



# Creating an R&D Strategy

Gary P. Pisano

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## ***Creating an R&D Strategy***

Gary P. Pisano  
Harry E. Figgie Professor of Business Administration  
Harvard Business School

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Perhaps no single issue today is higher on the agenda of senior management than improving innovation performance. Whether you operate in a technology intensive business like pharmaceuticals or electronics, or a more traditional manufacturing sector like automobiles, company growth in a highly competitive world hinges on superior R&D performance. And yet, no other endeavor frustrates management more than attempts to improve R&D performance. Many R&D organizations have endured repeated restructuring, oscillations between centralized to decentralized models, endless process re-engineering, a barrage of team concepts, and a host of other management interventions with little to show for it. It is no wonder that new attempts to “revolutionize” the R&D process are often met with jaded skepticism.

The failure of many organizations to improve R&D performance is not due to lack of effort or commitment by the management or people involved. It is due to a misconception about the drivers of R&D performance. Too often, R&D performance is boiled down to a few simple universal practices. Unfortunately, there is no one best model for R&D that is universally superior. There is no “magic bullet.” R&D performance results from the interaction of many different decisions and choices, including the size and location of R&D facilities, the division of labor between various groups, the choice of technologies used inside the R&D organization, the selection of personnel, the allocation of resources, the design of processes for managing projects, and other factors. An R&D organization is like any other system: performance hinges on the coherence between the components. And, like any other system, R&D organizations cannot be designed to do all things equally well. They face trade-offs. Every approach to R&D has strengths and weaknesses. It is because of the need for coherence and the need to manage trade-offs that R&D *strategy* is an essential ingredient for achieving superior R&D performance.

### ***The Concept of Strategy***

A strategy is nothing more than a *commitment to a pattern of behavior* intended to help win a competition. “Hit to Joe’s backhand” is a strategy you might deploy for your Saturday morning tennis game against your friend. It does not mean you will hit every shot to Joe’s backhand (presumably, his weaker stroke), but you will try to emphasize shots to that side. Company strategies have the same flavor. Apple’s strategy, for instance, is to develop easy-to-use, aesthetically-pleasing products that integrate seamlessly with a broader system of devices in the

consumer's digital world. This strategy provides a guiding orientation for a broad range of Apple's business decisions such as the selection of new R&D projects, the design of products, the composition of project teams, the choice of suppliers, the focus of marketing campaigns, the layout of Apple's retail stores, and even hiring of people. The strategy implies a pattern of behavior with respect to all of these decisions.

There are three essential purposes (and requirements) of a good strategy. A good strategy provides *consistency, coherence, and alignment*.

1. *Consistency*: Advantage is not the result of a single decision, but rather the cumulative outcome of a series of decisions, actions, and behaviors over time. A good strategy provides a framework for making consistent decisions over time that build cumulatively toward a desired objective.
2. *Coherence*: In a complex organization, many decisions are made each day that can shape competitive capabilities (who gets hired and promoted, which projects get funded, which pieces of equipment are bought, which partners are engaged for collaboration, etc.). They are often made in far-flung corners of the organization (and, today, in different parts of the globe). Strategy provides an integrating mechanism to ensure these tactical decisions are coherent. Without a strategy, it is impossible to achieve coherence. Organizations sometimes try to compensate for poor strategy by creating committees and other communication mechanisms to ensure decisions are integrated. But such devices are a poor and inefficient substitute for good, clear strategy.
3. *Alignment*: Organizations thrive when their strategies are aligned to the realities of the environment or the broader organizational context in which they operate. An R&D organization needs to have a strategy that is aligned with the broader business strategy of the organization in which it operates. A strategy should help drive alignment.

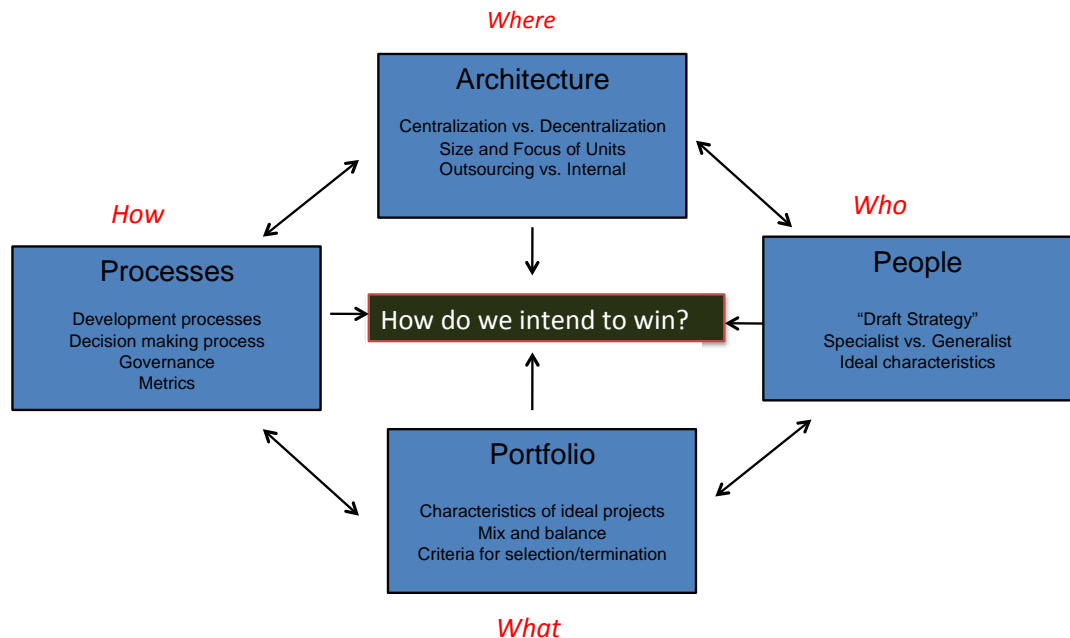
All strategies—whether for a game, a whole business, or a function like R&D—come down to certain “core hypotheses” about what it takes to win. For instance, in our simple example at the outset, the strategy “hit to Joe's backhand” is predicated on the assumption that Joe's backhand is his weaker stroke. Apple's strategy of providing easy to use, aesthetically pleasing, integrated

system is predicated on a core hypothesis that customers will be willing to pay a significantly higher price for products with these attributes. Underlying many R&D strategies are hypotheses or assumptions about technical or scientific approaches that are likely to be most fruitful. Apple has made a “bet” around the virtues of integrated hardware and software systems. Boeing is making a “bet” that new composite materials will offer superior flight performance in airframes. Different automobile companies are making different technical “bets” on different engine systems to achieve higher environmental performance (hybrids, fuel cells, all electric, diesel, etc.). Some pharmaceutical companies are “betting” heavily that genomics will lead to personalized medicine. There is often no way to “test” these hypotheses in advance. Thus, at some level, all strategies are “bets”. Experience executing a strategy provides “data” that may cause you to revise your core hypotheses.

### ***Elements of an R&D Strategy***

R&D strategies, like all strategies, must start with the devilishly simple question: “how do we intend to win?” The “game plan” for an R&D organization can be broken down into 4 strategic levers: architecture, processes, people, and portfolio. Together, decisions made in each of these categories constitute the R&D strategy (see Figure 1).

**Figure 1: Elements of R&D Strategy**



1. **Architecture** refers to the set of decisions around how R&D is structured both organizationally and geographically. This category includes decisions such as centralization vs. decentralization of R&D; the size, location, and focus of R&D units (e.g. focus by market? focus by technology?); whether R&D units report to business units or are autonomous; whether research is organizationally separated from development; and the degree to which R&D utilizes external resources and partnerships. There is no single best architecture for an R&D organization. For instance, a highly centralized R&D organization facilitates communication and integration across different functional groups; at the same time, centralization forfeits the benefits of having a geographically diversified "footprint" of R&D facilities located close to different global technology hotspots. The better approach depends on the organization's "core hypotheses" about what it takes to win. If it is betting on integration, then the centralized model is better. If it thinks tapping geographically diverse knowledge bases is the key to winning, then the decentralized model is a better route.
2. **Processes** are the formal and informal ways that R&D is carried out. This category includes choices about project management systems, the governance of projects (including the nature

of senior management reviews), the sequence and flow of critical project tasks, the timing of reviews, and the metrics and indicators used to track projects. Too often, certain kinds of development processes are pitched as “best practice” when, in fact, process design is very contingent on the overall R&D strategy. Consider the choice between a highly “structured” R&D process (with tightly specified procedures, review points, etc.) and a more “flexible” process. Which is better? Again, it depends on broader R&D goals and other choices. An R&D organization working on highly novel (and highly uncertain) technologies may need much more process flexibility so that it can have the latitude to explore and iterate. In contrast, where R&D must be tightly coordinated with other functions (like manufacturing), a more tightly specified process may be necessary to “keep everyone on the same page.”

3. **People** are obviously an enormously important aspect of an R&D system. Despite the growing use of sophisticated instrumentation, computer simulation, and laboratory automation, R&D is still a labor intensive process. Thus, choices about human resources-- such as the mix of generalists vs. specialists, technical backgrounds and training, work styles, career paths, lay off policies, etc.—have a significant impact on R&D performance. Again, there is no one best human resource strategy for R&D. Take for instance lay-off policies and career paths. Some companies implicitly promise their R&D workforce that they will have relatively steady employment and seek to attract people who will tend to stay at the company. Other companies are comfortable with a degree of “churn.” They do not expect people to stay along, but neither do they promise much job security. Which approach is better? That depends on the location of the R&D laboratories (an architectural choice). If one is located in a technology hot-spot (like say Silicon Valley or Boston), a high “churn” model may be perfectly reasonable (and, unavoidable). But, if the R&D labs are more geographically isolated, then it is much more important to promise some degree of job security to attract talent.
4. **Portfolio** refers to the desired resource allocation across different types of R&D projects and the criteria used to sort, prioritize, and select projects. The R&D portfolio should reflect the priorities of the R&D strategy. For instance, a pharmaceutical company that “intends to win” by discovering its own first-in-class drugs should have a very different portfolio allocation than a company that is trying to win by developing follow-on drugs in already established drug classes.

In evaluating an R&D strategy, it is important to ask a few basic questions. First, have we been absolutely clear about how we intend to win? Everyone should understand what the priorities are and what they mean for them. Second, are the choices we are making about architecture,

processes, people, and portfolio coherent? Are there any major conflicts between our policies? Third, do all our choices form an integrated “system” focused on the key priorities (how we intend to win)? Finally, because a strategy is a “hypothesis”, we need to evaluate our R&D strategy against performance data, and recognize when the time has come to reject our initial hypothesis, and change strategies.

### ***Application Example: The Case of Pharmaceuticals***

Facing intensifying competition, more demanding (and price sensitive) payers, patent expirations, and higher regulatory burdens, pharmaceutical companies have been in search of new R&D strategies to increase R&D productivity. Because of the high levels of uncertainty in pharmaceutical R&D, attrition rates and the timing of attrition dominate the effect of direct project costs in pharmaceutical R&D productivity. While managing projects effectively and efficiently is certainly important, better selection of projects and better decisions about which projects to advance have a much bigger impact on overall R&D costs (this is due to the fact that development costs escalate as projects progress). It is no wonder, then, that most efforts to improve R&D productivity have focused on the attrition problem. As shown in the examples below, there are multiple potential strategies for dealing with the attrition problem. The purpose of the examples below is **not** to highlight effective models for improving attrition management. Each of these models has strengths and weaknesses (trade-offs), and some have serious flaws. The purpose is to illustrate how core hypotheses about the underlying root causes of problems can have a profound influence on strategic choices. The examples also illustrate how organizations can be biased toward different strategic levers (architecture, process, people) in their R&D strategies.

#### *GlaxoSmithKline: Breaking R&D Up into Smaller Units<sup>1</sup>*

In January 2000, after the merger of Glaxo and Smithkline Beecham, the newly formed GlaxoSmithKline (GSK) restructured R&D around organizational focused therapeutic area units (cancer, neurology, etc.). These were initially called Centers of Excellence in Drug Discovery (CEDD). Each CEDD was responsible for the development of molecules in its designated therapeutic realm, from discovery through proof of concept. GSK continued to centralize early discovery (target identification and molecule discovery) and late stage development (Phase III clinical trials, registration, etc.). Each CEDD had its own leader and management team, and possessed most of the functions required to move a molecule from discovery to proof of concept. CEDDs were given complete autonomy over the management of their portfolios up through proof

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<sup>1</sup> This section based on Harvard Business School Case Study, “GlaxoSmithKline: Reorganizing Drug Discovery (A)”, #9-605-074.

of concept. They could select projects, make in-licensing decisions, determine project level funding and strategy, and decide which projects to advance and terminate. After proof of concept, the CEDD would present programs to a centralized governance committee (composed of senior management from R&D, business units, and corporate headquarters) for a “go/no-go” decision for full development. CEDD-heads were fully accountable for the performance of their therapeutic area portfolios. CEDDs were to be rewarded based on proof of concept successes advanced into full development.

The CEDDs model was predicated on the assumption that smaller, focused, autonomous, and more accountable units would make more efficient decisions regarding portfolio advancement. In essence, it was an attempt to create “biotech-like” organizations inside the larger corporate framework. The CEDDs model sees the core problem of R&D productivity as one of misaligned incentives, poor governance, and lack of focus. It is based on the principle of moving decision-making and control of projects closer to the organizational locus of the relevant information. The CEDDs model later evolved into what GSK called into Discovery Process Units (DPUs) that were smaller, and even more focused therapeutically than the original CEDDs. Using our framework above, the CEDDs/DPU model can be seen as a largely **architectural** intervention in the R&D strategy.

#### *Wyeth: A Metrics Based Approach<sup>2</sup>*

In 2001, just about the same time GSK initiated its change effort, Wyeth Pharmaceuticals was taking a very different approach to the R&D productivity/attrition problem. Like their counterparts at GSK, Wyeth senior management saw poor productivity as rooted in poor decision-making and misaligned incentives. However, instead of restructuring R&D around focused units, Wyeth chose to attack the problem through standardized development processes, metrics and performance targets, and incentive schemes aligned with those objectives. R&D remained centralized. Under the Wyeth strategy, the R&D organization received specific target objectives for molecules at each phase of the development cycle (e.g. 12 new clinical candidates per year). If this level of performance was achieved, the *entire* R&D organization was eligible for a financial bonus. If the target was not hit, no one got a bonus. Prior to the start of the initiative, Wyeth had been advancing an average of 3 molecules into clinical trials per year. The target objective for new clinical candidates was initially set at 12 per year. In addition to precise numerical targets (and tying financial rewards to them), the company deployed a structured development process with tightly specified milestones and reviews for every program. All projects would follow the same process.

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<sup>2</sup> This section based on Harvard Business School case study, “Wyeth Pharmaceuticals: Spurring Scientific Creativity with Metrics,” #9-607-008.



The Wyeth model was predicated on an assumption that it would be possible to make the drug R&D process more predictable by using a more “repeatable” process, by setting clear performance objectives, and by clarifying decision-making. Decisions regarding project advancement were governed centrally (unlike the CEDDs/DPU model). Performance bonuses were R&D wide, rather than tied to the performance of any given therapeutic area’s performance. Whereas the GSK CEDDs/DPU model was oriented around architecture, the Wyeth model was focused on *process*.

*Novartis: Betting on Science*<sup>3</sup>

In the early 2002, Novartis embarked on a major change in its R&D strategy. It opened a research laboratory in Cambridge, Massachusetts. This was not an unusual move. Other large pharmaceutical companies, including Merck and Pfizer, had opened research laboratories in the Cambridge area to be close to a thriving ecosystem of biotechnology companies and leading academic institutes. What was unusual is that the Swiss-based company decided to move its research headquarters from its home in Basel to Cambridge. This move was part of a broader strategy to make drug discovery at Novartis based on deep scientific understanding of underlying pathways and mechanisms of action. Being geographically close to biotechnology companies and institutions like the Broad, Harvard University, MIT, and the academic medical centers like Massachusetts General Hospital was viewed as essential to accessing and absorbing the relevant science. At the same time, the company recruited heavily from the Boston area academic science community (for instance, it hired Mark Fishman, a professor at Harvard Medical School and Mass General Hospital to head research). It hired over 1000 scientists to staff its Cambridge research laboratories.

Novartis pursued a decentralized research model. Discovery research was organized under the auspices of the Novartis Institutes of Biomedical Research (NIBR), headquartered in Boston, and under the leadership of Fishman. NIBR was not part of Novartis’s Pharmaceutical Division. Instead, it was an autonomous organization reporting directly to the Novartis CEO. Focal areas for research were determined by NIBR, and based on two basic criteria: large unmet medical need and an opportunity to develop deep biological insight. Analysis of market size and net present value were explicitly rejected as criteria for project selection at the research phase. One strategy for project selection was to identify pathways that might span multiple disease areas, and to initiate clinical trials in the disease with a small, well defined patient population. This could allow proof of mechanism in human subjects to be established more quickly. Once proof of

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<sup>3</sup> This section based on Harvard Business School case study, “Novartis AG: Science-Based Business,” #9-608-136.

mechanism was established, the research program could then be expanded to larger therapeutic applications. NBIR had responsibility for programs from discovery through proof of concept. Research was organized around both *platform groups* focused on specific research capabilities (e.g. proteomics, pathways, molecular biology, medicinal chemistry, etc.) and *therapeutic areas* focused on diseases (oncology, cardiovascular, neurological, etc.). NIBR had six research sites: Cambridge, MA; Basel, Switzerland; Horsham, UK; Emeryville, CA; East Hannover, NJ; and Shanghai, China. While each site had some degree of specialized focus, there was also a high degree of overlap. For instance, both Cambridge and Basel had biology and platform groups; all sites had discovery chemistry groups; discovery in several therapeutic areas (like oncology) were carried out at multiple sites.

The Novartis research strategy addressed architecture (separate NIBR), people (locate where the talent is), process (establish proof of mechanism in well-defined patient populations first), and portfolio (select projects based on scientific attributes). It was predicated on the “core hypothesis” that improved scientific knowledge of the disease pathway and mechanism would allow better decision-making about drug candidates to advance, and this would ultimately reduce later stage attrition rates. The strategy was, in essence, a bet on the science.

### **Summary**

Strategy is a systematic approach to solving a problem. Some problems have small stakes (e.g. how can I beat Joe at tennis this weekend?). Some problems are more important (e.g. what’s the right approach to finding a cure for Alzheimer?). In this note, we outlined a way to develop a systematic approach to addressing the problem: how can we make our R&D organization more competitive and effective? This involves consistent and coherent choices across architecture, processes, people, and portfolio. The pharmaceutical examples above give some flavor of how and why different companies pursued different strategies to essentially address the same problem. The differences were largely rooted in different “core hypotheses” (bets) on the underlying root cause of the problem. This suggests that the very first question to be answered in strategy development is: *what’s our shared understanding of the root cause of the problem we are trying to solve?*