a price tag on efficiency. Project directors have been known to seek project completion (or even project incompletion) rather than the best answer to a problem, the ideal product attribute, the preferred positioning, or the most likely customer servicing problem.

Early editions of marketing research textbooks stressed a phase of research Lyndon O. Brown called the Informal Investigation. Its purpose was to expose the researcher to the full dimensions of thought and hypothesis on the part of people connected with the problem. What precise information gathering and analysis activities he later undertook were coincidental to the essential element of his task--bringing the research function to a problem that had yet to yeild all of its realities. This process of full familiarization is still an integral part of our training of doctoral candidates.

Unfortunately the marketing research director who wants to apply these ideas to new product development pays a heavy price in terms of lost managerial flexibility. He must assign a researcher to a product development activity and let him stay with it through the full stream of gestational activity. This assignment would cover busy periods and slack periods, it would span the full range of research problems and skill requirements, and it would cover the full development cycle from idea generation to post-introductory evaluations. It would yield the ideal researcher-developer involvement and relationship.

It would also be expensive, because the marketing research director would lose the many advantages of the project system; so he resists the ideal. Researcher A runs an early attitude study. Researcher B runs a product placement test, Researcher C works with his agency counterpart on some ad testing, etc. Researchers laugh at the apocryphal story of a colleague who said, "I'm going to lunch. If my product manager calls, get his name." Developers have been heard to complain similarly about their marketing researchers.

What is lost is the intimate familiarization of researcher with background and people. The "quick and dirty" research is never feasible. Nor is the even more speculative action of simply asking an experienced marketing researcher what he thinks a customer's reaction would be to some relatively minor proposal. Unless a research need is worthy of a project, it's apt to get no research attention at all.

In summary, this fifth hypothesis suggests that our increased sophistication has come at some sacrifice. The new product development function has suffered at the expense of better control over research on already established products, where the project system is totally defensible.

<u>Hypothesis VI: The typical director of marketing research had a defaulted on his responsibilities as keeper of the research conscience.</u>

The principle here is not really very complex. New products are terribly demanding (uncertain environments, time pressures, political sensitivities), and ideally they require the professional input of the department's top researcher, plus the strength of his presence on various interfaces within the company.

But how reasonable is it to expect such a continuing personal involvement? Perhaps its not reasonable at all. In the first place the sheer number of projects coming through the department may be overpowering, in which case the top researcher gives each an organizational approval (more likely budgetary, not professional). Second, he may not be able to follow each new product development activity closely enough to know when to blow the proverbial research whistle--to demand that research be started (or stopped), or to insist that research findings be reviewed and reinterpreted. Such action is courageous and dangerous. There is some evidence (after the fact) that it isn't taken often enough.

Furthermore, this conscience of the department is not an easily delegated function. Courageous acts are high-risk acts to anyone, but especially to younger researchers who (1) may be hoping to get promoted someday to the very departments they should whistle down, and (2) realize they don't have the research experience necessary to make the risk worthwhile.

Consequently, a critical role is lost in the bureaucratic shuffle. Directors don't have the time or don't recognize the specific occasions of need, and nondirectors find the risks unacceptable. In either case, new product development activity suffers.

It is entirely plausible, of course, that this hypothesis is so irrevocably in conflict with the earlier one suggesting more delegation of authority to the research members of new product teams that we can't have both. On the other hand, the apparent conflict may simply be a managerial opportunity for which at least two options come to mind. First, only seasoned, confident research people would be assigned to new product work; these persons would have little difficulty standing firm as conditions dictated. Second, the marketing research department could spin-off the new product marketing researchers under their own leadership, possibly giving them the time and involvement essential to the task of research technique guardianship.

Hypothesis VII: The economics of product development will always lead to a substantial failure rate.

An inviolate guideline in new product marketing research is that the expenditures for more information relating to any particular decision must, on the top side, be consistent with the expected costs of that decision's execution. This is but a sophisticated generalization of obvious facts: how much you can spend depends partly on how much you lose if the decision is wrong.

In some new product development situations the potential loss from any one decision is great (a full-scale introductory television advertising program, for example) but, surprisingly, most of the time it is not. Unless a critical step is coming up (e.g., does the drug work effectively in its present form?) or a major expense is involved (e.g., the TV expenditure), there probably isn't a defensible basis for a sizeable, marketing-research expenditure.

In total there is, of course. An overall product development project may cost \$300,000, yet during the process it may never come to a point at which the next decision warrants a \$20,0000 research project. This seems to be especially true for many industrial products and for selected consumer durable goods if the introductory marketing outlays are rather small.

It is not difficult to imagine specific situations, either consistent or inconsistent with this hypothesis, but researching their overall frequency is thwarted by the need to have inside information on prior knowledge, decisions, and costs which either doesn't exist or is obviously highly proprietary. It must suffice to conclude that at least part of the failure rate of new products results from decisions which may have been made wisely in isolation, but which were wrong in total.

Beyond the matter of marketing research costs relative to decision costs, there is another line of reasoning bearing on the economics of new product marketing. This reasoning holds that rising populations and incomes have increased rapidly the pay-offs for successful new products. As the reward goes up, so can the costs, including the pro-rated costs of failures. If development and marketing costs in recent years have increased less than the pay-offs from our more affluent markets, the increased profits may more than cover the costs of an increased stream of failures.

Although stretched to make the point, one could prove that if the possible dollar pay-offs increased and new product development costs didn't, the percentage of new product failures should actually increase. It follows that less extreme givens could provide logical support for a constant failure rate, on economic grounds alone.

Hypothesis VIII: Environmental changes have negated our enhanced marketing research skills.

This hypothesis holds that if one could go back to the market conditions existing in 1950 and bring to 1975 the marketing research skills of only one firm in each industry, their success rate would jump remarkably. Since the other firms are not going to sit still, however, we can see why advances in research skills might not reduce the overall failure rate. To the extent that all new entries are more nearly on target, products will still fail even though they are better products than they would have been without the research.

More significant than competitor parity, however, is the fact that we now simply have a great deal more competition--more products from more firms. Thus there is a smaller gap of consumer dissatisfaction left to fill; each increment of improvement in less; and is greeted by proportionally reduced consumer enthusiasm.

Off hand, this sounds like marketing research is still clearly at fault; our measuring devices should be able to define the gaps of dissatisfaction and should be able to measure the perceived value of whatever closing of those gaps our new products produce. If the value is major, go on to market; otherwise forget it.

But, as gaps close and as competitive entries become more frequent, the demands put on the measuring technologies may be come quite unreasonable. With pay-offs increasing, too (increased affluence), product developers may be pushing marketing research well past its technical (if not its economic) capability.

Hypothesis IX: Predicting new product sales and profits is an inherently impossible task.

First I want to reiterate a point made earlier: there is no significance attached to this hypothesis' appearing last. It is neither the least important nor most important, neither least likely nor most likely. It is placed last on the list because it threatens to make all the others irrelevant. It says, in essence, given the best marketing research we can conceive and execute, given unchanged market conditions, there will continue to be product failures. There will be colossal failures and unnoticed failures, surprising failures and not-so-surprising failures.

Why? Because every new product can succeed only as persons or firms in the marketplace modify their behavior. And, the hypothesis holds, we will never be able to forecast the milieu within which that behavior operates to a degree to a degree which will permit more than a very low order of accuracy in decision making. Thus it follows that product developers will continue to make mistakes and will continue the stream of failures that we have seen throughout history.

One might suspect that this hypothesis violates one of the premises underlying this investigation, the one relating to progress in the development of marketing research techniques. Actually, there is no conflict. Certainly there has been progress in marketing research, but this progress may not have added significantly to our skills in the particular area of new product development. To the extent that major behavior and attitude changes are essentially unpredictable, it never will. It is also possible that research advances have been essentially peripheral--helping us make new product decisions, but not those decisions critical to ultimate market success.

This could be explained by showing for example, that, we're now much better at assessing what consumers think of our marketed products, or what they think of our new campaign, or where our sales are being made. These are excellent progress, but they may not be of particular value to the development of truly new products. Similarly, our enhanced research skills may lead to better sizes, colors, or shapes of widgets, but still leave us guessing as to whether people will buy widgets of any type if introduced to the market.

Summary

This essay has addressed the very broad question "Why was the rate of new product success not climbed as a result of the many advances in marketing research technology over the past 25 years?" Research indicates that:

1. The overall rate of new product failures remains high, perhaps as high as twenty five years ago.
2. This failure rate is undesirable.
3. Most reasons given for failures are (or should be) amenable to marketing research.

Granting these three premises (they can be argued, but only against the grain of available data) leads logically to the question discussed in this paper. The purpose of this discussion has been to present nine hypotheses proposed by various persons for answering it.

Six of the hypotheses relate to the way in which marketing research is utilized in the new product development process. The seventh cites an economic rationale, the eigth blames changing environments, and the ninth simply says all the advances in the world won't let marketing researchers predict the unpredictable.

There may be other plausible explanations, but these nine offer both a priori logic and some empirical evidence from the everyday realm of new product development. Whether they are researchable is questionable. Certainly some attempt should be made to verify or deny each of the nine, but before that we probably need a period of review and discussion to (1) add other, overlooked hypotheses, if any are known, and (2) refute any of the nine which shouldn't actually be on the list.

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