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Editorial on Measurement of Outcome of R&D

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Editorial on Measurement of Outcome of R&D

Most developed economies of the world tend to credit their advanced educational institutions and R&D activities in science and technology for their success. The strong historically proven correlation between the R&D effort and economic development is hard to dispute. However, I believe that there are many more factors affecting the outcome of R&D. Successful R&D may not necessarily result in economic returns; on the other hand, many nations have prospered with little or no R&D effort. Nevertheless, I favor strong support of "R" in academia and "D" in industry and policies that can forge a strong link between the two as a formula for success. Academia is where human talent can be nurtured and cultivated to carry out cutting-edge research; industry can then utilize such talent to develop new technologies that are economically beneficial and socially valuable for the general well-being. Many attempts to do both in academia have met with limited, if any, success. As I have noted in several of my earlier editorials, the underlying objectives and timescales of research and development activities are widely different, which lead to limited success.

Science thrives on basic research; it is challenging, creative, and risky and can lead to game-changing technologies. The timescale required is typically long. It is carried out for its own sake with no limitations requiring specific applications and short-term return on funds invested. Academia is an ideal location to house such activity. The knowledge thus created is generally made available freely to whoever wants it and can digest it. High-level human talent is needed to utilize such basic advancements for engineering or technological applications. Applied or engineering R&D is motivated by real-life needs and opportunities; it is also constrained by shorter timescales and cost effectiveness. Science can propose new avenues that economics may deny. Not all basic research can be transformed into practical applications, but it is hard to predict which ones will be. Support of basic research is therefore expensive and perhaps more appropriate to the richer nations of the world.

R&D should ideally be taken as an investment in the future health of an industrial state. However, it is easier to consider it as an operating expense for accounting and tax purposes; this is what most nations do. Because R&D outcomes need a long gestation period, it is nearly impossible to demonstrate returns on such expenses on a quarterly or even annual basis. R&D projects in industry thus become susceptible to frequent terminations and new start-ups. This can, although not necessarily, lead to wasted financial and human resources as many projects are left incomplete and nonconclusive. R&D policy therefore needs to be driven by highly technical and creative teams who can appreciate the intricacies involved and not focus only on the bottom line in the short term. It is important to be cautious in embarking on new R&D projects and even more careful and responsible in terminating such projects.

One major problem that policy makers face is the inherently dynamic and local needs of R&D; yet this is affected by globalization. What competing nations or companies do does affect such policies. Clearly, a collaborative effort can save financial and human resources but often the proprietary nature of industrial R&D prohibits this option. On the other hand, major global issues affecting all nations, e.g., greenhouse gas emissions and climate change, can and have led to successful cooperation in R&D across geopolitical boundaries.

Closely linked to the economic returns on R&D is the issue of intellectual property (IP) rights. There is scholarly work that shows that IP can decrease innovation by limiting use and enhancement of patented technologies. When Switzerland and The Netherlands stopped patent protection for some years, studies showed a rise in innovations measured by some metrics. I think the problem is too nonlinear and longer term to arrive at such definitive conclusions. However, it is true that IP issues have slowed down industrial interactions with academia due to the protracted nature of resulting negotiations and associated additional costs. If academia wishes to produce research outcomes of industrial interest, a simpler cost-effective IP regimen is clearly needed. I am not aware of universities that prospered as a result of their technological discoveries. It is impossible to do without industry participation and support.

Finally, one must face the key issue of evaluation of R&D outcomes. This is truly a tough nut to crack. It is hard to place a dollar figure on the prior IP that human talent brings to any R&D project. The same is true of soft and hard knowledge that institutions bring to a given project. When a well-educated Ph.D. in science and technology moves from one country to another, several studies show that an equivalent of over a million dollars is transferred to the new (developed) economy. Yet, this is never taken into any R&D analysis. Without such human talent no R&D is possible. Also, without injection of financial resources, human talent cannot be utilized effectively; hence the need for both types of resources to succeed in

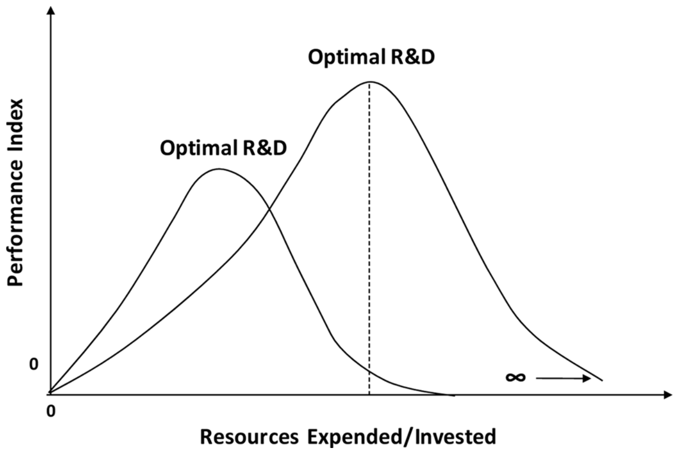

FIG. 1. Effect of resources on performance index.[1]

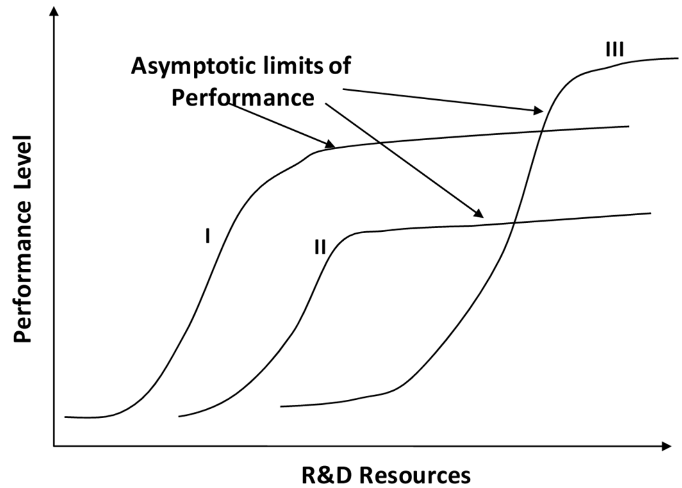

FIG. 2. Asymptotic behavior of performance index.[1]

the R&D game! My point is that the real cost of (or investment in) R&D is hard to quantify. As for the benefits and return on such investments, this is even harder to quantify. So, some simplistic cost/benefit analysis needs to be devised. In university research it is possible to use both objective criteria (citations, impact factor of journals, etc.) and subjective criteria (impact measured by visibility of researchers involved again following diverse concepts). Such criteria are often misused, even abused. Whereas academic research is driven and dictated by granting body decisions, industrial R&D is driven by market forces. The former is required to change to new directions depending upon changes in policy, which can be major enough that academics often veer into areas with little basic expertise. The outcome then is not consistent with the funding consumed. This is an issue that requires scholarly research.

In the past I have proposed a simplistic (relatively speaking) way of measuring research productivity. One can define a research productivity index (RPI) that simply divides measureable outcomes by the equivalent human and financial resources consumed. The numerator is hard to pinpoint accurately, however. In fact, even the denominator is hard to quantify correctly because much prior IP is excluded from it. If no resources are available or infinite resources are available for a given research project (or group), RPI will be zero. Thus, assuming that no R&D yields negative results, there ought to be an optimal level of R&D funding, which results in the best productivity in a given area. Some areas require massive injection of funds for research, so the optimum funding level will be at a higher level of resources. This is depicted in Fig. 1. Excessive funding can be detrimental to RPI because it is not funding that generated new ideas; funding allows the ideas to be tested and implemented. Because most research works are in serial and not in parallel fashion, a massively parallel system (good for computing!) is not healthy for research, especially in new areas. A sustainable level of R&D support should exist for a given area. Again, research in this area seems to be lacking.

The ubiquitous "S" curve describes well the performance of technological processes as a function of resources expended to develop it. Once the asymptotic limit of performance is approached, research managers should look for alternative pathways to spend their (scarce) R&D budget. No infusion of new resources will enhance performance of a "saturated" technology. Figure 2 shows schematically why one may pursue Technology III rather than the current one (I) or a new one (II) that has a lower asymptotic limit. Identifying the limiting states is where the vision and ability of research managers is put to a severe test. It is important to know when to start new R&D; it is equally important to know when to terminate such support.

There has been much scholarly research in recent years on the role of left brain and right brain thinking on innovation and hence on R&D. This will be the theme of a separate write-up in the future. Suffice it to say that R&D should be managed by and even carried out by people who utilize the "full" brain in their thinking. A purely analytical but intuition-challenged person is unlikely to support or carry out cutting-edge, innovative research.

I hope that some of the ideas presented here will lead to further debate and even some scholarly research on how the R&D game should be played. The rules for this game are yet to be written clearly.

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