TWO

PLANNING FOR MARKET RESEARCH

The first step in planning a market research study is to identify the underlying decision problem. The importance of this initial step cannot be overemphasized. The more secure the researcher's grasp of the decision problem, the greater the probability that the results of market research will make a difference to the firm. Conversely, when the decision problem is left tacit or never developed, the research effort may be misguided or may address only part of the problem or even the wrong problem altogether. This is to reiterate that good market research is conducted to serve the needs of business decision makers. If one loses sight of this imperative, then research activities may simply be an expensive way to satisfy idle curiosity, or an exercise in politics to justify decisions already made, or an excuse for dithering and failing to act.

This prescription to articulate the decision problem at the beginning may sound straightforward, but it is surprisingly difficult to implement in practice. One difficulty is that the person responsible for designing and implementing the market research study is generally not the same individual as the decision maker who must act on the research results. This separation of responsibilities makes communication failures all too likely. If the researcher does not spend enough time in dialogue with the decision maker, the full dimensions of the decision problem may not come into view. When this happens, the decision maker is likely to be disappointed with the results of the research, finding them to be either beside the point or only half an answer.

Even when the decision maker and researcher are the same individual, it is still important to spend some time articulating the decision problem prior to designing the market research study. The reason is that most decision makers do not face isolated, clearly defined problems. Instead, they face tangled messes. Thus, a decision maker may find him- or herself musing,

> Sales fell short last year. But sales would have approached the goal, except for six territories in two adjacent regions, where results were very poor. Of course, we implemented an across-the-board price increase last year, so our profit margin goals were just about met, even though sales revenue fell short, so maybe there's no reason to be concerned. Yet two of our competitors saw above-trend sales increases last year. Still, another competitor seems to be struggling, and word on the street is they have been slashing prices to close deals. Then again, the economy was pretty uneven across our geographies last year . . .

Simultaneously, our decision maker is grappling with the dissonant views and varying agendas of colleagues. One colleague takes the sales shortfall as an opportunity to push once more for an expansion of the product line; another reiterates that the alignment of sales incentives with sales performance goals has not been reviewed in years and that one of the regions in question saw considerable turnover in the sales force. Just then, our decision maker's own manager may pop in with a reminder that a revised sales forecast is due at the end of the quarter. What a mess!

In short, whether or not the researcher and decision maker are the same individual, an effort must be made to identify the focal decision problem. Once the decision problem has been stated, you can make an intelligent judgment about whether to do market research at all, and if so, which technique to use. If the decision problem is not articulated, then the organization either does not do any market research, blundering forward as best it can, or defaults to whatever research technique is either traditional within the firm ("Let's send customers a questionnaire") or the personal favorite approach of some key manager ("Focus groups would be good here"). I cannot emphasize this point strongly enough: It is impossible to make an intelligent selection from among the many market research techniques available, absent a clear and comprehensive formulation of the decision problem the research is supposed to address.

FROM DECISION PROBLEM TO INFORMATION GAP

Table 2.1 outlines a process for identifying decision problems and translating these into a research design. Returning to our "sales are down" example, a good first step is to generate alternative statements of the decision problem. Here are some examples:

A. We need to overhaul our sales compensation system. What changes should we make?

B. Our product line has to be broadened. What expansions would be best?

C. We have to improve the price–performance ratio of our offering to make it more effective. Should we adjust price, add functionality, or do both?

D. We need to identify corrective actions in the six lagging sales territories.

Each of these problem statements can be mapped onto the decision maker's musings reproduced earlier. However, each statement is going to take you in a very different direction as far as conducting any market research is concerned. In fact, at least one of these decision problems—the sales compensation issue—can't be addressed by market research as conventionally understood. True, some sort of investigation may be conducted in this instance, as when you gather information on the compensation practices of other firms in your industry for purposes of benchmarking), but market research, at least from the perspective of this book, should not be confused with the broader category of social science research or the even broader activity of fact gathering in general. *Market research*, as I shall use the term, refers to a specific set of information-gathering activities focused on customers. Thus, problem statements B, C, or D can be addressed through some kind of market research, as defined in this book, whereas problem statement A cannot. In other words, one of the first fruits of attempting to formulate alternative problem statements may be the realization that market research is beside the point. If decision makers have other information that suggests that the sales compensation system is out of whack and that this misalignment is beginning to hurt company performance, they may well choose to nominate that problem as *the* problem and attack it first, without getting involved in market research per se. A more general account of the limiting conditions on market research will be given in the final chapter.

Table 2.1 Planning Process for Marketing Research

Stage	*Issues to Be Resolved*
1. Identify and articulate the decision problem	• Who is the decision maker? • What are alternative ways to state the problem? • Do these statements get at the problem or are they only symptoms of some deeper problem? • Is this a decision that can be addressed through market research?
2. Pinpoint the information gap: key questions that must be answered before a decision can be made	• What specific questions are most pertinent? • Is there one question or many questions? • Can this question be answered with the time and money available?
3. Evaluate which research technique(s) would be most appropriate for answering these questions	• One research technique or several? • Techniques used in combination or in sequence?
4. Design the research study	• What specific objectives should guide the research? • Who should participate? (i.e., if primary research, how many of what kind of customers?; if secondary research, what data sources should be consulted?) • Estimate needed budget, time frame, and other resource requirements.

Is it possible now to choose which of the remaining formulations represents the *best* statement of the decision problem at hand? In the abstract, as an outside researcher having only the information reproduced in these pages, there really is no way to determine which of the remaining statements represents the best formulation—only the decision maker knows. That is, the decision maker possesses a great deal of other knowledge, both explicit and tacit, that is essential for selecting which of the remaining statements should be used to guide market research. Until the decision maker weighs all the information available and comes to a conclusion such as "I'm really worried that we're not price competitive," or "My hunch is that the sales problem is local to those half a dozen territories," the design of market research cannot proceed. In the abstract, any of the remaining statements could be *the* problem statement

(and these are far from an exhaustive list). Each statement is capable of guiding subsequent market research, each captures at least some of the uncertainty facing the decision maker, and each is plausible as a response to the triggering complaint—sales are down.

Role of the Decision Maker

The discussion thus far suggests several practical insights into the conduct of market research. First, if the researcher and decision maker are not the same person, then it is imperative that the researcher have some kind of meaningful dialogue with the decision maker. The decision maker has to decide what the problem is, and in the real world, beset by complicated messes and the competing agendas of colleagues, this is no easy task. Hence, to be effective, market researchers cannot simply be order takers ("Three focus groups, coming right up"). Order takers fail their clients, because to be effective, good researchers have to help clients think through the problems at hand. Order takers also fail in their own business because the model is flawed—successful market researchers have to be consultants, not order takers. If the decision maker and researcher are one and the same person, then the decision maker must conduct this Socratic dialogue with him- or herself, first generating alternative problem statements and then selecting the best candidate among them.

Second, it should be apparent that each of the remaining problem statements leads to very different sorts of market research efforts. Thus, a focus on broadening the product line may not delve deeply into pricing issues or involve a comparison and contrast of specific sales territories. What may be less apparent is that every alternative problem statement foregrounds or privileges some possible answer to the triggering complaint and minimizes or excludes other potential answers or resolutions. If you choose to focus your research on the six lagging territories, you are implicitly rejecting the idea that there is anything wrong with your product line per se. In selecting a problem formulation, you may be mistaken (after all, you haven't conducted any research as yet!), and this mistake may not be recoverable in the time available. There is no way to escape this dilemma. It serves as a reminder that problem formulation has to be done carefully. If you get the problem right, then some kind of market research will probably be helpful. If you get the problem wrong, then it may not matter how good the research is.

To continue through the stages outlined in Table 2.1, let's suppose that the decision maker has a strong hunch that there really is a localized problem

in the six territories—or at least, wants to rule this out before proceeding to any other investigations. Once you have settled on a decision problem, as captured in statement D, the next step is to brainstorm the kinds of questions that have to be answered before corrective actions can be undertaken. For example, are the six lagging territories distinctive in some other way, relative to the remaining territories, beyond the difference in sales growth? Do the six territories share any common factors that are uncommon among the remaining territories? If you could find other shared differences or commonalities, you can examine these as potential causes for the sales shortfall in these territories.

Each such question identifies an information gap that market research might be able to close. Given questions of this sort, you can ask whether they are answerable at a reasonable cost and begin to identify an appropriate research technique. Note again that once you accept a problem formulation that focuses on the six problematic sales territories, you cease to ask questions about differences that are general across the firm's markets, such as your price–performance ratio or problems with product line breadth.

As phrased, the question about factors shared by the six territories, that in turn distinguishes them from other sales territories, seems eminently answerable. Generally, the firm will maintain one or more databases containing descriptive data on each territory. External databases should also be available, allowing us to ask about the overall economic health or growth rate of each territory, population factors associated with each territory, and so on.

In this case, then, the initial selection of research technique will be to tap into existing archives of data—that is, conduct secondary research. You design a secondary research effort by specifying the kinds of archived data you wish to examine and the specific variables you will analyze, in this case, for the purpose of comparing the six territories with the remainder. Thus, you might look to internal databases for data on sales calls undertaken, the ratio of wins to losses, sales force turnover in each territory, and so forth. You could consult external databases for information on competitor presence and activity in each territory, economic conditions affecting each territory, and so forth.

From Research Design to Implementation

At this point, the research design is essentially complete. You have formulated the decision problem, generated specific research questions to be addressed, and selected an appropriate research technique capable of addressing these

questions. What remains is to conduct the research, analyze and interpret the results, and formulate corrective actions (which, in some cases, may themselves need to be vetted by additional research). To complete the loop, one of two outcomes is likely in the case of the running example. On the one hand, analysis of secondary data may produce a "smoking gun." For instance, you may discover that the struggling competitor, who slashed prices last year, has a strong presence in each of the six lagging territories but has much less of a presence in most of the remaining territories. You now have a potential explanation for the overall sales shortfall, in terms of localized competitive price-cutting, and can begin to generate potential responses. These might include authorizing a higher level of discount when going head to head with this competitor or more heavily promoting those aspects of your product's functionality where this competitor's product is weakest, and so on. Specific actions will now be founded on data.

Alternatively, your search for shared commonalities and differences across problematic and unproblematic territories may come up empty. After all, in any given year, there will always be six territories at the bottom of the list, and your average sales performance will always look better if you exclude the worst six territories on the list. In other words, the decision maker's hunch may be wrong. Sales growth may have been lower across the board. Perhaps the strong territories were not as strong as they should have been, even as the weakest territories were particularly weak. This outcome will probably lead you to reformulate the decision problem in more general terms so as to identify corporate-wide factors that could explain the sales shortfall. New research will have to be designed, probably taking the form of some kind of exploratory research involving customers, to get at issues such as breadth of product line, price–performance ratio, brand image, and so forth.

Note that this second outcome, in which factors distinguishing the six territories failed to emerge, in no way constitutes a failure of research planning. Given the decision maker's mindset, industry knowledge, prior expectations, and so forth, it was imperative first to investigate the idea that the sales shortfall was fundamentally a local problem specific to certain territories. This is particularly the case inasmuch as a relatively quick and inexpensive research process was available to investigate this decision problem (secondary research is typically among the quickest and cheapest of research techniques). Only once the secondary research comes up empty can the decision maker proceed with confidence to address other formulations of the problem, which are likely to entail more difficult, prolonged, and expensive market research.

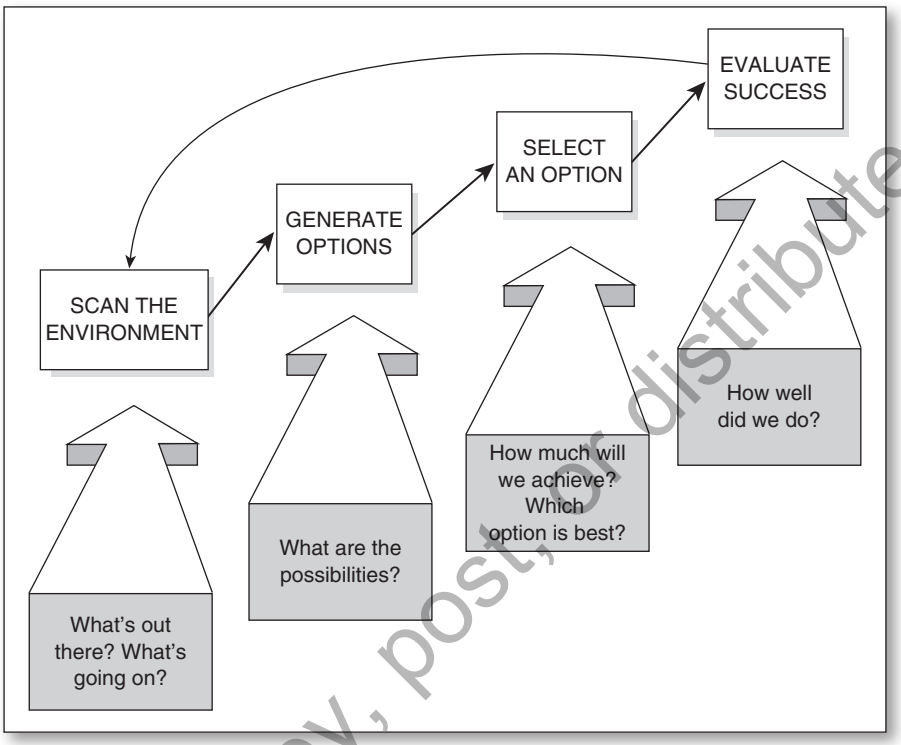
TYPES OF DECISION PROBLEMS: THE DECISION CYCLE

Formulating the decision problem is a task that has to be done anew each time that market research is contemplated. The range of researchable decision problems is as wide and varied as business itself. Nonetheless, it seems to me that the vast variety of potential decision problems can be clustered into a smaller number of fundamental types. The utility of such a typology of decision problems is that it will allow us to make generalizations about the applicability of specific market research tools. The typology may also be useful in guiding our initial efforts at formulating the decision problem in a specific case, insofar as it provides examples of typical decision problems.

Figure 2.1 presents a simple typology of decision problems organized as a cycle that unfolds over time. After this model has been discussed, we will examine the alignment between specific research techniques and specific stages in the decision cycle. The goal in that discussion is to show that once you have located your particular decision within the decision cycle, you will have simultaneously narrowed the range of appropriate research techniques to a small number.

The notion behind the decision cycle is that any major decision—developing a new product or entering a new market, for instance—proceeds through a series of smaller subdecisions. Alternatively, smaller and more localized decisions, such as the problem we worked through in the previous section ("Why are sales down?"), can be situated in the model and seen in context as representing one kind of a decision rather than another. As a general rule, major decisions such as the development of a new product may require research activities at each stage of the decision cycle (see chapter 15 for examples). In the case of more minor or localized problems, there may be a single set of research activities corresponding to a single stage of the decision cycle. The remainder of the decision cycle is then worked through informally without the aid of formal research. Thus, in the running example, if secondary research had shown there to be a specific problem with the six lagging territories, options for addressing the problem might have been generated by management discussion, the best option selected through further discussion, and the results monitored simply by reference to monthly sales figures routinely distributed. Nonetheless, the fundamental assumption underlying Figure 2.1 is that any researchable decision can be logically parsed into four steps, however truncated a particular step might be in practice. Every decision begins with a look at the surrounding context, proceeds to the generation of decision alternatives, and continues to the selection of one alternative, which then requires an

Figure 2.1 The Business Decision Cycle



assessment of outcomes, which segues into a scanning of the environment in preparation for a subsequent decision. Finally, I will argue that the distinction of four stages within any decision is consequential for the kinds of research that need to be done at each stage. That is to say, the eligible research techniques are stage dependent.

The first stage in the cycle is to *scan the environment*. What's going on? What's out there? This activity of environmental scanning can be thought of as a sharpening and focusing of the activity of intelligence gathering, which, for any alert manager, should be ongoing. An example of scanning the environment would be to compile analysts' reports on the strategies, strengths, and weaknesses of your major competitors. In this early stage, you might also examine reports on how the market is segmented, who the biggest users of this product category are, what applications dominate, and so forth.

The second stage in the decision cycle is to *generate options*. What are the possibilities? What specific directions might be worth pursuing? What choices

do you face? For example, if a product line has come to seem aged and tired, there is probably more than one possible approach to rejuvenating it, and all of these need to be identified and explored. If you are seeking to expand your market, you will want to identify all the possible groups that could be targeted for expansion. Likewise, before selecting a new theme for your ad campaign, you would want to examine a variety of candidate themes. Stage 2 can be thought of as the creative part of the decision cycle. The goal is to broaden your horizons so that you don't neglect opportunities or miss possibilities.

The third stage in the cycle is to critically examine and then *select an option* from among those generated in Stage 2. Which of these options is best? How much will this option achieve for us? It is at this stage that you must decide exactly what functionality a product will offer. This is where you determine which one among several markets is likely to be the largest, the most lucrative, or the best protected against competitive counterattack. Stage 3 is crucial because resources are always limited. This is a uniquely stressful stage because you have to commit to one option and abandon the remainder. You may have generated half a dozen attractive alternatives for market expansion, but the lack of money, people, or time will inevitably force you to select one or a few on which to concentrate your efforts.

The fourth and final stage is to *evaluate the success* of the decisions you made. How well did you do? Did you take market share away from the competitor you targeted? Did the new ad campaign change attitudes among the intended audience? How satisfied are customers who bought the new product? Results from the fourth stage are added to the stock of market intelligence possessed by the firm. These results also influence management's ongoing strategic review of business directions and set the stage for the next decision. For in business, decisions never stop.

MATCHING TOOLS TO DECISIONS

Research Objectives

A central purpose of this model of the decision cycle is to help you decide which market research tools might be useful at any given point. To do this requires a third concept that can bridge the gap between decision stages on the one hand and the market research toolbox on the other. Here the concept of a *research objective* is helpful. A research objective states, in a single sentence,

what result you hope to achieve through the use of some particular research technique. An example might be, "Identify areas of satisfaction and dissatisfaction with our current product offering." Good research objectives always start with an action verb. If you leave out the verb, you end up with something vague and empty—a wish, hope, or yearning.

Articulating your objective in this concise and concrete way has two benefits. First, it forces you to stop and think: Really, what kind of information do I need given my formulation of the decision problem? This is a nontrivial benefit. Although a decision problem has been articulated, this problem was extracted from a mess, and that mess tends to reappear in the form of a wide range of poorly articulated issues and queries. Most managers are buffeted by numerous conflicting deadlines, interruptions, sudden changes of course, and the like. A requirement to spell out the specific information desired from this market research expenditure usefully concentrates the mind.

A second benefit of spelling out your objective is that you often discover that the objective you have just written out is insufficient—it reflects only part of what you are trying to accomplish. In conceptual terms, articulating research objectives represents a continuation and intensification of the initial attempt to formulate the decision problem. To continue the example given above, you may well realize that your actual objective is more comprehensive and better corresponds to this two-part statement: (1) identify areas of satisfaction and dissatisfaction and (2) prioritize areas of dissatisfaction according to degree of negative impact on revenue. Having reached this point, you may realize that the research procedures required to *identify* areas of dissatisfaction are not the same as those required to *prioritize* them. To identify requires an exploratory approach that can uncover what exists; to prioritize requires a precise and confirmatory approach that can take a set of existing things and order them from best to worst or most to least. With that realization, you are well on your way to articulating a research *strategy* encompassing multiple data collection activities that holds some promise of meeting all your information needs with respect to the decision problem at hand.

Table 2.2 lists a dozen verbs that often form the basis of research objectives along with some examples of typical objects for each verb. Thus, one can *identify* opportunities or problems or choice criteria, *select* markets or product concepts or ad themes, and so forth. Table 2.2 may not reflect *all* the verbs that provide a useful starting point for formulating market research objectives, but

Table 2.2 Examples of Research Objectives

Verb	*Some Possible Objects*
Identify:	Problems, opportunities, choice criteria . . .
Define:	Concept, design, potential . . .
Describe:	Decision process, usage, work environment . . .
Explore:	Perceptions, reactions, remedies . . .
Generate:	Hypotheses, alternatives, explanations . . .
Evaluate:	Feasibility, attractiveness, potential . . .
Select:	Product, concept, ad execution . . .
Test:	Preference, direction, profitability . . .
Measure:	Growth, size, frequency . . .
Prioritize:	Segments, needs, opportunities . . .
Monitor:	Trends, competition, events . . .
Track:	Spending, satisfaction, awareness . . .

it should cover most situations you will encounter. If you want to use a verb from outside this list, ask yourself whether it really adds anything and especially whether it is concrete and specific enough. For instance, in my experience, a favorite word of businesspeople in the context of market research is *validate*. But what does this mean? To validate is to confirm the correctness of some idea you hold—in other words, to test. Whereas *validate* is a long and somewhat unfamiliar word, thus vague in applicability and diffuse in meaning, *test* makes it clear that you are going to attempt to prove the truth of some proposition using fairly rigorous means. With *validate*, you could kid yourself that a dozen customer visits might be enough to validate your idea, whereas with *test* you are unlikely to convince yourself or anyone else that a dozen interviews is adequate. Hence, *test* is a more useful word because it gives more guidance as to what kind of market research might be able to fulfill your objective. *Validate* blurs the focus of your research planning; *test* sharpens it.

Decision Stages, Objectives, and Tools

Next, Table 2.3 integrates decision stages, research objectives, and individual research techniques. For each stage, certain research objectives are characteristic and customary. In turn, each research tool plays a primary role in achieving certain objectives and can contribute secondarily to the achievement of others. Table 2.3 is intended to serve several purposes. First, it provides the means to perform a quick check on a research proposal submitted by someone else in your organization. If someone wants to do focus groups in order to *select* which ad execution will have the strongest appeal, a warning light should go off in your mind: Focus groups are not listed among the tools used to select an option. Second, Table 2.3 provides a planning and scheduling tool for specifying needed market research over the life of a project. It affords you multiple opportunities to ask questions such as, What activities am I going to undertake so as to scan the environment? or, How will I go about identifying possible new applications for this instrument? A third benefit of Table 2.3 is that it provides three possible entry points to kick off your market research planning. Sometimes you will feel most confident about where you are in the decision cycle; sometimes a particular verb like *identify* or *explore* will be the hook; and sometimes you will be focused on a particular research tool. You can enter Table 2.3 from any of these points and build toward a complete research strategy from that point.

Table 2.4 provides an alternative viewpoint on the relationships mapped in Table 2.3. Now the individual research tools provide the rows and the individual research objectives the columns in a matrix. Where Table 2.3 was decision focused, Table 2.4 is tool focused. It facilitates correct use of each tool via the graphic symbols, which specify that the tool is a primary means of achieving an objective (double check), contributes secondarily to that objective (single check), or is generally misleading or dangerous in the context of a certain objective (X-mark). Blank cells indicate either that a tool bears little relationship to a certain objective, and hence, no warning is needed, or that it is meaningless to make any overall endorsement or prohibition, because so much depends on how the objective is interpreted in the specific case.

EFFECTIVE APPLICATION OF RESEARCH TOOLS

Parts II and III of this book discuss in considerable detail the strengths and weaknesses and best applications and misapplications of individual research

Table 2.3 Decision Stages, Research Objectives, and Research Tools

Stage	*Objectives*	*Tools*	
		Primary	*Supporting*
Scan environment – What's out there? – What's going on?	• Identify • Describe • Monitor	• Secondary research • Customer visits	• Focus groups • Surveys
Generate options – What are the possibilities?	• Generate • Define • Explore	• Customer visits • Focus groups	• Secondary research
Select option – How much will we achieve? – Which one is best?	• Evaluate • Test • Select • Prioritize	• Experiments, surveys • Conjoint	• Secondary research
Evaluate outcomes – How well did we do?	• Measure • Track	• Surveys • Secondary research	• Customer visits

tools. Here, in the course of elaborating on Tables 2.3 and 2.4, I will only attempt to flesh out the brief description of the tools given in Table 1.1. The focus here is primarily on the research objectives and how each tool relates to them; subsequent chapters focus on the tools and their execution.

To set the stage for this discussion, it helps to revisit the toolbox metaphor that underlies this book's treatment of market research. The toolbox has several compartments, corresponding, for instance, to the distinction between exploratory and confirmatory research. Within each compartment, there is the equivalent of a hammer, screwdriver, wrench, saw, and so on. It would be silly to always go to the same compartment to use the same tool as the last time you had an information gap. Yet it is not uncommon to encounter businesses that, faced with a need for market research, *only* conduct surveys or *only* do customer visits or *always* do focus groups. You will be much more effective if you can acquire a sense of the distinctive contribution of each tool together with an understanding of how the tools work together over the course of a project.

Every tool in the carpenter's toolbox is adapted to performing a specific task: hammers for driving nails, saws for making smaller pieces, wrenches for tightening bolts. It is the same with the market research toolbox: Each tool is effective in certain applications and ineffective in others. Just as market

Table 2.4 Research Tools Matched to Research Objectives

	Stages/Objectives											
	Scan Environment			*Generate Options*			*Select Options*				*Evaluate Outcomes*	
Tool	*Identify*	*Describe*	*Monitor*	*Generate*	*Define*	*Explore*	*Test*	*Evaluate*	*Prioritize*	*Select*	*Measure*	*Track*
Secondary research	✓✓	✓✓	✓✓	✓	✓	✓	✓	✓	✓	✓	✓✓	✓✓
Customer visits	✓✓	✓✓	✓✓	✓✓	✓✓	✓✓	X	X		X	X	
Focus groups	✓	✓		✓✓	✓✓	✓✓	X	X		X	X	
Survey research	✓	✓✓	✓✓	X	✓	X	✓✓	✓✓	✓✓	✓✓	✓✓	✓✓
Conjoint	X					X	✓✓	✓✓	✓✓	✓✓		
Experimentation	X	X		X	X	X	✓✓	✓✓	✓✓	✓✓	✓✓	✓

Note: A double check indicates that a tool is a superior means of addressing the objective, a single check indicates it is appropriate for pursuing the objective, and an X indicates that the tool is *not* appropriate for this objective. Blanks indicate that the appropriateness or inappropriateness of the tool is uncertain, depending on exactly how the objective is interpreted in a particular context.

research should be done only when there is a payoff, each individual research technique should be used only where effective. There is no requirement to always visit customers or always field a survey or always use any one of these techniques.

Secondary Market Research

This research technique encompasses any data collected by someone else for some other purpose that also happens to be useful to you as you pursue your purposes. Common examples of external secondary research include data compiled by the Census Bureau and other government agencies, reports written by consulting firms and sold to interested parties (e.g., "Five-Year Projections for Mobile Advertising Revenue"), and publicly available information, such as articles in the trade press. Searching Google to understand what your competitors are doing would also be secondary research, as would almost any other Internet search intended to support a particular marketing decision. Common examples of internal secondary research would be sales records, customer databases, and past market research reports.

Secondary research has obvious relevance to the environmental scanning stage of the decision cycle. It is almost always quicker and cheaper to answer a question through secondary data than through conducting your own primary market research. In virtually every project, your first step should be to amass whatever secondary research is available and glean whatever insights you can. Secondary research can be used to identify market opportunities, describe market structure, and monitor competitive activity. For example, suppose you install and service video cameras used for security purposes. Using secondary research, you might discover that automated teller machines (ATMs) in non-bank locations offer a rapidly growing market for video security. You might encounter this fact in the trade press or perhaps in a syndicated report on market trends for small video cameras.

Because secondary research comprises so many diverse activities, one or another kind of secondary research may also play a supporting role in both generating *and* selecting options. Thus, a market opportunity identified at an earlier point may be further defined through secondary research. Continuing with our example, secondary research might help you formulate two market expansion options: (1) target large banks with extensive off-premises ATM networks or (2) target convenience store chains that have recently installed

ATMs in their stores. Information on market size or market structure gained through secondary research may also help you evaluate the relative profitability of two different strategic options. Thus, your own internal records may indicate to you that cameras mounted in very small stores require, on average, more servicing than cameras located in larger buildings, for which customers are billed per occurrence. This might be sufficient to cause you to select convenience stores as your initial target market, inasmuch as cameras associated with their new ATMs are likely to generate substantial service revenue.

A particular type of secondary research becomes of primary importance when you reach the fourth stage. Quite often you want to evaluate the outcome of a decision by measuring changes in market share for yourself and key competitors. Syndicated reports (regular studies, produced by independent consulting firms, to which you and other members of your industry subscribe) are often a source of market share data. Nielsen is a prominent example in the consumer sphere. Alternatively, your own review of secondary data may help you answer this question. Thus, if you can find information on how many ATMs were installed in a region last year, you can compute your share of these installations relative to your goals.

Big Data

Since Big Data, for purposes of this book, is secondary research writ large, with a particular focus on web behaviors and web-based databases, its profile in terms of the objectives served is very similar to that of secondary research overall. Here I'll simply give another example of its use. Suppose you are a service business of some kind—a vendor who offers training in some set of business skills, for instance. You will have a website, of course, and this site will serve multiple purposes: to promote individual training modules, to handle registration, and generally, to help people find you and assess whether they might benefit from what you offer. Since you are selling knowledge and skill, you will probably populate this site with more than promotional content: you may put up some teasers tied to particular workshops, maybe maintain a blog, include a reading or two, and so forth.

Where Big Data comes in is that if you are a business of any size, your website may record thousands of page views per day. Because the site consists of hundreds of pages, there are many possible routes through the site. The environmental scanning question here is not so much "What's going on?" as

"How are people using my website, and which elements are working well or poorly?" Software such as Google Analytics or Adobe Marketing Cloud can handle the staggering amounts of data thrown off by the day-to-day operation of a website of this kind. You can get counts of where the user came from (i.e., whether they came from a keyword search ad you placed on Google, or the one you placed on Bing; or from the user's own unsolicited search). You can get counts of where people go after landing on your home page, and which of these secondary and tertiary destinations are more likely to take the browser to a scheduling or registration page—that is, which content on the site is most effective in actually selling your training. You might find that a blog post written several months ago continues to attract more than its share of views, even though people have to search for it. That's a post that arguably should be turned into a piece of standalone, enduring content, and featured more prominently.

In sum, Big Data on the web offers a much more fine-grained and complete documentation of some types of customer behavior than had ever been available before, with highly actionable relevance to both environmental scanning and assessment of how well you are doing.

Customer Visits

Customer visits can be thought of as a combination of exploratory observation and face-to-face interviews. Hence, visits may be very helpful in the environmental scanning stage. Listening to customers describe problems can help to identify new product opportunities. Walking around the customer site facilitates rich descriptions of product applications. Regular contact with customers helps you to monitor emerging market trends and changes in the business environment.

Customer visits are also crucially important, along with focus groups, in the generation of options. This is because the loosely structured nature of these interviews allows for surprises. Similarly, extensive exposure to customers and their way of viewing the world often provides a fresh perspective. Moreover, the intensive dialogue that a two-hour face-to-face interview permits helps you to define issues and explore perceptions in depth.

Customer visits should almost never be used to test, evaluate, or select options. The small sample size and an unknown degree of interviewer bias make it impossible to trust the results of customer visits in this connection. As will be developed subsequently, these same shortcomings are less of an issue

when customer visits are used appropriately to scan the environment and generate options. The lone exception is when you are planning to visit *all* your customers. This might be possible because these customers are all other divisions internal to your firm, or because the market for your product is very limited with only a few large buyers. If you can visit all your customers, then you have a census and not a sample, and the limitations cited above are less pressing. Even here, the portion of your visit devoted to testing and selecting among options will probably have a quite different feel relative to the rest of the visit and relative to more conventional applications of the visitation tool. To explore and to confirm are profoundly different activities.

Customer visits may sometimes play a minor supporting role in the evaluation of decision outcomes. Although in principle, customer visits are just as ill-suited to measuring and tracking as to testing and selecting, visits can potentially supplement more formal and confirmatory approaches such as survey research. Thus, although it is important to confirm whether your customer satisfaction numbers have gone up or down, it will not always be clear *why* the pattern of results takes the form it does. In this situation, a series of visits to customers whose satisfaction has increased and to customers whose satisfaction has not changed or has gotten worse is often illuminating. Such an application of customer visits serves as a reminder that the final stage of one decision cycle tends to merge with the first stage of the next decision cycle.

Focus Groups

In a focus group, 8 to 12 consumers meet in a special facility for approximately two hours. The facility enables you to view the group from behind a one-way mirror and to make audio and video recordings. The group discussion is moderated by a professional interviewer in accordance with objectives set by you. Focus groups are very similar to customer visits in being exploratory interviews, except that they lack an observational component. Hence, they are somewhat more narrow in their applicability. To a considerable degree, however, customer visits and focus groups are substitutes for one another, presenting the same opportunities and suffering from the same limitations. In the course of a given research project, most firms will do one or the other, but not both, if interviews are pertinent at all.

Same as any kind of interview, focus group studies can be useful in the initial exploratory stages of the decision cycle where you are scanning the

environment and generating options. For instance, you might do some focus groups to identify emerging issues as viewed by customers within a particular segment of the market. At a later point, you might use focus groups to explore the pros and cons of several possible themes being considered for a new ad campaign. Part of generating options is defining these options in as much detail as possible, and the give-and-take of group interaction can be quite productive in this respect.

Focus groups are probably more effective at exploring, defining, and generating (Stage 2) than at identifying, describing, and monitoring (Stage 1); hence, their relegation to a contributing role during the environmental scanning stage. The power of focus groups comes from the interaction of customers within the group and whatever synergy results. The stimulus of group interaction is particularly useful when the goal is to generate fresh perspectives, define the differences among subgroups within the market, or explore consumer reactions. It is less useful when you want extensive descriptive data.

As with customer visits, generally speaking, focus groups should never be used to select among options. Again, the problem centers on the small samples of customers involved. Similarly, the skill brought by the outside interviewer to the conduct of focus groups may be more than outweighed by the distorting potential of group influence and dominant participants. Problems of group influence and conformity pressure, together with the fact that focus groups are a laboratory rather than field procedure, make it impossible to recommend their use for even a contributing role during Stage 4, evaluation of outcomes. In this sense, focus groups constitute a more specialized tool than either secondary research or customer visits.

Survey Research

A survey takes place when a fixed set of questions is asked of a large sample of customers. In many cases, the sample is carefully selected to represent the total population of interest. The questions are mostly descriptive and classificatory in nature: In a B2B context, questions asked of customers might include: “How big is your firm?” “When did you buy the product?” “How many other vendors did you consider?” and the like. Questions may also be structured as rating scales: for example, “on a 10-point scale where ‘10’ is outstanding, how would you rate our performance on each of these aspects of service delivery . . .” What makes “asking customers questions” a survey

rather than an interview is that the questions are fixed and asked the same way of every customer every time. Surveys provide a quantitative approach to asking questions of customers, interviews a qualitative approach.

Surveys can play a supporting role in environmental scanning. If you need a fairly exact factual description of the behaviors and simple perceptions of some customer group and if such data cannot be gleaned from existing secondary research, then it may make sense to execute a survey. If, however, good secondary data already exist, it is rarely cost-effective to do your own survey unless this takes the form of a small, fast, tailored survey directed at filling in a few gaps in the available secondary data. If the needed secondary data do not exist, and if you simply must have precise descriptive data on such matters as the frequency of certain applications among particular customer groups, or the average dollar amount of equipment purchases, or the average rating of your speed of service response relative to key competitors, then a survey may make sense.

You should ask yourself, however, whether you really need precise descriptive data at this early point in the decision cycle. Is it really that important to be able to state with precision that 54% of the time, this medical instrument will be used on auto accident victims, 24% on mothers undergoing childbirth, 18% on victims of gunshot wounds, and 4% with others? At this early point, what is the value added by these precise percentages as opposed to what you could gain from a program of customer visits? A couple of dozen visits would probably reveal that auto accidents, childbirth, and gunshot wounds were "major" applications, even though the exact percentages would be uncertain. In addition, and in contrast to the limited data supplied by a survey, the visits would provide opportunities to describe in depth how each of these applications place different demands on the instrument and on hospital staff, how this instrument interfaces with other equipment in the hospital, and so forth. Such rich descriptive data are often more useful, early in the decision cycle, than the thinner but more precise data yielded by surveys.

It is even more important to understand that surveys are far less useful in the generation of options than customer visits or focus groups. The relative weakness of surveys at this point in the decision cycle has several sources: (1) the fact that the questions to be asked are fixed in advance; (2) the reality that the phone interviewers who may implement the survey probably lack the ability, the motivation, or the opportunity to deeply probe customer answers, and that customers racing through a self-administered web survey will be similarly unmotivated; and (3) the unfortunate truth that the impersonal nature

of the survey contact—the certain knowledge that one's responses are but grist for the statistical mill—will inhibit and limit the customer's investment of the energy required for discovery, exploration, and depth. Surveys are a confirmatory tool whose proper purpose is to limit, narrow, and specify; hence, this tool is largely incapable of expanding, broadening, and reconfiguring your understanding. Go easy on surveys early in the decision cycle.

Survey research comes into its own at the third stage of the decision cycle. All of the features that had been of dubious relevance or even liabilities at the earlier stages are here either neutralized or converted into strengths. In Stage 3, the time for discovery and in-depth insight is past; now it is time to make hard choices and allocate limited resources. Perhaps you only have the resources to write new software for one or at most two of your instrument's applications, and you must determine which application predominates. Large investments may follow from decisions of this type, and it makes sense to invest a sum of money in determining precisely which application is largest, is growing the fastest, or has the weakest competitive presence.

Survey research is also of primary importance in the evaluation of outcomes. The classic example is the customer satisfaction surveys now conducted by many firms. Whether administered through the web or by telephone, in such surveys, often conducted by a neutral outside firm, a standard series of questions is asked focusing on product and vendor performance. The surveys are often repeated on a quarterly basis so that changes in satisfaction can be tracked over time. Another example is the tracking studies conducted after initiating an advertising campaign. These telephone surveys track awareness, brand attitude, and perceptions in those areas addressed by the advertising campaign. Here again, descriptive precision is an absolute requirement; otherwise, comparison over time becomes impossible.

Experiments

The purpose of an experiment is to test which among a small number of treatments stimulates the greatest response. For example, you may be considering two different appeals for use in an e-mail promotion, and you want to know which appeal will be most successful in driving customers to your website. You could draw a sample of 1,200 e-mail addresses, randomly assign 600 each to receive alternative versions, and then count which appeal produced the largest number of hits to your website.

Experiments are highly specific in their contribution to decision making. Experimentation is *not* of much use in the initial stages of environmental scanning and option generation or in the final stage of outcome evaluation. Early in the decision cycle, you don't know enough to design a good experiment, whereas toward the end of the cycle, you want market data on what actually occurred, not experimental predictions of what is most likely to occur. Experiments are primarily intended for use in option selection. In fact, their design corresponds exactly to the crux of many business decisions: that is, which of these options is the best? Moreover, experiments can sometimes answer a related and very important question: How *much* will we achieve? For instance, the response rate for the winning headline in the direct mail example would allow us to estimate what the response rate will be for the mass mailing, and this in turn allows us to draw up a pro forma income statement showing the cost of the promotion and the anticipated revenue gain. Some kinds of conjoint analyses can generate such estimates as well, but arguably on a weaker empirical basis than in the case of field experiments.

Conjoint Analysis

In a conjoint study, consumers are presented with various product configurations consisting of a set of features each delivered at a specified level, and asked to indicate their preference. Thus, a computer monitor might be described in terms of resolution, price, screen size, contrast ratio, and so forth. Some subset of all the possible permutations is rated, and the mathematical analysis of consumer preferences gives insight into how consumers make trade-offs among different features and price points. Although there are many different ways to implement conjoint studies, regardless of format, the goal is always to build a model of how a customer makes a choice among the various product offerings available and, thus, to identify and quantify choice drivers (for example, how many dollars more, if any, will a consumer pay for a monitor with a resolution of 1080 × 1920?). The goal of conjoint analysis is thus to answer questions such as, "Which product attributes are most influential on the purchase decision?" and "How do customers make trade-offs between performance level and price?"

Conjoint analysis is again a valuable tool with strictly limited applicability. It makes little sense to use conjoint analysis during environmental

scanning. Too little is known to justify use of a precise and narrowly focused tool of this kind. Conjoint analysis is not really appropriate for the generation of options, either. This is because to perform conjoint analysis, one must be able to say exactly what the key product attributes are, and part of the purpose of generating options is precisely to discover what product attributes might matter at all. Logically, environmental scanning and options generation precede and lay a foundation for more confirmatory techniques such as conjoint analysis.

The primary purpose of conjoint analysis is to assist in the selection of the best option in the specific sense of the optimal product configuration or price–performance point. When serious uncertainty remains about whether one bundle of features or another is the most attractive to consumers or about how to construct the optimal bundle of features, conjoint analysis is often a good choice. In turn, by the time one gets to the fourth and final stage of evaluating outcomes, as with experiments generally, the time for conjoint analysis has probably passed.

SUMMARY

Now that the contents of the market research toolbox have been spread out before you and each tool briefly situated within the decision cycle, a few summary statements are in order.

1. Secondary research is *the* all-purpose market research tool. Partly because of the great diversity of the types of information that can be obtained and partly because much secondary research is both cheap and quickly obtainable, your first impulse in planning any inquiry into customers and markets should be to ask, Has somebody else already gathered useful information on which I could build? The answer won't always be "yes," but the question should always be asked.

2. Interviews and surveys are probably the most heavily used techniques. The application of both these tools is a matter of asking questions and getting answers. If the issues with which you are concerned can be phrased as direct questions that customers are able to answer, then interviews or surveys will probably be rewarding.

3. Customer visits and focus groups anchor the exploratory end of the continuum. Here you may have some sense of what your key issues are or what some of your questions may be, but you are uncertain about what kinds of answers are even possible. By contrast, surveys anchor the confirmatory end if *descriptive* information is the goal. Here you know both the key questions and the range of possible answers, and your goal is to pin down the exact frequency of each possible answer.

4. The selection of options, unlike the other decision stages, tends to require highly specialized research tools such as conjoint analysis and experimentation. It is an error and a mark of ignorance if the management of a firm exclusively conducts customer visits, or surveys, or a review of secondary resources when the primary goal is to select an option. Selecting the best option—pricing is a good example—often requires you to go beyond asking questions of customers and to instead create environments in which customers act or choose so that you can analyze these behaviors to infer the answers you require. Both conjoint studies and experiments take this approach.

DOs AND DON'Ts

Do plan on using a variety of techniques over the course of a project. Make every effort to find the right tool for the job at hand. Every tool is specialized, and no tool is perfect.

Don't confuse exploratory and confirmatory techniques. Don't try to squeeze precision out of tools that can't provide it, and don't expect discoveries and new insights out of tools whose purpose is to narrow down the possibilities and eliminate options.

Don't fixate on specific research tools. Keep the focus on the decision to be made and what information would be most helpful. Let the tool follow from the research objective.

DISCUSSION QUESTIONS

1. Evaluate the claim that "every research technique has both special strengths and key limitations."

a. Are there really no globally superior research techniques—no technique so robust, so scientifically sound, as to have wide applicability with few limitations?

b. And are there really no globally inferior or weak market research techniques, with few strengths, that should generally be avoided, however familiar or customary?

c. Focus your answer on the pragmatic circumstances of a manager with profit and loss responsibility, and an investment mindset, as described in the appendix to this chapter.

d. Would your answer be different in the B2B versus the B2C sphere?

2. What should a manager do if market research is likely to be valuable, but no funds are available? To answer this question, be specific about the options you selected among. Doing absolutely nothing to gather information is certainly one option, but there are others as well. Which of these options is the most managerially sound way to proceed when market research really would have been helpful, but no funds are available?

   a. This question is best answered by anchoring yourself to a specific product category and a particular type of marketing decision (i.e., new product introduction, selection of customers to target).

      Extra Credit: Select two different product categories, broadly defined, and answer for both. For example, discuss a B2B versus B2C case, or an intangible service versus a consumer packaged good.

   b. Now, to vary the question a bit: Suppose there are some funds available, but these are insufficient to do all the market research that would definitely be helpful. Perhaps there are enough funds to do an interview study but not follow it up with a survey; or enough to do a conjoint analysis but not lay the foundation with interviews. What's the best way to proceed, in general, when there are not enough funds to do everything that should be done?

      This time, for extra credit, discuss B2B versus B2C cases specifically.

SUGGESTED READINGS

Churchill, G. A., & Iacobucci, D. (2009). *Marketing research: Methodological foundations*, 10th ed. Chicago, IL: Cengage. Malhotra, N. (2009). *Marketing research: An applied orientation*, 6th ed. Ontario, Canada: Pearson Education.	These are standard textbooks on marketing research that provide more detailed coverage of the specific tools discussed here and a thorough introduction to the statistical analysis of market research data.
Grover, R., & Vriens, M. (2006). *The handbook of marketing research: Uses, misuses, and future advances*. Thousand Oaks, CA: Sage.	If you are an established professional new to marketing research but with advanced training elsewhere in the social sciences, you may find this volume more palatable than the two student-focused textbooks listed above.

APPENDIX 2A: FINANCIAL PLANNING FOR MARKET RESEARCH

An important part of planning for market research is estimating the budget required to fund the desired project(s). In this appendix, I assume that some kind of market research seems to be indicated, so the financial question centers on how much to spend. The formula given also allows a determination that very little spending can be justified, in which case market research will probably not be done. However, a fuller account of the boundary conditions on market research, including potential payoff or lack thereof, is reserved for the final chapter.

This simple equation lays out the conceptual issues involved in developing a budget for market research:

$$\text{Market Research Budget} = K \times R \times 1/F$$

Let K be the amount at stake with respect to the decision the research is intended to support. This amount is the contribution to profit that could be lost or foregone if the wrong decision is made (let contribution equal revenue

minus the cost of goods sold, including fixed costs directly attributable to the decision, e.g., new capital equipment required to launch a new product). For instance, if, upon introduction, crucial features in the product are lacking, what would that cost you? If you end up targeting the wrong application or the wrong group of customers, how expensive would this mistake be? If the product is a bust and withdrawn from the market, what would be the charge against profit?

In a corporate context, for most new product decisions, *K* may be an amount in the millions of dollars. If we drop down to the level of some particular marketing initiative—for example, reorganizing sales territories to align with customer segments—the amount at stake may drop to the hundreds of thousands of dollars. A simple rule of thumb is that as *K* begins to drop below $500,000, it becomes more and more difficult to cost-justify any kind of formal market research beyond secondary research. You can always go on Google for a few hours for nothing out of pocket, and in most cases you should; but as soon as you begin contemplating formal market research, the meter starts at about $10,000 and quickly ticks higher. And as developed next, that level of expenditure presupposes a *K* of $500,000 or more. One can of course do poorly conceived and ineffective market research for much less than $10,000; but why spend any money at all on faulty or imprecise data?

Second, let *R* be the reduction in the odds, expressed as a percentage, of making a wrong decision. This number is going to be quite a bit more fuzzy than the first number but can be estimated as follows. Suppose that 50% of new product introductions in your industry break even or turn a profit. The odds of failure, defined as incurring a loss, might then be estimated as 50%. You might then suppose that if effective market research were done, the odds of failure would decrease to 33%. *R*, the reduction in the odds of failure, is then estimated as 17%. Note that when the situation is either terminally confusing, or already quite clear, then *R* will be very small, maybe close to zero.

In fact, *R* will be large only when candidates for the "right" decision can at least be glimpsed, *and* when there are a manageable number of such candidates, *and* when there is little confidence or consensus among decision makers about which candidate decision is the best one to make. If decision makers are confident they know what to do, how is market research really going to improve the decision (or even alter it)? If the number of viable directions is large, will the cost of good market research exceed the payoff? If the path forward is terminally uncertain, is it believable that market research can

improve the odds? Again, a more extended account of some of these limiting conditions will be given in the final chapter.

The underlying model here may be familiar to you from the decision-making and investment literatures. What the equation does is estimate an expected return by quantifying an outcome and then weighting this quantity by its probability of occurrence. The logic may become more clear if we let K and R be vectors rather than simple quantities. In that event, K^1 through K^5 might correspond to the financial impact of (1) a disastrous new product introduction, (2) a disappointing new product introduction, (3) a mediocre product introduction, (4) a good but not great product introduction, and (5) a blockbuster success. R^1 would then be the reduction in odds of a *disaster* to be expected from conducting market research, R^2 would be the reduction in the odds of a *disappointment*, R^3 would be the reduction (or increase) in the odds of a *mediocre* result, R^4 the increase in the odds of a *good* result, and R^5 the increase in the odds of achieving a *blockbuster*. Multiplying each element of K by the corresponding element of R and summing over the products would then yield the expected return from conducting market research.

F, the final element in the equation, can be defined as the desired return on investment (ROI) on the market research expenditure. It might also be thought of as a fudge factor. The result obtained from $K \times R$ reflects a chain of assumptions and guesses and will always be somewhat uncertain. It would be unfortunate if you invested $250,000 in market research, based on an expected return ($K \times R$) of $900,000, when in fact the possible return was only $200,000 or so. Letting F be a number like 5, 10, or even 20 makes it much more likely that the market research investment will return a multiple of itself—as opposed to more or less netting out to no gain. In my opinion, F should never be less than 5, corresponding to an ROI of 500%. There are just too many other expenditures a manager could make to improve the odds of success, and market research is just too fallible an activity to justify any lower bar for target ROI.

The analogy here is to value-based pricing. In that approach to pricing, if your offering is estimated to save the customer $200, you can't price it at $200, or $175, or even $125—a customer will not feel motivated to spend a *certain* $125 to *maybe* save $200. Only if you set a price on the order of $40 or even $20 will the customer be motivated to pay that certain amount to achieve savings that are only promised and hypothetical. The fudge factor serves the same purpose in the context of setting a maximum market research

budget. If F is set at 5, then the market research investment has the potential to pay for itself five times over. This is appropriate, because there are so many other investments you could make to improve the odds of new product success (additional research and development [R&D], larger advertising budget, etc.), and many of these alternatives will claim to pay back a multiple of themselves. The actual value of F in an individual case will vary with the conservatism of the firm and the perceived certainty of the estimate of $K \times R$. The more foreign market research is to the corporate culture (not uncommon in the case of technology companies in Silicon Valley) or the more skeptical the stance of management, the higher F should be.

The utility of the budget equation becomes apparent when it is combined with basic cost information concerning market research. These cost data will be discussed in more detail under the individual techniques, but some basic guidelines can be given. First, as mentioned earlier, $10,000 is about the floor for any execution of a particular market research technique (secondary data can of course cost much less, and I will return to this point). A more common level of expenditure for an individual technique would be $20,000 to $30,000, and most projects of any magnitude will want to combine multiple research techniques. As a rule of thumb, then, a meaningful market research effort over the life of, say, a new product development project is unlikely to cost less than $50,000, will often exceed the $100,000 range, and may require much, much more.

With this cost information in hand, the financial planning equation can be put to work. First, let the corporate contribution margin be 25%, let the reduction in the odds of failure attributable to good market research be 10%, and let the fudge factor be 20. Translated, assume a skeptical management, skimpy margins, and a lot of uncertainty. With these numbers, you can see that the new product has to have revenue potential of about $60,000,000 if one is to justify a market research budget on the order of $75,000. Specifically,

- If an important mistake in product design will cause the product to only break even rather than make a normal contribution to profit, the cost of a mistake (the amount at stake) is $15,000,000 (= 25% contribution × $60,000,000 in sales revenue).
- The maximum market research budget is then $1,500,000 (due to the expected 10% reduction in the odds of making a mistake).
- Applying the fudge factor of 20 yields the budget of $75,000.

By jiggering any of the assumptions just made, one can easily get the required revenue potential down to $20,000,000 or so. Thus, there exist software and other technology businesses with contribution margins well above 25% and even 50%. Alternatively, it may be more reasonable to assume that a mistaken product will produce an actual *loss* rather than break even. Moreover, the situation might be such that the reduction in the odds of error due to good market research will be more than 10% (but I discourage students from setting *R* much above 20% to 30%). Last, a more market-focused or more confident corporate culture might set a lower fudge factor. By a somewhat more heroic rearrangement of our assumptions, combining any two of the revisions just named, we could get the revenue level down under $10,000,000. Even by the most heroic assumptions, $2 million to $4 million, in terms of product revenue, is probably the lower limit for justifying a sophisticated market research effort that includes at least two distinct data-collection efforts and costs upward of $50,000.

Quite a number of useful conclusions emerge from this financial analysis. On the one hand, any Fortune 1000 corporation has many, many products with annual revenue potential in the tens of millions of dollars range, indicating again the pervasive opportunity for conducting market research. On the other hand, most small businesses and most technology startups will have to use ingenuity and rely heavily on secondary data and seat-of-the-pants reasoning rather than on market research studies per se (see the final chapter). In fact, it is probably fair to say that most mom-and-pop businesses cannot afford to purchase conventional market research. Much can be done on a shoestring, but it will mostly consist of secondary data along with an open, inquiring frame of mind.

Continuing along these lines, the higher the profit margin, the greater the opportunity to do market research, or make any other investment in long-term market success. Conversely, the lower the capital costs for introducing and then terminating a failed new product, the less the justifiable expenditure on market research. When I began consulting for insurance and financial services firms, I was quite struck by the contrast between their research budgeting and that of the equipment manufacturers with which I was then most familiar. To design and manufacture a new instrument or other electronic product inevitably entails a substantial R&D and capital expenditure. Introducing a new financial service or program often incurs modest costs that are several orders of magnitude less. In such cases, actual market introduction provides a relatively quick and

inexpensive test of whether the program was or was not a good idea. Given this low cost of test-by-launch, upfront market research has to be inexpensive if it is to be done at all in the financial services sphere. Moral of the story: If it won't cost you much to be wrong, then you also should not spend very much on market research.

The logic of the equation has particularly troubling implications for program managers. This job category includes people who manage documentation, customer service, or lines of product accessories and the like. Program managers have no less need for market and customer information than project and product managers (these are parallel job titles in the engineering and marketing functions), but their efforts seldom have the kind of assignable revenue impact required to justify a substantial market research budget. Two solutions make sense for people in the program manager position. The first is to concentrate on secondary data, and the second is to find ways to piggyback on the market research efforts of project and product managers. If a program manager can add a question or two to a research study, this may have little effect on the cost of the study while yielding an invaluable supplement to his or her ongoing effort to stay on top of market developments. Program managers who regularly execute such piggyback strategies gain a constant stream of research data at little direct cost.

On a final note, a more subtle implication of the financial equation is that a short-term focus makes it difficult to adequately budget for market research. For technology companies in particular, substantial market research efforts may be best focused at the *product platform* level and not at the level of an individual product configuration. That is, just as smartphone manufacturers offer a variety of screen sizes, camera capacities and memory, at different quality levels, so also many technology products come in large and small, high-end and low-end versions, each aimed at a particular application or industry segment. Although each is a somewhat different product, all rest on the same basic assembly of technologies—the platform. Sales at the platform level, especially over the several years' life of the platform, will almost always be large enough to justify a substantial research budget, because although product life cycles have often shrunk to months, platform life cycles still last for years. Unfortunately, accounting systems and organizational groupings are often structured in terms of products. If the platform has no budget code, and if no team or individual has platform responsibility, then effective budgeting for market research becomes difficult.

Stepping back, the financial equation provides a way of acting on the truism that market research has to be considered an investment. It becomes clear that market research really *is* expensive and that the stakes have to be high to justify it. Conversely, the equation serves as a lever for use with those penny-wise, pound-foolish technical managers who choke at the idea of spending tens of thousands of dollars on something as intangible and squishy as market research. When a new product line is expected to generate revenue on the order of $100 million and there are some excruciating uncertainties concerning its design and intended audience, then a market research expenditure of $100,000 is a trivial price to pay if the odds of success can be materially improved. Note again that this kind of high-stakes situation is most likely to arise at the level of a product line or product platform and is much less common at the level of an individual product configuration or stock-keeping unit.

Note also that while the *K* component in the equation provides a bracing reminder that market research planning is basically about money payoffs, the *R* component provides an equally important reminder that market research itself boils down to uncertainty reduction. To the extent that you feel certain about what will happen or what will work, market research grows less necessary. For instance, if management has already made up its mind, for good or bad reasons, then market research can't reduce the odds of a wrong decision because it is not going to have *any* effect on the decision. Studies conducted under these circumstances are just politics and basically a waste of time and money. Conversely, when uncertainty is very high—your environment is essentially chaotic—market research may be beside the point. Since this situation is the more common one in technology firms, an example might help. Suppose that the success or failure of a given project hinges entirely on whether the technical standard to which it adheres does or does not end up dominating the market some years hence. Suppose further that the dominance or defeat of that technical standard is not within the control of company management or of any definable group of people, that it will, in fact, be a function of so many interlocking factors that it is impossible to grasp their interrelations. In that situation, the most that market research may be able to offer is an early warning of whether the technical standard is or is not moving toward dominance. If that early warning would not be helpful, then it may be best to spend nothing at all on market research in this connection and put the money to other uses, such as lobbying for the chosen standard at technical gatherings.

See again the final chapter for a more extended development of this kind of boundary condition on market research.

Perhaps you expected more than "uncertainty reduction" from market research. You hoped, in a nutshell, to achieve some kind of *guarantee* of making the right decision. Not to be too blunt, you were naive. Market research is a social science, not a physical science, and a young social science at that. It can reduce uncertainty but never eliminate it. On average, across a large business, over a period of years, this small reduction in uncertainty can be very lucrative and repay the cost of the research many times over. But all market research can ever do is reduce the odds of making a costly error and increase the odds of making a profitable decision. If instead it is certainty that you want, then may I suggest you go to a chapel.

CASES FOR PART I

NOTE TO NONSTUDENT READERS

This is the first of a number of suggested cases newly incorporated into the fourth edition. They are primarily intended to support use of the book in a classroom setting.

But what if you are not a student reader of this book, but a member of the original target audience consisting of professionals seeking a briefing? For you, the cases provide real-life examples of complex business situations where market research may play a role. (Anyone can purchase these cases from http://hbsp.harvard.edu.)

The questions following the synopsis are meant to focus your reading. Ideally, you'll be able to discuss the case and your answers with someone more senior or experienced.

SUGGESTED CASE: THE COOP (HARVARD, #9-599-113)

Synopsis

Mr. Buckmeister, an entrepreneur who built a chain of 76 fast-food chicken restaurants from scratch, has a problem: After many years of above-category growth in sales, something has changed for the worse, and sales in some previously strong stores have been noticeably weak in the current year. He faces conflicting advice from his team about how to proceed.

Discussion questions

1. State the decision problem Mr. Buckmeister is facing. (Hint: The decision problem is *not* whether to do market research, or what research to do; it is always a business problem involving revenue, growth, profitability, market share, and the like.)

2. Is there an opportunity to invest in market research? Make the case for an expenditure on market research. What should he do first? And what next? And what after that?

3. Evaluate the various research proposals on offer, including Mr. Buckmeister's own proposal for a simple comment card to be left by the cash register.
   (a) Can any of the suggested techniques be ruled out as obviously off point?
   (b) Which ones, if any, appear most relevant to the decision problem?
4. Formulate a complete research plan, indicating what research to do first, and what follow-up research might be in order.
5. There's obviously some tension between two of his VPs, the one representing the Quality perspective, the other the Marketing view. If you are in a marketing class, you can't just take the side of the Marketing VP—you need to make the case about what's wrong, if anything, with the recommendations coming from the Quality VP.

SUPPLEMENTAL CASE: DATA FARM

Synopsis

This is a fictitious case to be used immediately following the Coop. It won't make any sense if the Coop case is not fresh in your mind. I wrote it to parallel the Coop situation in every way except one: Data Farm is a B2B technology case. It provides an opportunity to discuss how the exact same decision problem has to be addressed somewhat differently when the firm manufactures a high-cost tool sold to businesses, as opposed to a discretionary consumer good, such as fast food.

Discussion Questions

1. What should Mr. Bilsurvant do first, and then what next, and then what? Is market research potentially relevant here?
2. Review your recommended set of research activities for the Coop case. Which research activities will be the same for Data Farm, and which ones, if any, will have to be adjusted or replaced by some other technique or procedure?

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3. Overall, how different are the demands placed on market research by B2B technology firms, relative to firms selling a discretionary food purchase to consumers?

DATA FARM CASE

Data Farm is a manufacturer of rack servers. Its servers provide scalable processing capacity to a variety of customers, including:

(1) firms that provide outsourced data centers (i.e., take the day-to-day management of the corporate data center out of the hands of a corporation);

(2) website providers, who use the servers to support browsing of the website;

(3) corporate data centers that have not been outsourced; and

(4) laboratories and other specialized business operations that require a great deal of server capacity to support their day-to-day work. A portion of this business represents government entities.

The Data Farm product takes the form of server modules. Any number of modules can be combined. An individual module costs a few thousand dollars, but almost no customer buys just one, or even just 10; most purchase orders include dozens, hundreds, or thousands of modules. Because of the small size of the individual module, customers can buy precisely as much processing capacity as they need, and efficiently expand capacity when and as needed.

A dedicated sales force sells Data Farm products. Various service plans contribute substantially to overall revenue, as do supporting elements such as racks, inter-server and network connections, and management and monitoring software. Sales represent a mix of: (1) initial sales to new customers; (2) expanded capacity purchased by existing customers; and (3) upgrades to new versions of the server purchased to replace existing Data Farm servers.

Generally speaking, the rack server industry follows Moore's law: Processing power of a given size module doubles every 18 months. With changes in the supporting software, such as how the server interfaces with storage and/or with network routers, a rack server may be regarded as "old/out of date" after 24 to 40 months, and in most cases, will be obsolete after 36 to 48 months. Depending on customer operations, any time after 18 months of use, a business case can sometimes be made that it would be more effective to discard the still-functioning servers and replace them with the newest model.

CHALLENGE: SALES HAVE STALLED

In all other respects, Mr. Bilsurvant, the entrepreneur who heads up Data Farm, finds himself facing in 2010 exactly the same strategic challenge as Mr. Buckmeister of The Coop faced in 1995: After a long period of above-category growth, sales have stalled. Like The Coop, sales have been particularly problematic in about 20 sales territories.

Make these additional substitutions to see the parallels with The Coop in more detail:

For "76 restaurants"	Substitute "76 sales territories." All of Data Farm's servers are sold direct through its sales force, with a typical sale between $100K and $2M.
Instead of Anita McMichaels, VP of Quality, and her concern for quality and taste tests …	Make her VP of Sales, convinced that there is a problem with inadequate training and a need for systematic course development to improve selling skills. Or possibly, a need for a revamped sales compensation plan
Instead of Trevor Wallace, VP of Marketing …	Let him be VP of Business Development, arguing that the problem lies with the relative performance of the servers against competitors, and possibly their pricing, and not with any problem in sales training or sales force management.

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For the income statement …	Add a zero to every entry, so that Data Farm has about $500 million in sales.
Also in the income statement:	Make any other changes to convert this to a manufacturer's statement (substitute "manufacturing operations" for "store operations," etc.). In particular, substitute "sales force expense" for "advertising."

Keep everything else the same as The Coop.