

# On the balance between strategic-basic and applied agricultural research

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Strategic-basic research refers to basic research conducted in strategically selected areas expected to be of social benefit. Recent literature on the processes of basic research and its links to applied research has not been widely discussed in relation to agricultural research. This may have important implications for the question of the optimal allocation of research resources. The links are reviewed and combined into a framework for considering the allocation question. A numerical model suggests that only a small number of the model's parameters substantially affect the optimal level of basic research, and that it is not important to identify the optimal solution precisely, since the benefit function is extremely flat around the optimum.

## 1. Introduction

Alston *et al.* note that, 'it is relatively difficult to quantify the benefits arising from [basic] research' (1995, p. 8). Garrett-Jones *et al.* review several potential methods for doing so and conclude that, 'There is . . . no outstanding theoretical or methodological approach' (1995, p. xiv). Rosenberg is less understated: 'The difficulties in precisely identifying and measuring the benefits of basic research are hard to exaggerate' (1990, p. 168).

Indeed, it has been claimed that the task is hopeless, impossible — that because it is unpredictable and difficult to measure, basic research is fundamentally unplannable (Zimen 1995). On questions of detailed content and outcomes, it would be hard to disagree. On questions at a general level, such as the overall balance of funding between strategic-basic and applied research, however, some thought and analysis may contribute to better planning. The sums of money involved are certainly large enough to warrant an attempt. We should be encouraged by the example of farmers' management of their farms. They provide ample evidence that unpredictability in an economic

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system does not mean that management plans and decisions cannot or should not be made.

For most writers on economic or policy dimensions of agricultural research, the perceived difficulty of addressing basic or strategic research has been sufficient to steer them away from the subject. Most of the rapidly growing number of books on the evaluation and/or economics of agricultural research include very little, if anything, on the subject (e.g. Alston *et al.* 1995; Alston and Pardey 1996; Horton *et al.* 1993; Pardey *et al.* 1991; Ruttan and Pray 1987). The primary exception to this rule is the recent work by Huffman and Evenson (1993), which I will draw on later. There is also very little in the agricultural economics journals, with Frisvold's (1991) paper one of the few to dare.

Despite this general neglect, basic research is an extremely important topic. Huffman and Evenson (1993) report that in 1984, 26.9 per cent of all agricultural research by federal and state institutions in the United States was basic. Was this US\$490 million well spent? In particular, what is the optimal allocation of funds between basic and applied agricultural research, and is the actual allocation even close to that which would maximise the socio-economic benefits? This is the primary question addressed in this article.

The funding allocation is a particularly timely question given that, throughout the western world, science policies in general have tended to become more interventionist (Dasgupta and David 1994) and more concerned with accountability (Alston *et al.* 1995) and national benefit (Industry Commission 1995a; Garrett-Jones *et al.* 1995). The general trend certainly applies to agriculture, perhaps more than average, and there has been a rapid growth in the application of formal quantitative evaluations of agricultural research. This is critical for basic research if Nason (1981, p. 24) is right that, 'Project selection methodologies of a formal, quantitative nature reduce the tendency to perform basic research.'

Our grappling with the question of optimal balance will start with a discussion of the distinction between basic and applied research, followed by a brief review of existing literature on the potential contributions of strategic-basic research to socio-economic objectives, and its links to applied research and technology. This literature will form the basis for a simplified modelling framework describing the contributions and links. The framework will then be implemented as a numerical model, using hypothesised parameter values to explore and illustrate the potential behaviour of the system. Wide-ranging sensitivity analysis will suggest those parameters to which the answer to our primary question is most sensitive. It will also be used to explore the range of research allocations within which the socio-economic outcomes are close to the optimum.

**Table 1** Classifications of basic and applied research

Classification	Description
Pure-basic research	Experimental and theoretical work undertaken to acquire new knowledge without looking for <i>long-term</i> benefits other than advancement of knowledge.
Strategic-basic research	Experimental and theoretical work undertaken to acquire new knowledge directed to specific broad areas in the expectation of useful discoveries. It provides the broad base of knowledge necessary for the solution of recognised practical problems.
Applied research	Original work undertaken primarily to acquire new knowledge with a specific application in view. It is undertaken either to determine possible uses for findings of basic research or to determine new ways of achieving some specific and predetermined objectives.

Source: Australian Bureau of Statistics (1993).

## 2. Classifications of research

This article will primarily be concerned with strategic-basic research and applied research, as defined by the Australian Bureau of Statistics (1993) (table 1), rather than pure-basic research, which is generally conducted free of any thought of agriculture and for which the difficulties of planning are greater again. The terminologies used in the literature to describe different levels of research vary widely, but all can be related easily to the categories in table 1.

Given some of the statements in the literature, it is necessary to defend even the idea of distinguishing between basic and applied research. Rosenberg (1990) is most strident.

The distinction between basic research and applied research is highly artificial and arbitrary. The distinction is usually made to turn upon the motives, or goals, of the person performing the research. But that is often not a very useful, or illuminating distinction.

(*ibid.*, p. 169)

He further claims that 'the attempt to classify research into basic and applied categories is particularly hard to take seriously in some areas and disciplines, e.g., in the realms of health, medicine and agriculture' (*ibid.*, p. 170).

He supports the first claim by noting that important basic knowledge sometimes results from applied research, and that even where a scientist's motives are purely scientific, his or her employer's motives may be more mercantile. While conceding both of these points, they do not seem to invalidate a distinction being made on the basis of the scientists' motives,

and they seem irrelevant to the question of whether such a distinction is useful. Every institution that funds research must make a decision on the extent to which funding should be devoted to researchers who are constrained to achieving particular practical outcomes or to relatively unconstrained researchers in search of fundamental knowledge. As Rosenberg himself discusses, even strictly commercial organisations sometimes fund researchers with a basic orientation, and for sound commercial reasons. So the distinction is meaningful and, at least for some purposes, useful. The fact that applied research sometimes produces basic knowledge is something that may be considered when striking a balance between funding of basic and applied research, but it does not negate the fact that there is a balance to be struck.

Rosenberg's second claim, at least as far as it is directed at agriculture, is particularly hard to take seriously. Nobody who has worked with both basic and applied agricultural scientists could fail to be struck by the stark differences in their attitudes, motivations, priorities, methods, communication channels, and the natures of their products.

### 3. Strategic-basic research

In contrast to the agriculture-specific literature, the general literature on research policy has much to say about basic research. The early literature (Nelson 1959; Arrow 1962) focused on the public-good nature of basic research, and the justification that this provides for government funding of research. This has been very influential, and continues to be so in agricultural spheres. It is standard to argue that market failure, primarily caused by public good/free rider problems, is a necessary but not sufficient condition for government involvement in agricultural research to be justified (e.g. Alston *et al.* 1995; Alston and Pardey 1996). It is argued that these problems are likely to be substantially greater for basic than for applied research (e.g. Huffman and Evenson 1993).

More recently, attention has shifted. The largest set of recent literature on basic research is concerned with what is 'inside the black box' (Rosenberg 1982). It deals with the details, complexities, links and the human element of basic research (e.g. Brooks 1994; Dasgupta and David 1994; Pavitt 1991; Rosenberg 1990, 1991) and is particularly critical of the linear or 'pipeline' model on which much of the discussion about basic research is based. The pipeline model, first articulated by Francis Bacon in 1605, sees the impact of basic research flowing through a sequence of this type:

Basic research → Applied research → Technology → Economic productivity

The simplest model conceives of basic research as producing basic knowledge

that is available freely as a pure public good, being both *non-rival* or *indivisible* in consumption and *non-excludable* (see Randall 1987 for explanations of these terms). The Rosenberg school argues cogently that while there are spillovers from basic research, the real complexities of the system cause basic knowledge to depart substantially from a textbook pure public good. In this and other areas, there is much in these papers of relevance to the broad allocation question addressed here, but there appears to have been no attempt to pull together the various strands and ideas into a framework suitable for considering the question of resource allocation. The following sections of this article are an attempt to begin this task.

A third section of the literature focuses on estimating the impacts of basic research on economic welfare. These studies used statistical models to relate measures of research inputs (e.g. expenditure, number of scientists) or research outputs (e.g. number of published papers) to subsequent measures of economic activity or productivity. Only a minority of these studies distinguish between basic and applied research. Non-agricultural examples include Mansfield (1980), Griliches (1986) and Adams (1990), while in agriculture, there is Huffman and Evenson (1993). All these studies used versions of a pipeline model, and all found evidence that basic research contributes measurably to productivity. Given the simplicity of the estimated models and the substantial lags involved, this success is impressive and, perhaps, surprising. Indeed, the authors are very cautious in interpreting their results. Nevertheless the consistency and apparent reproducibility of the result from different data sets do tend to lend credibility and suggest that at a highly aggregated level, the pipeline model is a reasonable approximation.

A selection of Huffman and Evenson's (1993) results for US agricultural research is shown in table 2. Their category of 'pre-technology research' corresponds closely to strategic-basic research. The marginal internal rate of return of this category was found to be high, and substantially greater than those for applied research, extension or schooling.

**Table 2** Marginal internal rates of return for US public investment in sector-specific agricultural research, extension and education, 1950–82

	Crop sector	Livestock sector	Aggregate
All research	47.0	negative	40.6
Pre-technology research	62.2	83.2	73.5
Extension	40.1	negative	20.1
Farmers' schooling	22	19	40

Source: Huffman and Evenson (1993).

Note: Several sectors are omitted from this table.

**Table 3** Absolute and relative expenditure on basic biological science research funded by US federal and state institutions, 1984

	Crop sector	Livestock sector	Aggregate
Expenditure (\$ '000)	200,331	150,875	351,206
Share of total sector-specific research (%)	33.1	37.3	34.8

Source: Huffman and Evenson (1993).

There has been very little analysis or discussion of the appropriate allocation of resources between basic and applied research. On the basis of the results in table 2, Huffman and Evenson (1993) argued that, at least for livestock research, there has been a misallocation, with excessive emphasis on applied research and extension. It appears that the same may apply, to a lesser extent, to crop-specific research. The 1984 allocations are shown table 3. Given the marginal nature of the productivity results, the extent to which the expenditure on strategic-basic livestock research represents an underinvestment is not clear.

A fourth issue raised in the literature is that basic research is a long-term investment. There are substantial lags in all stages of the process: from basic research to applied research to development of technology to adoption of that technology. Statistical estimates for aggregate agricultural research in the United States and United Kingdom indicate that total lags of 30 years or more are normal (Pardey and Craig 1989; Chavas and Cox 1992; Schimmelpfennig and Thirtle 1994). A major implication of this is the importance of low interest rates (or other opportunity costs of capital) in encouraging research, particularly basic research.

A fifth issue is the conduct of basic research in the private sector. Most companies do no basic research (Rosenberg 1990), but amongst those firms with large research budgets, the proportion of basic research conducted was approximately 5 per cent in 1985 (Dasgupta and David 1994). In agriculture, Wilcke and Williamson (1977) found that the US private sector allocated only about 10 per cent of its research expenditures to basic research. In Australia, off-farm agribusiness firms are small relative to those in the United States, and the proportion of basic research they conduct is likely to be still lower. On the other hand, farmers in Australia have had majority control of the funding decisions in many of the rural research and development corporations that allocate farmer levies and matching government funds to research projects. These corporations have, in aggregate, allocated substantial amounts to pure-basic research (10 per cent) and strategic-basic research (close to 30 per cent) (Industry Commission 1995b). These allocations are not very different from the proportions

reported for Australian university-based agricultural science in 1992: 10 per cent and 36 per cent (Industry Commission 1995a). It is interesting to consider why these private investors are so much more willing to invest in basic research than others. Possible explanations include the following.

- To a significant extent, the free-rider problem resulting from the public-good nature of basic knowledge has been overcome by the imposition of a compulsory levy to share research costs.
- There may be a strong influence on the decision process by the minority of scientists on the panels.
- There may be a reduced incentive for commercially based decision-making resulting from the fact that the farmers on the panels have contributed only a tiny fraction of the funds. It is true that it is in the farmers' interests for the funded research to be commercially beneficial, but spending other people's money is not like spending your own.
- There may be differences in the definitions used for basic research.

Rosenberg (1990) notes that the fact that purely commercial, competitive firms fund *any* basic research reveals that its product is not a pure public good. If it were, firms would be able to rely entirely on basic research results produced in the public sector, or overseas, and their investment in it would be zero. Although his arguments are made in the context of private companies, it is clear that they apply equally well to a small country, such as Australia, facing the opportunity to draw on basic research conducted in large countries (Kay and Llewellyn Smith 1985). The motivations for private companies or small countries to invest in basic research will be further considered in the next two sections.

#### **4. Links to applied research and technology**

The pipeline model described briefly in the previous section incorporates a one-way link from basic research to applied research. All benefits of basic research are generated via this link, which feeds into technology and then social benefit. Some of the econometric studies of the impact of basic research on productivity have used this model with some explanatory success (e.g. Griliches 1986; Adams 1990). However, in reality, the links between basic research and social benefits are very complex (Turpin *et al.* 1996). There has been substantial criticism of the model for its failure to adequately represent the complexity of the system. Here we explore the links in more depth. The connections may be of considerable importance in considering the allocation of research resources. For example, Brooks (1994, p. 477) argues that:

Because of the many indirect as well as direct connections between science and technology, the research portfolio of potential social benefit is much broader and more diverse than would be suggested by looking only at the direct connections between science and technology.

To start with, we should acknowledge the argument that basic research offers more than direct utilitarian benefits. Garrett-Jones *et al.* (1995) propose that basic research contributes to national goals through its contributions to knowledge production, research training, international relations and intellectual culture. In these contributions, especially the last, non-financial values supplement the economic values from technology improvements that are normally the focus of policy discussion. The non-financial values may perhaps arise from impacts related to:

- ethics (e.g. protection of animal welfare);
- culture (e.g. protection of the rural lifestyle);
- aesthetics (e.g. protection of the rural landscape);
- equity (e.g. assisting relatively poor farmers);
- risk (e.g. improved reliability of crop yields);
- pure interest (e.g. understanding genetic adaptations of plants);
- patriotism (e.g. pride in a country's research achievements); and
- the environment (e.g. reducing the negative impacts of agriculture on habitats of threatened species).

In achieving some of these benefits, applied research may still be needed to realise the potential for benefits created by basic research. However, in others, such as pure interest or patriotism, the benefits may arise directly from basic research. The substantial long-term investments in astronomy bear testament to our willingness to fund at least some types of research for pure interest. However, I suspect that agricultural research, even if basic, is less endowed with this general-interest value.

Now consider the links that affect the relationships between basic research, applied research and public benefits. David *et al.* (1988, p. 69) emphasise the importance of these links:

Basic research interacts with applied research in a complex and iterative manner to increase the productivity of both basic and applied research. The development of links between the basic and applied research enterprises [is] critical to the productivity and economic payoffs of both activities.

Rosenberg (1990, p. 170) states that, 'When basic research in industry is isolated from the rest of the firm, whether organizationally or geographically, it is likely to become sterile and unproductive.'

Given this importance of close links, it is notable that among scientific disciplines, agricultural science has unusually close links between basic and applied research (Nairn and Noma 1985; Pavitt 1991). Perhaps this is why the empirically estimated returns to agricultural research are consistently so high. It is structured so as to provide and integrate the many elements needed to produce a complete and appropriate package of technology and related management information. For example, for a new crop species with system-wide impacts, there would be contributions from genetics, plant breeding, agronomy, weed science, extension, plant nutrition and pest management. The sensitivity of success to the failure of any of these elements can be high. An example is the legume crop lupins, which was partly adopted and then largely rejected in Western Australia during the 1970s (Marsh *et al.* 1994) due both to genetic limitations (especially poor disease resistance) and under-developed agronomic practices. Only when both of these limitations were overcome was the crop widely adopted. Thus, even if different researchers do not depend directly on each other for information, they may depend on each other indirectly for the realisation of economic benefits from their research.

## 5. A framework describing basic and applied research

Garrett-Jones *et al.* claim that a primary reason why existing methodologies are not suitable for valuing basic research is that they 'have not caught up with the new understanding of how basic research and technology are interacting' (1995, p. xiv). In this section, the specific interactions described in the recent literature are discussed and integrated with the traditionally understood pipeline model to produce a framework within which the resource allocation decision may productively be considered.

### 5.1 Basic research generates basic knowledge

This seems obvious enough, but it is legitimate to question whether the stock of basic knowledge can, in fact, be meaningfully measured in quantifiable terms. In supporting the proposition that it can, I would cite the success of Adams (1990) in specifying a variable representing increments to the stock of knowledge (based on counts of published articles) and relating it to manufacturing productivity in the United States. Adams discusses in detail the issues involved in selecting an empirical measure of this variable.

As a second example, consider Harwit's (1981) list of 43 (then) currently known principal phenomena that characterise the universe. Starting with stars, planets and novae, known since antiquity, there has been an accelerating increase in the stock of fundamental knowledge about the

universe, with relatively recent discoveries including quasars, masers, pulsars, superluminal sources and gamma-ray bursts. In some ways, this is stronger evidence than measures based on numbers of papers, since there is no guarantee that the papers actually contain new basic knowledge.

It is interesting that the usual economic assumption of diminishing marginal product does not appear to have applied to basic astronomical research. During the twentieth century, there has been a steadily increasing level of investment in the research, and a dramatically increasing rate of discovery of discrete new phenomena (Harwit 1981).

We have already mentioned the lags involved in the production of basic knowledge. It is obviously also subject to substantial risks (Anderson 1991). It is arguable whether the resource allocation decision should make allowance for risk aversion. Arrow and Lind (1970) argue that as long as a public investment is small relative to national income, its risks will normally be offset by uncorrelated returns from other investments. In this case, decisions about an investment can be based on the expected value of its economic return. Even within the portfolio of basic agricultural research projects, the outcomes, while risky, are likely to be substantially uncorrelated and, for a major research funding institution, the size of any one research project is small relative to the total. Thus, given the level of aggregation of this framework, the use of expected benefits as the objective is reasonable.

## **5.2 Applied research generates applied knowledge**

This is easier to conceive of, given the obvious measurability of outputs such as new crop species or varieties, improved production inputs and management systems.

## **5.3 Basic knowledge affects production of applied knowledge by applied research**

This is an accepted dogma of the whole debate, but there appears to have been almost no discussion of the form of the relationship. Frisvold (1991) makes no distinction between basic and applied knowledge, and assumes that basic and applied research are complements in the production of a stock of generic knowledge. To me, a more plausible conceptualisation, which will be used in the model that follows, is that for any given stock of basic knowledge, there is a maximum potential stock of applied knowledge that can be approached by conducting applied research. Basic knowledge increases the productivity of applied research by pushing out this frontier through the opening up of new potentials for applied research and

technology. It also may affect the rate at which applied knowledge approaches a given frontier by developing and sharing skills, methods and instruments (Pavitt 1987; Rosenberg 1991; Brooks 1994).

#### **5.4 Applied knowledge affects production of basic knowledge by basic research**

Unlike the relation shown in section 5.3, one would not expect discovery of basic knowledge to be absolutely constrained by the level of applied knowledge. Nevertheless, it is clear that applied knowledge and technology do affect the rate of discovery of basic knowledge. One mechanism is via the development of new technologies that can then be applied to basic research problems. An example is astronomy. Harwit (1981) observed that technological innovations arising from outside astronomy (or other basic sciences) have allowed some of the most important astronomical discoveries. Parallels in agriculture would include developments relating to computers and biotechnology. Another mechanism by which applied knowledge affects the discovery of basic knowledge is through transfer of new research ideas or perceived needs from applied to basic research.

Technological development indirectly stimulates basic research by attracting new financial resources into research areas shown to have practical implications. This has happened repeatedly for radical inventions such as the transistor, the laser, the computer, and nuclear fission power, where much of the science, even the most basic science, has followed rather than preceded the original conception of an invention (Brooks 1994).

#### **5.5 Basic research generates applied knowledge**

As well as affecting the production function for applied research, basic research may itself generate knowledge that is directly applicable. This spillover effect is most likely to operate through the commercial uptake of technological innovations developed in the course of basic research.

#### **5.6 Applied research generates basic knowledge**

Rosenberg (1990, p. 169) emphasises that, 'Fundamental breakthroughs often occur while dealing with very applied or practical problems', and gives several outstanding examples (e.g. Karl Jansky of Bell Labs, searching for the source of static in transatlantic telephone calls in the early 1930s, found that one source was 'star noise', a discovery that marked the birth of radio astronomy). According to Brooks (1994), during the conduct of applied research and development, many observations of potential importance to basic research are not documented or disseminated properly so that they can

be taken up by basic researchers. This highlights the importance of close and active links between basic and applied scientists.

### **5.7 Foreign knowledge contributes to local knowledge**

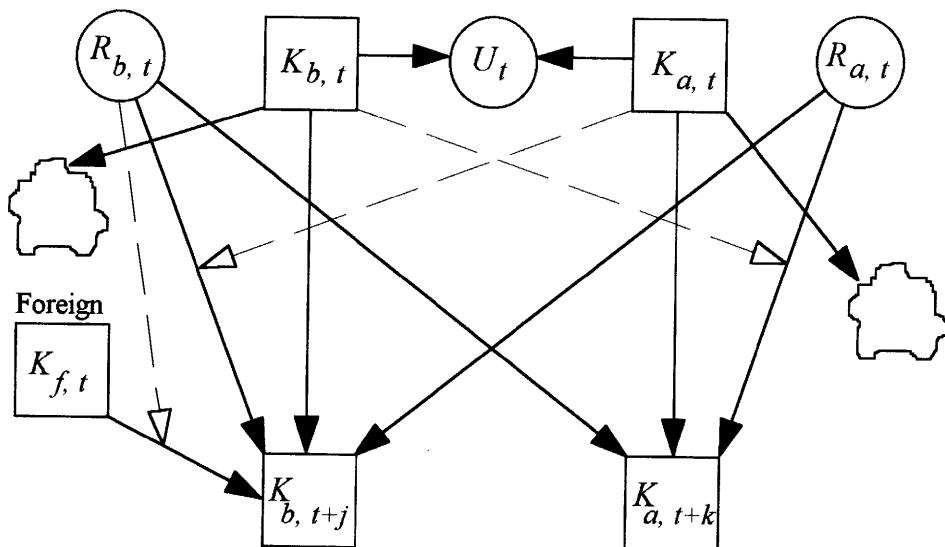
We noted earlier that if knowledge were a pure public good, a small country could free-ride on the basic research of other countries. The reality, however, is that knowledge is not 'on the shelf' and freely available to all. In particular, 'it frequently requires a substantial research capability to understand, interpret and to appraise knowledge that has been *placed* upon the shelf — whether basic or applied' (Rosenberg 1990, p. 171). Thus the price of 'free riding' may be high — it requires the funding of high quality researchers. Perhaps they must also be allowed (and funded) to conduct research, especially basic research, in order to have access to the international information networks, and in order for them to be willing to remain in the country.

In agriculture, given the great diversity of farming systems, physical environments, and economic policies between countries, there seems to be limited potential for importation of applied knowledge, apart from that embodied in specific technologies such as machinery. Animal and plant varieties and knowledge of their production functions resulting from applied research can be highly region-specific, as evidenced by the very existence of separate agricultural research institutions in different countries and different regions of the same country.

### **5.8 Knowledge dissipates or becomes obsolete**

It has been observed that in the absence of continuing applied research, agricultural productivity in a country is not maintained, but falls away (Alston and Pardey 1996). Causes of these falls include the development of resistance to control practices for insect pests, diseases and weeds. Recent evidence suggests that around 35 per cent of agricultural research in the United States might be classified as 'maintenance research' (Adusei and Norton 1990). Since the total stock of knowledge is much greater than the annual production of knowledge, the rate of annual depreciation of agricultural knowledge would be substantially less than 35 per cent, assuming that the 35 per cent allocation is approximately sufficient to offset the obsolescence.

Basic knowledge also may become obsolete. In his model of the impact of basic research on productivity growth in US manufacturing industries, Adams (1990) found a 13 per cent rate of obsolescence to be best fitting in the regressions.



**Figure 1** Links and dynamics of basic and applied research

### 5.9 Knowledge contributes to social welfare

As well as the obvious improvement in agricultural productivity generated by applied knowledge, a range of other social benefits of research was outlined in the previous section. In particular, note the possibility that benefits may arise directly out of basic knowledge, in addition to its contribution to the production of applied knowledge.

In figure 1 all these links between basic and applied research and knowledge have been brought together into a system. Arrows with dashed lines indicate that a variable interacts with another relationship in the system. Symbols used in figure 1 and in the following discussion are shown in table 4.

For simplicity, the figure illustrates the special case where  $j = n$  and  $k = m$ . The irregular-shaped elements represent 'sinks', which are the destinations for obsolete knowledge. The complexity of this system is a substantial departure from the standard pipeline model.

### 6. Numerical model

In this section, a numerical version of the framework is presented and used to illustrate the behaviour of the resource allocation problem. The first task is to select suitable functions and parameters. Selections are based on

**Table 4** Glossary of symbols

Symbol	Meaning
$R_b$	Expenditure on basic research
$R_a$	Expenditure on applied research
$K_b$	Stock of basic knowledge known locally
$K_a$	Stock of local applied knowledge
$K_f$	Stock of basic knowledge known externally ('foreign')
$\beta_1, \beta_2$	Parameters of the functional relationship between basic research and basic knowledge
$\alpha_1, \alpha_2$	Parameters of the functional relationship between applied research and applied knowledge
$M_a$	Maximum potential level of applied knowledge
$S_b$	Spillover parameter describing the production of basic knowledge per unit of applied research
$S_a$	Spillover parameter describing the production of applied knowledge per unit of basic research
$\phi_b$	Rate of obsolescence of basic research
$\phi_a$	Rate of obsolescence of applied research
$U$	Utility or social welfare
$j, k$	Lags between basic research expenditure and production of basic knowledge ( $j$ ) or applied knowledge ( $k$ )
$m, n$	Lags between applied research expenditure and production of applied knowledge ( $m$ ) or basic knowledge ( $n$ )
$P_b, P_a$	Social welfare per unit of basic or applied research
$r$	Discount rate
$t$	Time period

subjective judgment and assumption, drawing on information in the literature where possible.

Consider first the stock of basic knowledge at time  $t$ . We see from figure 1 that it depends on the stock of basic knowledge at time  $t-1$ , the rate of obsolescence of basic knowledge, the levels of basic research in previous periods, the level of applied knowledge (via its impact on the productivity of basic research) and the level of applied research. A possible function is

$$K_{bt} = K_{bt-1} + \beta_1 R_{bt-j} + S_b R_{at-n} + \beta_2 R_{bt-j} K_{at-j} - \phi_b K_{bt-1} \quad (1)$$

This excludes foreign basic knowledge as a direct contributor to local basic knowledge. This might be justified on the basis of arguments that foreign knowledge has to be filtered through local basic research to become locally available. The selected function for the stock of applied research is:

$$K_{at} = K_{at-1} + f(R_{at-k}) + S_a R_{bt-m} - \phi_a K_{at-1} \quad (2)$$

where

$$f(R_{at}) = (M_{at} - R_{at}).(1 - \exp(-\alpha_1 R_{at})) \quad (3)$$

and

$$M_{at} = \alpha_2 K_{bt} \quad (4)$$

Functions (1) and (2) reflect differences between basic and applied knowledge discussed earlier. Applied knowledge is conceptualised as being constrained by a maximum level depending on the stock of basic knowledge, whereas basic knowledge is not similarly constrained by applied knowledge. This means that in any period, applied research has a diminishing marginal product of applied knowledge, whereas basic research has a constant marginal product of basic knowledge. It also means that the impact of basic research on production of applied knowledge includes substantial lags. An increase in  $M_a$  in a period continues to have an impact on production of  $K_a$  in all future periods, although the effect eventually diminishes to negligible size.

The social welfare function is

$$U_t = P_{bt} K_{bt} + P_{at} K_{at} \quad (5)$$

The first term allows for the direct benefits of basic knowledge, due to factors such as pure interest, or patriotism. The second term includes the more usually measured benefits of agricultural research, primarily increased productivity. There is no attempt here to account specifically for the range of special circumstances affecting the measurement and size of research benefits, as outlined in detail by Alston *et al.* (1995). Rather, it is assumed that within each of the highly aggregated research categories, there are outcomes with a wide range of levels of benefit, reflected in probability distributions of benefit per unit of knowledge produced. The  $P$  parameters in equation 5 represent the means of these distributions.

The model is solved for 100 years in five-year periods with the objective of maximising the net present value of investment in basic and applied research. The starting values are assumed to apply to each of the two prior periods. The decision variable is the proportion of basic research. To simplify the problem, it is assumed that the institution wishes to commit itself to a particular allocation for the duration of the decision period.

Table 5 shows the selected units of measurement, starting values and parameter values included in the model. These are based on a hypothetical example of a large research institution with a budget of \$100 million per year.

Differing low and high values are shown if these were used in the later sensitivity analysis. The following considerations influenced the selection of parameters.

**Table 5** Units of measurement, parameter values and starting values for the numerical model

Parameter or variable	Unit of measurement	Low value	High value
$R_b^*$	\$m of expenditure	30	30
$R_a^*$	\$m of expenditure	70	70
$K_b^*$	\$m of potential applied benefit	1,500	1,500
$K_a^*$	\$m of applied benefit	350	350
$\beta_1$		5	10
$\beta_2$		0	0.001
$\alpha_1$		0.001	0.002
$\alpha_2$		1	1
$S_b$		0	0.1
$S_a$		0	0.1
$\phi_b$	Proportion	0.16	0.3
$\phi_a$	Proportion	0.16	0.3
$j$	Years	10	10
$k$	Years	10	10
$m$	Years	10	10
$n$	Years	10	10
$P_b$	Utils	0	0.05
$P_a$	Utils	1	1
$r$	%/year	5	10

Note: \* Values given for these variables are starting values, rather than parameter values.

- Starting values of  $K_b$  and  $K_a$  were set at levels corresponding to equilibrium values for one realistic scenario.
- The  $\alpha$ ,  $\beta$  and  $S$  parameters were calibrated subjectively by various means, with a range included to assess their sensitivity.
- The  $\phi$  parameters were based on evidence in the literature that between 35 and 70 per cent of agricultural research is allocated to 'maintenance'. Assuming that these levels are adequate to exactly offset obsolescence and that annual research generates 10 per cent of the total stock of knowledge, the obsolescence rate is 3.5 or 7.0 per cent annually, corresponding to 16 or 30 per cent over five years.
- The lag parameters of 10 years are not inconsistent with evidence for lags of over 30 years since (a) the basic and applied lags are cumulative and (b) they apply only to the *first* impact. Because of the model structure, there is an ongoing impact in subsequent periods for both basic and applied research.
- The discount rates are 5 and 10 per cent annually.

## 7. Results and discussion

The sensitivity analysis generated 512 ( $= 2^9$ ) solutions. Table 6 shows a selection that illustrates the impacts of the  $S$  and  $\phi$  parameters. Table 7

**Table 6** Optimal percentage of research budget to allocate to strategic-basic research ( $\alpha_1 = 0.002$ ,  $\beta_1 = 10$ ,  $\beta_2 = 0.001$ ,  $P_b = 0.05$ )

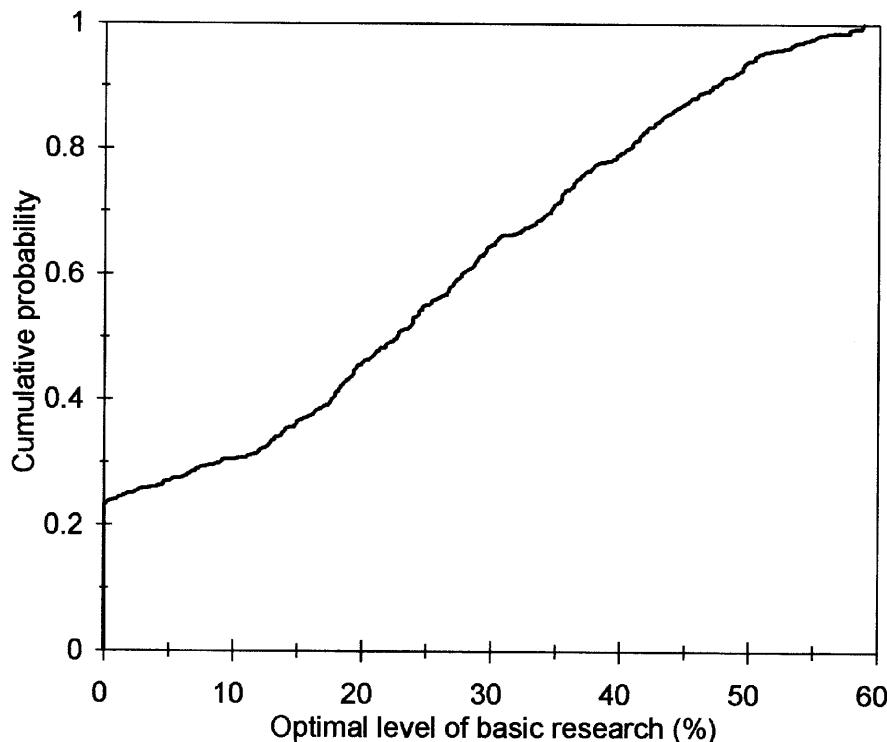
		$S_b$	0	0	0.1	0.1
		$S_a$	0	0.1	0	0.1
$r$	$\phi_b$	$\phi_a$				
0.05	0.16	0.16	36	43	35	43
		0.3	35	43	35	42
	0.3	0.16	49	56	48	55
		0.3	47	54	46	53
0.10	0.16	0.16	19	27	18	26
		0.3	16	24	15	23
	0.3	0.16	37	45	36	44
		0.3	33	41	32	40

**Table 7** Optimal percentage of research budget to allocate to strategic-basic research ( $S_a = 0.1$ ,  $S_b = 0.1$ ,  $\phi_a = 0.3$ ,  $\phi_b = 0.3$ )

		$P_b$	0	0	0.05	0.05
		$\alpha_1$	0.001	0.002	0.001	0.002
$r$	$\beta_1$	$\beta_2$				
0.05	5	0	22	34	47	47
		0.001	23	35	48	48
	10	0	32	40	58	53
		0.001	33	40	58	53
0.10	5	0	0	2	24	24
		0.001	0	5	26	26
	10	0	6	18	48	40
		0.001	7	19	48	40

shows the impacts of  $\alpha_1$ ,  $\beta_1$ ,  $\beta_2$  and  $P_b$ . Both tables include results for two different discount rates.

In most of these scenarios, the optimal level of basic research is substantial. In most of the reported cases, it is over 20 per cent. However, the results are quite sensitive to a number of the parameters; the reported results range from zero to 58 per cent. Over the full range of 512 scenarios modelled, the range is from zero to 59 per cent. It is certainly not possible to conclude that the optimal level is robust within a narrow range. Figure 2 shows the cumulative distribution of the optimal  $R_b$ , assuming that each of the 512 scenarios is equally probable. It shows that 23 per cent of the scenarios have an optimal level of zero basic research while 21 per cent have an optimum above 40 per cent. The mean and median of this distribution are both 23 per cent.



**Figure 2** Cumulative probability distribution of the optimal level of basic research

Tables 6 and 7 illustrate the relative sensitivity of results to changes in different parameters. Given the complex and non-linear model structure, it is not surprising that the impact of a given parameter change depends on the levels of other parameters. To investigate the overall sensitivity of results to each parameter, table 8 shows the average impact over all scenarios. The figures shown are the average difference in  $R_b$  between high and low parameter values for all 256 combinations of the other eight parameters.

As a result of the long lags represented, the discount rate is found to be the factor of highest sensitivity. There are three other parameters on which the results are most critically dependent:  $P_b$  (the intrinsic value of basic knowledge),  $\phi_b$  (the rate of obsolescence of basic knowledge) and  $\beta_1$  (the increase in potential applied knowledge per unit of basic research). The high sensitivity of results to  $P_b$  highlights the importance of pursuing the debate over the extent to which basic research yields benefits other than via its support of applied research. The dramatic difference in sensitivity of results between  $\phi_b$  and  $\phi_a$  is interesting, especially since the two parameters have been given the same values and enter the model in similar ways. The results

**Table 8** Average impact of parameter change on optimal funding allocation to strategic-basic research

Parameter	Average impact
$r$ (Discount rate)	-18.3
$P_b$ (Social welfare per unit of basic research)	18.0
$\phi_b$ (Rate of obsolescence of basic research)	16.8
$\beta_1$ (Parameter of equation 1)	11.4
$S_a$ (Spillover parameter, equation 2)	5.5
$\alpha_1$ (Parameter of equation 3)	3.3
$\phi_a$ (Rate of obsolescence of applied research)	-2.8
$S_b$ (Spillover parameter, equation 1)	-1.2
$\beta_2$ (Parameter of equation 1)	0.9

suggest that it is much more important to obtain accurate information about obsolescence of basic knowledge than of applied knowledge. The other parameters have relatively small impacts on results, the largest of them being for the spillover parameter  $S_a$ , representing the direct production of applied knowledge from basic research.

Finally, consider the sensitivity of expected benefits to the level of basic research. Although the optimal level of basic research changes in different circumstances, it is interesting to find that this does not necessarily imply that it is economically important to accurately identify the optimal level. Table 9 shows the expected value of the objective function for a stochastic version of the model incorporating the 512 scenarios already considered as equally probable states of nature.

The optimal level is 29 per cent (which differs from the average of the individual optimal levels for each scenario, 23 per cent). However, typically

**Table 9** Expected present value of gross benefits of research

Basic research (%)	E(PVB) (\$ million)	Difference from optimum (%)
0	409	7.9
10	429	3.5
20	441	0.8
29*	445	0.0
30	445	0.0
40	440	1.1
50	425	4.3
60	402	9.7

Note: \* Optimum.

for a complex system with many interactions, the expected benefits are very insensitive to the level of the decision variable. In general, it appears that at least in this model, it is clearly better to have some basic research than to have none, but that the range of levels of optimal or nearly optimal profit is wide.

### 7.1 Implications of the numerical results

It is not claimed that the numerical results presented above are empirically accurate. However, they do illustrate a number of principles which are likely to be relevant in the real world. Some of these are already noted above, but others are amplified here.

It is notable that, apart from the discount rate, the parameters that most influence the decision all relate to aspects of basic research: the impact of basic research on the productivity of applied research, the intrinsic value of basic knowledge and the rate of obsolescence of basic knowledge. If this result applies more generally, which is plausible, then the weakness of our current knowledge about the parameters and relationships of the basic research process may be cause for concern. On the other hand, the results suggest that it is not important to identify the optimal solution very precisely, since the benefit function is flat for a wide region around the optimum.

On the question of whether it is important to represent the full complexities of the research system, the model results suggest that the links emphasised in recent literature on basic and applied research are among the less important elements in determining the optimal level of investment in basic research. At least for the purposes of the allocation decision, understanding and quantifying these links may not be a high priority, although cultivating the links may still be important for improving the productivity of the research system.

## 8. Conclusion

Recent literature on basic research emphasises the links, feedbacks and interactions in the basic research/applied research system. The older 'linear pipeline' conceptual model of research is vigorously discredited. However, in considering resource allocations to basic research, it remains necessary to consider the relationship between basic research and applied outcomes. Studying this relationship is not tacit support for the simple pipeline model.

While the links may previously have been under-recognised, it does not necessarily follow that they must be relatively important in the question of resource allocation between basic and applied research. For one plausibly

structured and parameterised example presented here, they are not, at least not individually.

However, of the elements identified earlier, there are several that would combine to tend to encourage a diversified portfolio (independent of risk considerations), as shown particularly in sections 5.3, 5.4, 5.5, 5.6 and 5.9. Whatever the true forms of the relationships represented in equations 1 to 5, the payoff function along the continuum of allocations between basic and applied research is likely to have a plateau, in common with other complex systems involving interactions and feedbacks. If this is accepted, it implies that the precise allocation is not important, as long as it is sufficiently diversified. Rather than attempting to refine the allocations, energy and resources may be more productively focused on ways to improve links within the research system.

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