Planning versus Free Choice in Scientific Research

Martin J. Beckmann[a]

[a]*Brown University and T U München*

Abstract

The potential benefits of centrally planning the topics of scientific research and who should be engaged in them are studied by means of a linear program. The efficiency conditions show that the planning goals could be achieved through self-motivated choices of scientists without direction by planners, when the “scientific worth” of problems is well-defined and known, provided the costs of research are essentially only the opportunity costs of the researchers. When substantial capital costs for apparatus arise, this self-motivation fails and some central control over benefit/cost becomes necessary. How this may be combined with the widest freedom of choice is discussed in the context of existing practices in sponsoring research.

JEL Classification: D61, D71

Keywords: scientific worth, scientific potential, prestige seeking, opportunity cost, big science, benefit / cost criterion

1. Introduction

It is the economist’s creed that whatever exists is a fit subject for economic analysis. The world is our oyster. Thus we have now not only an economics of agricultural and industrial production, of transportation and of household consumption but also one of political decision-making dubbed “public choice”, an economics of the arts, of marriage, and of crime and punishment and perhaps last, and possibly least, an economics of science.

A less charitable view of this is that those who can, do science and those who cannot, talk about science.

But consider this: pure science produces nothing of market value. You cannot sell a mathematical theorem or natural law. This poses some hard problems for the economy. In the absence of markets, how is basic research to be financed, how are researchers to be motivated, and above all how are resources, mainly scientific talent to be allocated, that is efficiently and purposefully utilized? Even as the economy grows, the scramble for research funding turns fiercer, since the demand for research monies by scientists keeps growing faster than the sources of supply, i.e. the rest of the economy.

The illustrious science historian Derek de Solla Price has discovered exponential growth in scientific journals, the number of scientists, and other indicators of scientific activity such that science has doubled every 15 years since the founding of the Royal Society in 1665, while GNP in the long run has doubled only every 25 years.

Advanced industrial societies spend about 2.9 percent of their GNP on science, a sum big enough to cause public concern. (See for instance the Congressman George E. Brown report to the Committee on Science, "Space and Technology, One Hundred Second Congress, Second Session", Serial L, July 1992).

In the face of actual or alleged waste, the most serious instance of which is perhaps the not uncommon one of multiple discoveries and priority fights, strong controls are sometimes called for. It is tempting to envisage the desirability of planning by eminent authority, by a committee of the leading figures in a given scientific field, whose superior knowledge encompasses first of all what are the greatest unsolved problems in their fields but also who would be best qualified to work on those.

2. Model

More is required than a mere listing and ranking of scientific problems, rather an indication of how much work q_j, j=1,…n should be devoted to problems j and how much of it x_{ij}=1, i=1,…m should be assigned to scientist or institute i.

To guide these assignments the planning committee must have an idea of the qualification, i.e. the expected productivity of i in subject j, a_{ij}. At the very least: who is qualified a_{ij}=1 and who is not a_{ij}=0.

$$\sum_i a_{ij} x_{ij} \geq q_j \qquad j=1,\ldots\ldots n \tag{1}$$

The planning authority should look for efficiency, i.e. it should minimize costs in the sense of total scientific manpower input.

$$\underset{x_{ij} \geq 0}{Min} \sum_{i,j} x_{ij} \tag{2}$$

while observing the capacity constraints of individual scientists or institutes i.

$$\sum_j x_{ij} \leq c_i \qquad i=1,\ldots\ldots m \tag{3}$$

For a single scientist we may set c_i =1

The scenario (1), (2), (3) is a linear program with similarities to the transportation and the assignment problems. When the c_i and q_j are integers the optimal solution x_{ij} can be chosen as integers, too. Computation by the Simplex method offers no difficulties. We are concerned only with economic aspects here.

Feasibility: If the goals q_j are too ambitious compared to the capacities c_i, for example when

$$a = \underset{i,j}{\text{Max}}\ a_{ij} \text{ and } \sum_j q_j > a \sum_i c_i$$

then no feasible solution x_{ij} satisfying (1) and (3) exists: the planning task has to be reformulated by ranking the targets (1) and treating them in their preferred order. (This is not difficult to formulate mathematically, but will not be pursued here). We assume an adequate supply of qualified scientists. In fact, the planning problem as formulated can also be seen as one of finding the best employment of available scientific talent.

Efficiency: The economic implications of optimality in the solution of linear programs (1), (2), (3) are given by the so-called Koopmans efficiency conditions (Koopmans, 1950). These are most easily found by taking derivatives of the Lagrangean:

Lagrangean $L = -\sum_{i,j} x_{ij} + \sum_{i} u_i \left(c_i - \sum_{j} x_{ij} \right) + \sum_{j} v_j \left(\sum_{i} a_{ij} x_{ij} - q_j \right)$

involving the dual variables or "efficient prices" u_i and v_j. An optimal solution $\hat{x}_{ij}$ of the linear programs (LP) (1), (2), (3) must then satisfy

$$\hat{x}_{ij} \begin{Bmatrix} = \\ \geq \end{Bmatrix} 0 \Leftrightarrow a_{ij} v_j \begin{Bmatrix} < \\ = \end{Bmatrix} 1 + u_i \qquad i=1,\ldots\ldots m\ ,\ j=1,\ldots\ldots n \tag{4}$$

in addition to (1) and (3). Moreover

$$u_i \begin{Bmatrix} = \\ \geq \end{Bmatrix} 0 \Leftrightarrow \sum_{i} \hat{x}_{ij} \begin{Bmatrix} < \\ = \end{Bmatrix} c_i \qquad i=1,\ldots\ldots m \tag{5a}$$

$$v_j \begin{Bmatrix} = \\ \geq \end{Bmatrix} 0 \Leftrightarrow \sum_{i} a_{ij} \hat{x}_{ij} \begin{Bmatrix} > \\ = \end{Bmatrix} q_j \qquad j=1,\ldots\ldots n \tag{5b}$$

In fact (1), (3), (4), (5), (6) are necessary and sufficient for the non-negative variables $x_{ij} > 0$ to constitute optimal solution of the LP (Koopmans, 1950).

3. Valuation

To understand the meaning and the significance of the efficiency conditions the proper interpretation of the efficiency prices u_i, v_j is vital. We propose to call v_j the scientific worth of problem j and u_i the scientific potential of scientist or institute i.

Rewrite (4)

$$\hat{x}_{ij} \begin{Bmatrix} = \\ \geq \end{Bmatrix} 0 \Leftrightarrow a_{ij} v_j \begin{Bmatrix} < \\ = \end{Bmatrix} 1 + u_i \qquad i=1,\ldots\ldots m\ ,\ j=1,\ldots\ldots n \tag{4}$$

and $1+u_i \geq a_{ij} v_j$, "=" when $x_{ij} > 0$

Or simply

$$1 + u_i = \max_j a_{ij} v_j \tag{4a}$$

Then (4a) states that the scientific potential of scientist i is realized when he / she chooses the problem in which he / she achieves results of great scientific worth. This scientific potential is the source of a scientist's reputation or prestige.

Another implication of (4) is

$$v_j \leq \frac{1 + u_i}{a_{ij}}$$

$$\text{or} \quad v_j = \min_i \frac{1 + u_i}{a_{ij}} \tag{4b}$$

The scientific potential $1 + u_i$ may also be viewed as the opportunity cost of using scientist i. $\frac{1 + u_i}{a_{ij}}$ is then the unit cost of a result in problem j produced by a scientist i.

Equation (4b) then states that scientific problem j should be worked on by scientists i who measured by their potential have the least costs.

The meaning of (4) is therefore that optimality is achieved by an assignment of problems to scientists that make scientists achieve their potential and produce results at least opportunity cost.

Its significance is that problem selection can be left to the self-motivation of scientists. This is so when planners set goals that imply a scientific worth v_j to problems, but quite generally when the scientific community recognizes that problems are not all equally worthwhile but differ in their importance, that is in their scientific worth.

Accepting the notion of scientific worth as something real and assuming an awareness of scientific worth by the scientific community, the objectives of a rational science policy can now be stated as follows:

$$\underset{x_{ij} \geq 0}{Max} \sum_{i,j} v_j a_{ij} x_{ij} \tag{6}$$

such that

$$\sum_j x_{ij} \leq c_i \tag{3}$$

This LP is always feasible and the efficiency conditions are as before

$$\hat{x}_{ij} \begin{Bmatrix} = \\ \geq \end{Bmatrix} 0 \Leftrightarrow a_{ij} v_j \begin{Bmatrix} < \\ = \end{Bmatrix} W_i \tag{4c}$$

where W is the dual variable of (3) corresponding to $1 + u_i = W_i$

The second efficiency condition is now

$$W_i \begin{Bmatrix} = \\ \geq \end{Bmatrix} 0 \Leftrightarrow \sum_j \hat{x}_{ij} \begin{Bmatrix} < \\ = \end{Bmatrix} c_i \tag{3a}$$

The upper alternative $W_i = 0$ can be excluded since (5) would not be achieved, given $a_{ij} > 0$ for some j and all i. No scientist (worth the name) is without some potential.

The case for freedom of choice in scientific research rests thus on equation (4), (4a): self-motivation in the pursuit of scientific prestige (Reif, 1961) will lead scientists to the problems on which they can contribute the most, measured in terms of scientific worth.

4. Costs

But what about costs? Can the LP (5) for an optimal use of available scientific talent truly represent a social welfare function since there is no reference here to cost? In fact since the optimum is to be achieved with given quantities *c* of scientific talent, the problem as formulated tacitly assumes no alternative use, i.e. zero opportunity costs for the use of their talent. Any other cost, particularly capital cost is neglected.

This scenario represents what De Solla Price has called Little Science, in contrast to the Big Science of modern laboratories, for instance in particle physics. (De Solla Price, 1961). Big Science calls for a different formulation of a rational science policy, e.g. maximizing scientific payoff within a total budget.

$$\sum_{i,j} k_{ij} x_{ij} \le B \tag{7}$$

where k is the (capital) cost of work by institute i on project j and B the budget.

At this point we must note that a marginal rate of scientific worth v_j is valid only within the technical limits of projects j.

$$\sum_{i} x_{ij} \le q_j \tag{1a}$$

Adding this to the institutes' capacities (3) and the budget constraint (7) the planning problem becomes

$$\underset{x_{ij} \ge 0}{Max} \sum_{i,j} v_j a_{ij} x_{ij} \tag{6}$$

such that

$$\sum_{j} x_{ij} \le c_i \tag{3}$$

$$\sum_{i,j} k_{ij} x_{ij} \le B \tag{7}$$

The modified LP is feasible and has the efficiency condition

$$\hat{x}_{ij} \begin{Bmatrix} = \\ \ge \end{Bmatrix} 0 \Leftrightarrow a_{ij} v_j - m k_{ij} \begin{Bmatrix} < \\ = \end{Bmatrix} W_i + z_j \tag{8}$$

where m is a Lagrange multiplier reflecting the budget and converting money cost into units of scientific worth and z_j the Lagrange multiplier of (1a).

Under self selection person or institute i would look for max a_{ij} v_j disregarding cost rather than focus on the benefit cost ratio in criterion (8a) below.

Efficiency condition (8) may in fact be written as a benefit/cost that, using $z_j \geq 0$.

$$x_{ij} \begin{Bmatrix} = \\ \geq \end{Bmatrix} 0 \Leftrightarrow \frac{v_j a_{ij} - w_i}{k_{ij}} \begin{Bmatrix} < \\ = \end{Bmatrix} m \tag{8a}$$

Big scientists like generals do not mind costs. The imposition of cost discipline thus cannot be left to self-control by scientists but calls for some central agency such as a National Science Foundation. In the US in some cases Congress itself has stopped projects for their excessive costs, e.g. the Superconducting Super Collider in 1992 (Weinberg, 1993, 283).

Call $v_j\, a_{ij} - mk_{ij}$ the "social worth" as distinguished from the purely scientific worth of the research by institute or principal investigator i on project j.

According to (8) credit for achieving something of social worth or "scientific glory" $a_{ij}\, v_j - mk_{ij}$ is shared by the sponsor of j and the investigator i, $z_j + w_i$.

Rewriting (8)

$$\underset{i}{Max}\; a_{ij}\, v_j - mk_{ij} - w_i = z_j \tag{8b}$$

$$\underset{j}{Max}\; a_{ij}\, v_j - mk_{ij} - z_j = w_i \tag{8c}$$

means that each side seeks to maximize its share of scientific glory given the other side's assignment of credit or its opportunity cost.

We postpone the question of how to recognize and implement these objectives to discuss first the actual practice of decision making by the sponsors of j and the investigators i. In (8a) the decision problem appears in terms of a benefit/cost problem but benefits are not simply scientific worth but scientific worth above the investigator's or institute's scientific opportunity costs.

5. Practice

At this point we should look at the problem selection and assignment as handled in current practice.

Here we must distinguish between research undertaken by internal decisions in university departments, inevitably of the Little Science type and research sponsored from the outside.

Unsponsored internal research involving as cost only the researcher's time off from teaching or administration is left to individual initiative subject only to the consent of the department chair, which is usually easy to obtain. Current practice for the support of scientific research in the US usually involves a three-stage decision process. Scientists choose a scientific problem and formulate it as a research project described in an application. The application is submitted to any one of a given set of foundations whose announced goal is the support of worthwhile scientific research "for the benefit of mankind". The foundation administrators then call on several consultants who are recognized expert scientists in the respective field, for peer review. This means an evaluation of both the scientific worth v_j of the project and the qualification a_{ij} of the applicant i. The foundation then performs the benefit / cost analysis, admitting projects with

$$v_j\, a_{ij} / k_{ij} \geq m$$

and rejecting the others. If there exists more than one foundation and their consultants are different, the applicant has a chance of trying again, taking advantage of comments received from the rejecting peers.

6. Conclusion

How Close does this Procedure Come to the Recommendations (8), (8a) Derived Above?

The difficulty of strictly applying (8) or (8a) lies in knowing the opportunity costs w_i. An exact calculation of these requires nothing less than solving the LP (6), (3) and (7) numerically.

An approximation to the w_i may be seen in the market worth of the principal investigator i that is his / her salary (plus other benefits which may be substantial). If the w_i are omitted all together, the net social worth of all projects are overstated, but so also would be the cut-off point m.

These distortions may actually be small compared to the uncertainties about scientific worth v_j particularly when comparisons are needed between various disciplines.

What then is the State of "Freedom of Choice" in Sponsored Research Today?

In sponsored research the benefit / cost criterion appears as a restriction (but not as an a priori elimination) of the researchers range of choice. Freedom of choice remains within these cost-imposed limits.

Of course the decision processes about what research is to be done and by whom are only as good as the estimates v_j of scientific worth and a_{ij} of scientists' productivities. Peers may not be unbiased when asked to judge the intentions and qualifications of others who may be potential competitors. Also the scientific community may be blind to truly innovative ideas and divided on issues of scientific worth. Still, reliance on peer review may be our best hope, preferable to arbitrary judgements by innocent or not so innocent administrators. Remember "the price of liberty is eternal vigilance" (J.S. Mill).

References

De Solla Price, D., 1986, *Little Science, Big Science, and Beyond*, Columbia University Press, New York, 4–14.

Koopmans, T.C., 1950, *Activity Analysis of Allocation and Production*, Chicago.

Mill, John Stuart, 1859, *On Liberty*.

Reif, F., 1961, "The Competitive World of the Pure Scientist," *Science New Series* 134 No. 3494 (December) 1957–62.

Weinberg, S., 1993, *Der Traum von der Einheit des Universums*, Bertelsmann, München.