10

A Model of Agricultural Research[1]

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There is substantial evidence to support the notion that the rate and direction of agricultural research are influenced by economic circumstances.[2] The issues involved in this proposition are usually discussed in terms of "demand-oriented research" or "biased technological change" and focus on the payoffs to potential innovations.[3] However, as is so often the case, equilibrium is determined by both demand and supply. The basic building block of the theory of supply is the theory of production. The purpose of this chapter is to suggest a formal model of research, particularly of applied research, in the hope that this model will contribute a theory of the production of knowledge and help in understanding the supply side of the process of technical change.

The most important difference between the production of knowledge and the production of tangible goods is the strong element of uncertainty associated with the outcome of research work, and it is on this element that the present model focuses. The model is inspired by Stigler's work on the economics of information and is similar in some aspects to Nelson's treatment of research and development.[4]

The discussion starts with a presentation of an actual example. Applied research is modeled as a search in a distribution of unknown outcomes. One section discusses the properties of the model, and another is devoted to the effect of advancement in basic knowledge on applied research.[5] The last section draws some implications for the problems of the international agricul-

tural research centers and highlights a severe shortcoming in our understanding of the operation of research systems, particularly publicly supported systems.

The Development of Modern Sugarcane Varieties

To create the model, it will be useful to open our discussion with a short history of sugarcane variety development (see also chapter 8).

Four stages can be identified in the history of sugarcane breeding. First, sexual reproduction of the cane plant was not known, and as a result, improvements in the plant were very slow to come, based on occasional and rare cases of "natural" sexual reproduction. The second stage can be dated from 1887, when it was discovered that proper light and temperature conditions can induce flowering and thus sexual reproduction. Crossings were then made and offspring observed, and seedlings with superior potentials were selected and propagated vegetatively. At this stage, crossing was random. The major innovation of the third stage was the directed crossing of selected parents, aimed at specifically influencing characteristics of offspring. At this stage, wild, disease-resistant cane varieties were introduced into the breeding program. The fourth stage marks the modern period in sugarcane breeding wherein most of the effort has gone into the development of species suited to specific local climate and technology conditions.

Starting with the second, each stage was characterized by an innovation in the method of research. This innovation augmented significantly the productivity of research, which in turn encouraged the establishment of new experiment stations and the development of new varieties. After a while, however, returns to research began visibly to diminish, as the potentials revealed by the innovation were gradually exhausted.

In statistical terms, each crossing is a random drawing from the population of all genetically possible types. This selection is a process of search, by drawing, in a population of outcomes. Figure 10-1 depicts the four stages of sugarcane development. In the first stage virtually all the observations – cane plants in the field – were concentrated around one value with very little probability of discovering different types. In Stage 2 the *sample variance* was increased tremendously; both inferior and superior types were observed among the seedlings, and the best were selected. By identifying parents, search in the third stage was limited mostly to one portion of the population. That portion is in itself a new population centered around a higher mean than those of the original Stage 1 and Stage 2 populations. Similarly, Stage 4 marks a shift to a new, different population.

The selection depicted in Figure 10-1 is unidirectional, aimed, say, at achiev-

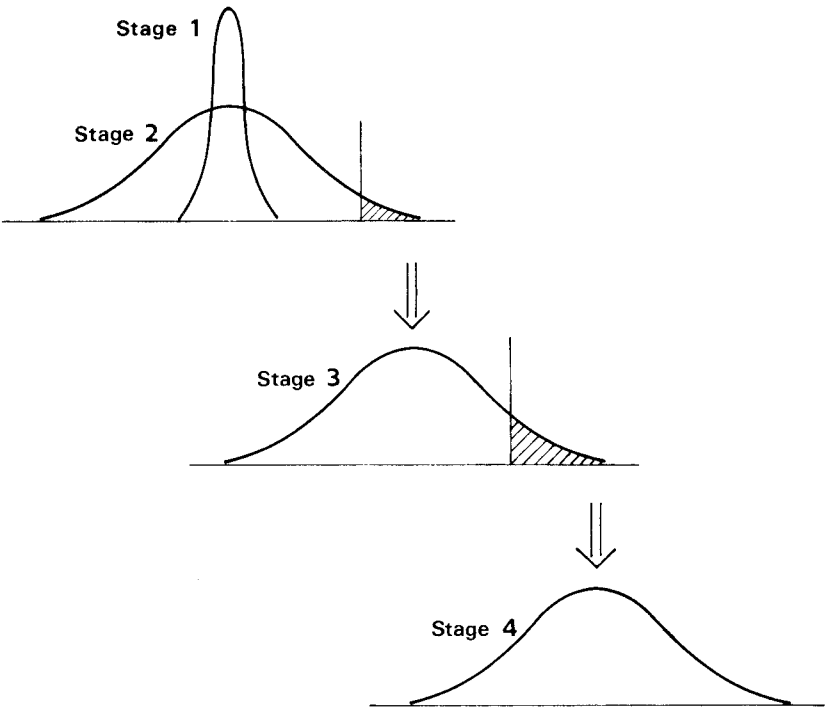

Figure 10-1. Stages in the development of sugarcane varieties.

ing higher yields. In reality, selection processes usually cover more than one characteristic of the organism. For simplicity, at this stage, the model to be developed below is also limited to unidirectional selection processes.

For concreteness, our discussion will be couched in terms of genetic-selection application and yields, but the model is of wider application. Research in other areas, such as organic chemistry, pharmacology, and the development of plant protection compounds, is technically a search and selection process, similar to breeding processes.

The Model

Imagine a scientist (or a scientific team) working to improve technology to increase the yield of a crop. To simplify, let us assume that income is proportional to yield and that the objective of the scientist is to maximize income of the whole system – research and production. Here, of course, is hidden one

of the crucial issues of applied research: What signals does the research system get from its "customers," and what dictates its response? We simply assume that the scientist is aware of the economic implications of his work and acts to maximize net benefits. The scientist faces a given distribution of genetic types, which he searches to find the best. An experiment is, in our formulation, a sample drawn from the distribution of unknown outcomes. In each period the scientist conducts such an experiment, that is, he draws one sample. The genetic types obtained in the sample are ordered by their yield level, the highest yield regarded as the outcome of that experiment. If the outcome of the experiment is better than the currently practiced technology (variety), the newly discovered technology will replace the old and yield (income) will rise. If not, the current variety is maintained. Experimentation may then continue.

The process is illustrated with the exponential distribution which can be taken to approximate the right-hand tail of the normal distribution. The exponential distribution is $f(x) = \lambda e^{-\lambda(x-\theta)}$ and it can be shown that for large samples the distributions of the maxima (which is what we are interested in here) converge to the same distribution for a large class of original distributions including the normal and the exponential.[6] In Figure 10-2 yield is measured along the horizontal axis, cumulative probability and probability density along the vertical axes. The experiment depicted in the figure is composed of a sample of three drawings from the random exponential distributions – with yields x_1, x_2, x_3. In the figure x_3 exceeds the current yield level, marked y; therefore a variety replacement will take place, yield increment being $\triangle y$ – the difference $x_3 - y$. Had the yield of the best variety in the experiment been smaller than y, the use of the current variety (with yield y) would have continued.

Since the actual outcome of the experiment is not known in advance, the value of the *expected* contribution, $E(\triangle y)$, for a given distribution of potential outcomes depends on the following two magnitudes.

1. The extent of experimentation, that is, the number of observations. The larger this number, the higher the probability of finding x values of larger magnitude. The probabilities, the density functions for the highest value in a sample, are depicted for five values of n in Figure 10-3.

2. The value of y – the current yield level. The higher the current level of technology, the less probable it is that an improvement will be discovered.

The economic value of the expected technological improvement is the present value of its future contributions. For simplicity of exposition we shall regard it here as $1/r\,[E(\triangle y)]$. This will be the correct contribution if experimentation is to take place only once; if it continues in later periods, an improvement today "spoils" the chances of improving technology tomorrow. This

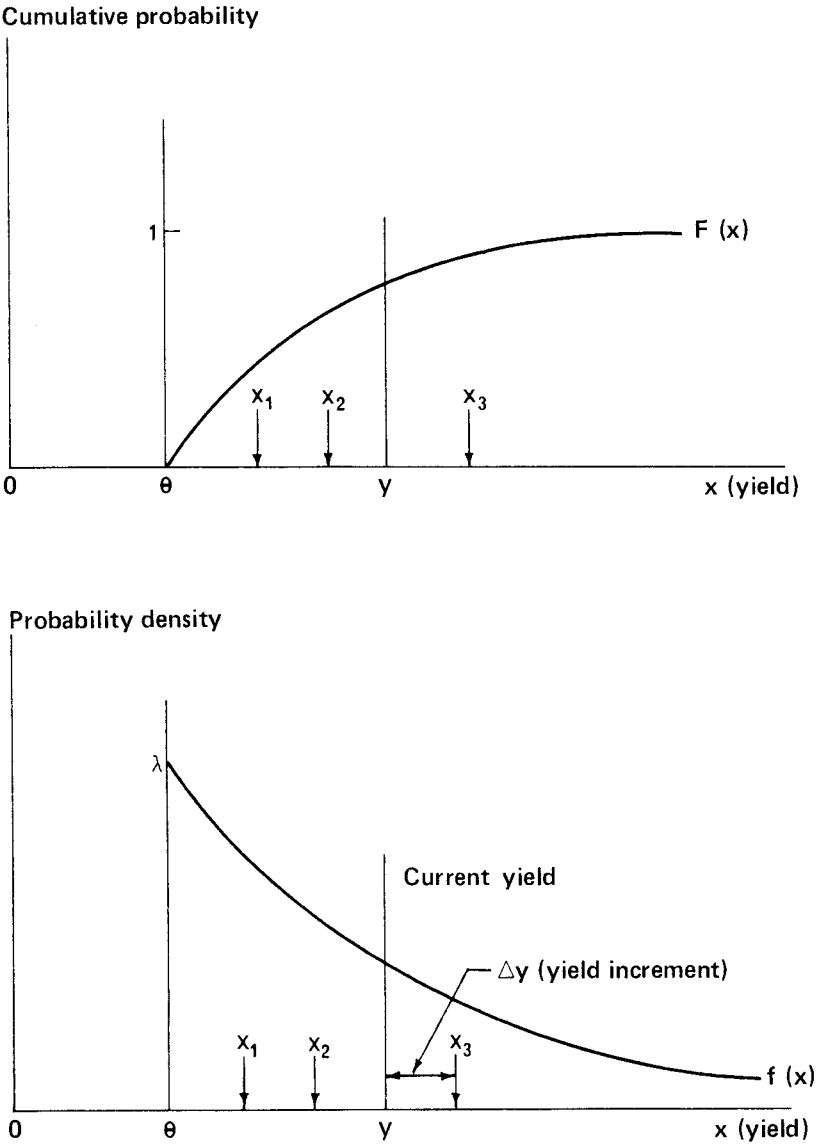

Figure 10-2. The exponential distribution with a sample of three observations.

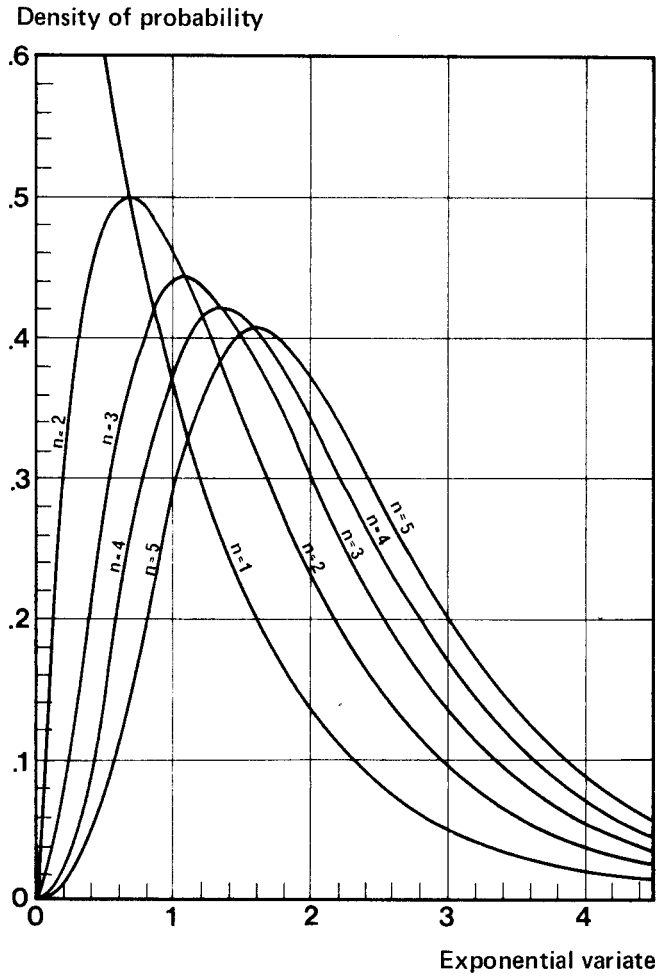

Figure 10-3. Density functions for maximal values of the exponential distribution $f(x) = e^{-x}$ for $n - 1, \ldots, 5$.
Source: E. J. Gumbel, *Statistics of Extremes* (New York: Columbia University Press, 1958), by permission of publisher.

means that for an ongoing research project the economic value of the expected technological improvement is lower than $1/r\,[E(\triangle y)]$, but this does not affect the relevant properties of the research system to be analyzed below.

The economic problem of the research system is to decide on the optimal amount of experimentation to conduct in each period – on the optimal num-

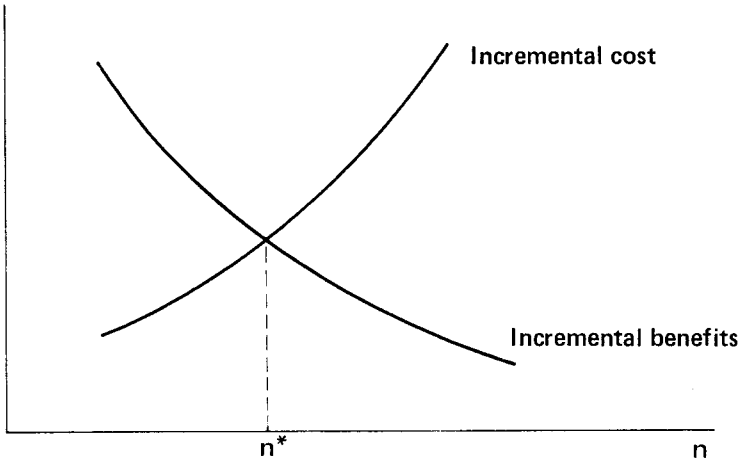

Figure 10-4. Optimal number of observations, n* (the smooth graphs in the diagram approximate step functions).

ber of observations, n, to draw. A reasonable assumption is that cost of research is an increasing function of n. Average costs may decrease for small values of n. This introduces nonconvexities which are assumed away at this stage. As an examination of Figure 10-3 will reveal, incremental technology improvement is a decreasing function of the number of observations; that is, each additional observation adds something to the expected technology increment, $E(\triangle y)$, but these additions get smaller and smaller as the number of observations increases. The magnitude $E_n(\triangle y) - E_{n-1}(\triangle y)$ is positive and decreasing with n. We get, therefore, the familiar equilibrium position determined by the intersection of marginal (incremental) cost and returns (see Figure 10-4).

Implications and Extensions

Technological research is more fruitful the wider the divergency between the level of theoretical scientific knowledge and the level of technology in practice. Nelson and Phelps made this assumption in their model of technological diffusion and schooling.[7] Evenson and I estimated the increase in the productivity of research in one country owing to the availability of relevant knowledge in other countries.[8] Technological gaps also explain rates of international technology diffusion when adaptive-type applied research is needed to facilitate the transfer of knowledge (see chapter 8).

In our framework a formal definition of the *technological gap* is the differ-

ence $y - \theta$ (see Figure 10-2). Optically, this may seem inappropriate, as one is inclined to define the technological gap as the difference between some potential technological ceiling and the current level of technology. But such a ceiling can probably never be defined, and our measure, the magnitude $y - \theta$, measures the ease at which new technological improvements can be achieved. On more formal terms, the *smaller* the difference $y - \theta$ the higher the probability of finding technologies superior to the currect practice for a given rate of experimentation – a given n.

With our assumptions, $\triangle y$, the technological change, can be only positive – technology can only improve with time, closing the technological gap as it improves. Even if experimentation continues at the same rate (n = const.), technological change will decrease with time. Moreover, since the marginal expected returns to research will decrease with time (as the technological gap closes), optimal n will decrease with time, reducing even further the acceleration rate of technology. Eventually, a point will be reached at which expected returns will be smaller than the cost of a single observation, n = 1, research will stop, and technology will stagnate forever.

It is worthwhile to recall at this point the assumptions that led to our conclusions: (a) technology will stagnate so long as basic knowledge is constant (θ = const.); and (b) research will stop as its payoff diminishes. Even if θ = const., research will continue if technology deteriorates or is subject to obsolescence. This might be termed maintenance research – the research necessary to maintain current productivity levels.

The optimal rate of experimentation depends on the present value of future probable benefits from research – $1/r\,[E(\triangle y)]$. The *lower* the rate of interest, the higher the present value of future benefits and the higher the optimal level of technological research. Like any investment, research (investment in knowledge) is a decreasing function of the rate of interest.

Scale can enter the economics of research in several ways. Evenson discusses the effect of the scale of the experiment station.[9] Here economies of scale in the creation of knowledge stem mainly from the interaction of scientists working in different disciplines and from the integration of research and graduate university training, factors which cannot be introduced into our model in its present, simplified one-product stage.

Another, different aspect of scale effect is the size of the industry which is affected by the new technology. The larger the industry, the higher the benefits to research. But, particularly in agriculture, size of industry as measured by acreage, for example, is also associated with diversity in conditions of production. In this case benefits will not be directly proportional to scale.[10]

Recall that the first stage in the development of the modern sugarcane va-

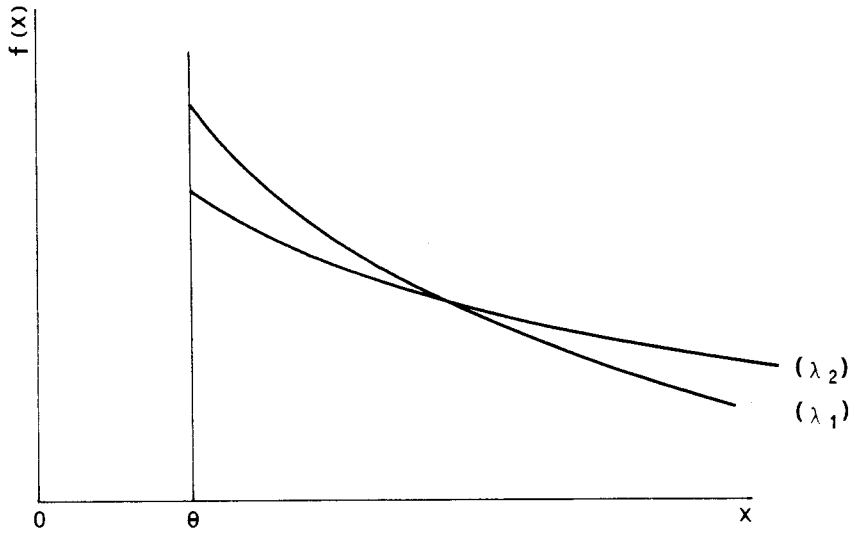

Figure 10-5. The effect of an increased variance $[\mathrm{Var}(x) = \frac{1}{\lambda^2}, \lambda_2 > \lambda_1]$.

rieties consisted, in fact, of the discovery of a variance-increasing technique. The importance of research designed to develop a genetic pool that incorporates greater diversity (variance) is not always properly appreciated. The collection of varieties, radioactive radiation, methods for creating new chemical compounds—all these are variance-increasing techniques which are followed by search and selection types of research work.

In our model the variance parameter is λ. (In the exponential distribution it is also the mean parameter $[E(x) = \theta + 1/\lambda]$.) Reducing λ will increase the probability of finding higher x values (see Figure 10-5) of improving technology. Often applied research will not be economically justified until ways to increase the variance of the samples observed are developed.

Basic Research

Basic research widens the technological gap and increases the probability of finding superior technologies. In our model, basic research will shift the parameter θ to the right (see Figure 10-6) and will increase optimal experimentation.

It is interesting to note here a *steady-state property*. Assume that basic research and applied research proceed at constant rates (per period $\triangle\theta$ = const., n = const.). Then the rate of advancement of technology in practice will ul-

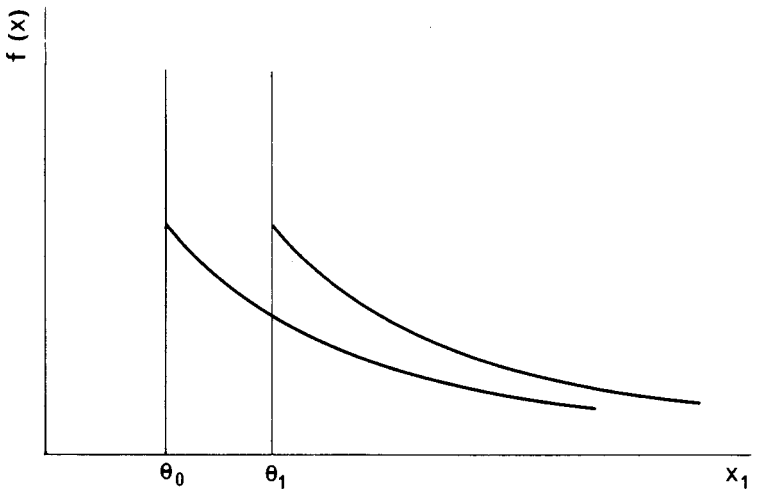

Figure 10-6. Basic research shifts the population searched to the right.

timately converge to the rate of advancement of basic knowledge. (A special case has already been encountered: in the absence of basic research, technology will eventually stagnate at a constant level.) The level of technological research in the steady state does not affect the *rate* of technological advancement, but only the *level* of technology in practice (see Figure 10-7).

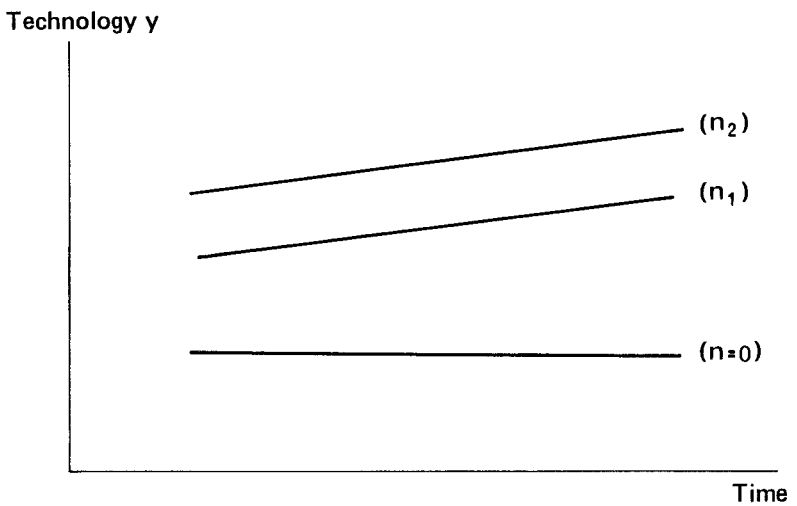

Figure 10-7. Time path of technology in the steady state at different levels of experimentation ($n_2 > n_1$).

The proof of the steady-state property is based on the fact that technological improvements at a rate faster than the advancement of basic knowledge will close the technological gap and decrease the probability of further technological change. Slower technological improvements will increase that probability. The endogenous variable in the system is the technological gap – it will be adjusted to maintain a steady-state equilibrium, with technological change fluctuating stochastically around a constant rate of change.

It can also be shown that in the long run, if basic knowledge proceeds at a constant rate, constant rate of experimentation is optimal. Thus in the long run the steady state is the optimal state of a research system fueled by a constant rate of new basic knowledge.

Concluding Remarks

Perhaps the most dramatic recent development in the field of agricultural research is the establishment of the international research centers. With their size and resources they can draw the best scientists, and they are well equipped to perform their task. Two elements contribute substantially to the success of these centers: (1) a wide technological gap – an example which comes immediately to mind is the case of the *Norin* 10 Japanese dwarf wheat variety which served as the basis for the development of the Mexican wheats and later provided principles for the breeding of the "miracle rice" varieties; (2) the large variance of the populations they search – the International Rice Research Institute (IRRI) alone has a collection of 30,000 rice varieties, and the International Center for the Improvement of Maize and Wheat (CIMMYT) performs tests all over the world. The subtropics and the tropics have been comparatively neglected in agricultural research, and the potential exists there for successful international efforts. (On these issues see chapters 7 and 9.)

Yet eventually, once the gap has been closed and the large variance exploited, the rate of progress of technology will be limited by the advancement of basic knowledge. Thus, in principle, the contribution of the international centers can be expected to decline in time (after an initial stage of acceleration).

The initial focus by the centers on wide-base technologies was clearly consistent with the objective of efficient use of research resources. Mexican wheats are now grown over large areas and under diversified geoclimate conditions. In following this procedure, the centers opened up technological gaps in the countries that were recipients of the new technologies and dramatically increased the payoffs to local adaptive research. Such explicit signals are being recognized by public authorities. As the potential gains are recognized, the effect is to expand and strengthen research institutions in many developing countries. The more successful the international centers, the more produc-

tive will local research work be. This will have a significant impact on the future role of the international centers. They cannot be expected to remain the sole producers of modern technology. Rather, additional emphasis will be given to their function as clearing houses for knowledge and genetic material. An excellent example is IRRI's recent International Rice Testing Program in which "scientists are cooperatively developing a 'critical mass' of genetic technology that will feed improved genetic materials and breeding information to scientists across the rice growing world." The main feature of the program is that "each nation contribute[s] . . . genetic materials, testing results and breeding information to help develop the critical mass effect."[11] Such developments will reduce the *relative* contribution of the international centers to global agricultural research. Furthermore, more effective scientific communication systems can be expected to reduce the importance of the geographic concentration of scientific work in one locality. These effects should not be expected to materialize fully in the near future, but the centers should be ready for them, as they should be welcome. The success of the international research efforts will induce competition among national systems. This result should be welcomed as an indication of the successful contribution of the institutes to global research capacity.

I have tried to point out clearly and explicitly the simplifying assumptions of the model presented in this chapter. I hope that despite its limitations the model can serve as a starting point for a theory of research and as a steppingstone for further analysis. Two reservations are worth mentioning in conclusion.

In the present formulation, basic scientific knowledge is exogenous to the applied research system. Ruttan has pointed out the reverse link whereby advances in technology – in instrumentation, for example – contributed to further advances in basic science.[12] Changes in factor supplies or in demand may also exert an independent impact on the productivity of technological research. This observation sheds strong light on the "system" nature of the science-technology complex which stretches even further. For example, the level and quality of university education is to a large extent a function of the quality of the research conducted at the institutes of higher learning. Thus a high-quality scientific community breeds the technical personnel that later conducts the applied and adaptive research. On the other hand, the demand for technical skills, if properly channeled, strengthens institutions of higher learning and "pure," basic science. (I am, of course, abstracting from a host of complicated and important issues which are outside the scope of our present discussion.) Against the background of this broad view, ours is a partial analysis – one building block of the system.

There remains one shortcoming in our understanding of the operation of

public research systems which I should like to reemphasize here, and this is the nature of the connection between the demand, or the payoffs to research, on the one hand, and the policy maker and the scientist on the other. What is the "market" in which such a system operates? What signals does it follow? What "profits," if any, are maximized? Hayami and Ruttan showed that research in Japan and the United States reacted efficiently to economic incentives.[13] Why did it not react in a similar manner in many other places? This gap in our knowledge and understanding is part of a larger gap: the lack of an economic theory of bureaucracy and our meager understanding of the process of economic development.

NOTES

1. This study was supported in part by the United States-Israel Binational Science Foundation.

2. Yujiro Hayami and Vernon W. Ruttan, *Agricultural Development: An International Perspective* (Baltimore and London: Johns Hopkins Press, 1971).

3. An exception is the "innovation possibility frontier" suggested by Charles Kennedy, "Induced Bias in Innovation and the Theory of Distribution," *Economic Journal*, 74 (1961), 541-547. For a critique of Kennedy's model see H. P. Binswanger, "A Microeconomic Approach to Induced Innovation," *Economic Journal*, 84 (1974), 940-958. See Hayami and Ruttan, *Agricultural Development*, for references to other sources on these issues.

4. G. J. Stigler, "The Economics of Information," *Journal of Political Economy*, 69 (1961), 213-215. R. R. Nelson, "Uncertainty, Learning and the Economics of Parallel Research and Development Efforts," *Review of Economics and Statistics*, 43 (1961), 351-364.

5. This is a nonmathematical exposition. For rigorous formulations and proofs, see Robert Evenson and Yoav Kislev, *Agricultural Research and Productivity* (New Haven: Yale University Press, 1975), chapter 8.

6. B. Epstein, "Elements of the Theory of Extreme Values," *Technometrics*, 2 (1960), 27-41.

7. R. R. Nelson and Edmund S. Phelps, "Investment in Humans, Technological Diffusion, and Economic Growth," *American Economic Review*, 56 (1966), 69-75.

8. Robert E. Evenson and Yoav Kislev, "Research and Productivity in Wheat and Maize," *Journal of Political Economy*, 81 (September-October 1973), 1309-1329.

9. Robert E. Evenson, "Economic Aspects of the Organization of Agricultural Research," *Resource Allocation in Agricultural Research*, ed. Walter L. Fishel (Minneapolis: University of Minnesota Press, 1971).

10. For an empirical attempt to cope with some of these problems see Evenson and Kislev, "Research and Productivity in Wheat and Maize."

11. *IRRI Reporter* (Los Baños: International Rice Research Institute, February 1975).

12. Vernon W. Ruttan, 1975, personal communication.

13. Hayami and Ruttan, *Agricultural Development*.