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*The Impact of New Technology on Foodgrain Productivity to the Next Century***

INTRODUCTION

Our view of the world derives from our recent experience with endeavours to understand the role and function of international agricultural research concerned with major food grains. We have also been interested observers of developments in improved technology generally and the likely implications for future impacts of agricultural research. We take the view that the experience in agricultural research and relationships with agricultural productivity over the past few decades are indeed relevant to considering what is going to happen over the next few: 'The future will be like the past because in the past the future was like the past' (Weinberg, 1975).

This is not to argue, of course, that there is not going to be any change in agricultural productivity and production over these future years. Rather, the changes that will occur will bear a strong resemblance to past changes as progress proceeds through the existing R&D pipeline (second section). The particular innovations will naturally differ but their net effects will, inevitably, be similar to those observed in the post-World War II era. In turn, many of these changes themselves mirror strongly those of earlier periods, although the rate of change was significantly different and, for some major crops in some major producing areas, of sufficient magnitude to warrant description as 'revolution'.

Turning in the third section to new technological pipelines, some observers have coined the term 'biorevolution' to hint at the likely continuance of green revolutionary trends, recognizing that, in this future scenario, biotechnology will be an important driving force and may do for crop production things analogous to what was achieved through more conventional plant breeding techniques in the lead-up to the Green Revolution (Buttel, Kenny and Kloppenburg, 1985; Wolf, 1986; Longworth, 1987).

EXISTING R&D PIPELINE EFFECTS

The dramatically rapid adoption of modern wheat and rice varieties that was characterized as the Green Revolution has occurred on most of the areas for which

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the varieties are suited. There are still opportunities and need for increased productivity in the many ecologies where the modern rices and wheats are not suitable. Of course, it was not only the modern varieties that were critical to accomplishing the rapid changes in productivity and production. The adoption of these varieties was significantly influenced by the availability of irrigation, and has yet to happen in many areas which are not well endowed with irrigation infrastructure. Mechanisation was also an important factor in many areas where modern varieties were quickly adopted. The most significant factor of all, however, was probably the changed adoption of more intensive fertilizer use. Nitrogenous fertilizers in particular became much more profitable with the modern varieties under fairly favourable crop moisture regimes. The widespread use of much heavier doses of fertilizer explains a large proportion of the apparent gains to crop productivity. Disentangling these several important effects is not straightforward and has yet to be done in a definitive manner.

Potentials for increasing food production can conveniently be considered for the short, medium and long terms. In the short term, increased food production can only come from fuller utilization of existing technologies through their wider spread or their intensification. Wider spread of known technologies occurs in response to changing incentives that make such technologies more attractive, by increasing farmers' knowledge of the technologies, and by assuring adequate supplies of complementary inputs. In many countries these changes require political and economic policy changes, which could be forthcoming with a demonstration of great unexploited technical potential for increased production.

In the medium term, adaptive research to change production technology, and further investments to change the environment to make existing technologies more attractive, must be the principal sources of potential increased production. Adaptive research may include technology transfer, although the potential for direct transfer across agricultural ecologies is necessarily limited.

In the long term, advances in basic sciences and their applications to agriculture will probably be major factors determining the rates of output increase. The theoretical possibilities offered by recombinant DNA and other techniques of molecular biology appear to be large but until there has been more experience with such technologies there is little that can safely be said about their potential.

More of the same

Existing high productivity technology can contribute to further increased production if its use is extended to new areas. What is the potential for further spread of semi-dwarf wheat and rice technology?

Dana Dalrymple, in a series of reports culminating in Dalrymple (1986a and 1986b), has monitored the spread of semi-dwarf varieties of wheat and rice for principal producing countries in the developing world. By 1982–3 semi-dwarf varieties had spread to about 50 per cent of the wheat and rice area, leaving an apparent ample scope for further spread. However, examination of their spread across countries shows that their rate of spread has slowed. Walker and Singh (1983) have argued that high yielding varieties of sorghum and millets also seem to have reached a plateau of adoption in India, usually at about 50 per cent. Thus, while there is still some scope for further spread of semi-dwarf varieties, it is unlikely to be rapid and, because they will spread mainly to non-irrigated or newly irrigated land, the associated productivity gains will be considerably lower than on the initial adoption areas.

What scope exists for further intensification of production practices where semi-dwarfs are already so important in, say, Asia? This could be answered with good (but, unfortunately, non-existent) production function estimates that separated the effects of fertilizer, irrigation and variety. Herdt and Capule (1983) and Barker and Herdt (1985, p. 268) have used a land-quality based approach to analyze the contribution of each input in the case of semi-dwarf rice. Individual models were developed for eight countries that produce 85 per cent of Asia's rice. The adequacy of the projected level of production of 409 Mt by 2000 can be judged only by comparing it with the projected level of demand. Demand was projected using estimates of income, population growth rates and income elasticities. With the base-run supply projection, only Thailand will export in the year 2000, when net imports for the eight countries are projected to reach 35 Mt in order to hold real prices constant. If self-sufficiency is imposed, rice prices are projected to be nearly double their 1980 levels by 2000 and consumption to fall from 135 to 125 kg/head.

Under this projection most countries will reach what are, with present technology, rather high levels of fertilizer application and modern varieties will have spread about as widely as could be expected given each country's irrigation capacity. There may appear considerable scope for the extension of irrigation, especially in Thailand, Burma and Bangladesh, as well as in a number of other countries where only one-half to two-thirds of the rice area is projected to be irrigated by 2000. However, irrigation is increasingly expensive and in most countries its construction is actively constrained by the capacity to mobilize the necessary human and capital resources.

It is highly unlikely that the irrigated area can grow much faster than is assumed in the base case but to determine the potential effect of greater investments in irrigation, a rapid growth scenario was developed in which it is assumed that irrigated rice area grows at twice the rate of the historical period. Average irrigated area for the eight countries would reach 62 per cent compared to 54 per cent in the base case, and modern varieties would reach 72 per cent of the total rice area. Under this scenario, rice production is projected to be 466 Mt, net imports would reach only 13.6 Mt, and Thailand, Burma and Sri Lanka would all export if rice prices are held at their 1980 levels.

There seems little significant 'unused potential' in current rice technology and continued improvements in technology as well as increased fertilizer use and irrigation investments will be needed to produce enough rice to feed Asia over the coming several decades. Similar studies are only just appearing for wheat (CIMMYT, 1988) and, while revealing diminished scope for varietal improvement (down to 0.7 per cent per annum) and continuing opportunity for crop management research, predict total yield gains of about 2.3 per cent per annum to 2000.

Farmers' versus 'potential' yields

Clearly, many factors contribute to crop yield. Crop variety, fertilizer nutrient level, control of pests and availability of water are all important. Planting date, soil chemical characteristics, drainage and weather conditions at harvest are less often mentioned but are also important. Solar radiation and temperature are usually the overriding factors determining potential yields. Thus, depending on what factors are controlled at what levels, a number of different yield levels may be thought of as 'potential'. Some definitions may help.

For convenience, the terms 'experiment station' and 'on-farm' trials are used. Each is understood to be representative of such conditions in the region of interest. 'Environmental conditions' and 'management factors' are used to mean, roughly, non-controllable and controllable factors. Experiment stations are observed to have invested more than most farmers in controlling environmental factors. In fact, there is a continuum of situations some of which can be identified fairly unambiguously, namely:

(a) physiological (special greenhouse) potential yield;
(b) experiment station maximal yield;
(c) experiment station 'economic' optimal yield;
(d) on-farm trial maximal yield;
(e) on-farm trial 'economic' optimal yield;
(f) farmers' typical yield in on-farm trials:
(g) average (say, official statistical) yield.

Research trials can be misleading indicators of potential achievable on commercial and subsistence farms. Their trends lead those in farmer's production but a gap between them is normal. Farmers everywhere are responsive to opportunities offered by new technologies or changed economic conditions and rapidly adopt what benefits them. Researchers can easily be misled about what is beneficial to farmers for a number of reasons: they may use inappropriate prices, fail to account for all the costs that farmers face (Perrin *et al.*, 1976) or, worse, ignore both prices and costs and assume that farmers' production conditions are represented by experiment stations.

It is thus normal to observe a substantial difference in yield between experiment station and average yields. Maize data from the United States illustrate the point. For the decade of 1923 to 1933 experiment station yields in Cass County were 112 per cent higher than county average farm yields. For 1943 to 1953 they were 49 per cent higher, and from 1967 to 1977 they were 55 per cent higher (Fargo AES, 1986; North Dakota, annual). Over the entire period, the experiment station yields were an average of 64 per cent higher than the county yields and 98 per cent higher than North Dakota yields. Average farm yields increased 112 per cent from the 1923–8 period to the 1973–8 period, while experiment station yields increased 66 per cent. Similarly, for a shorter period, maize yields in North Dakota in the early 1960s averaged 1.5 t/ha while experiment station yields were nearly 4 t/ha. By the early 1980s state yields had increased to 4 t/ha while station yields were 7 to 8 t/ha.

A comparison for soybeans has revealed a similar phenomenon. These data match results from 63 experiment stations with the average yields in the counties in which they were located (Ruttan and Schoenek, 1982). In 1943–7 the experiment station yields were 73 per cent higher than the county averages. For 1959–63 they were 69 per cent higher and for 1975–9 they were almost the same percentage higher. Average farm yields increased about 40 per cent over the period and average experiment station yields increased 35 per cent.

In Illinois, the Morrow plots have demonstrated, for more than 100 years, the effect of different soil management treatments on maize and other crop yields. While not strictly a maximum yield experiment, various treatments have been designed to demonstrate high yields and provide a basis for comparison (University of Illinois, 1982). The Allerton Trust Farms in Pratt County were deeded to the University of Illinois in 1946. They are not experimental but 'are managed to produce maximum income to support the operation and maintenance of the Robert H. Allerton Park and Conference Center' (Swanson, Smith and Nyankori, 1977).

Morrow plot yields remained substantially above Allerton Farm and county averages over a 20-year period of comparison, even though there was some variation. In the mid-1970s state average yields (6.4, 2.9 t/ha for maize and soybeans, respectively) approached the county average yield which by then had reached the Allerton Farm yields (8.0, 2.5 t/ha), but experiment station yields (9.4, 2.9 t/ha) maintained an advantage over these.

The long continuation of a yield gap in several US situations, paralleled in Australia for a range of crops (Davidson, Martin and Mauldon, 1967), illustrates that this is the 'normal' situation and cannot be taken as *a priori* evidence of exploitable technology. The dramatic difference in the gap between experimental and county average yields, on the one hand, and maximal profit and county average yields, on the other, also suggests that experiment station yields need to be looked at carefully before any conclusion is drawn that they reflect yields that could be economic.

Some potential yields

Rice: IR8 and TN1, the first modern rice varieties released to farmers in 1965 had their shortcomings, but were extremely high yielding as long as insects and diseases were absent. When such pests attacked them, they quickly succumbed. Since 1965 a series of newer, much more insect and disease resistant varieties have been produced by IRRI and the national rice research programmes of Asia. One of these varieties, IR36, is estimated to have been grown on 20 million hectares during the early 1980s; but by 1985 newer varieties had largely but not completely displaced it.

Casual familiarity with these facts has led many observers to assume that the rice varieties developed in the 20 years since IR8 was released must be higher yielding than IR8. Average experimental yields from a long-term set of fertilizer response trials organized by IRRI and conducted on three widely dispersed Philippine government research stations and at IRRI (IRRI annual report; Flinn and de Datta, 1984) reveal a different picture. Yields of IR8 declined after 1965 and yields of the highest yielding entry also declined, although less rapidly. They certainly do not show an increase over time. This is not to suggest that the newer rice varieties do not have advantages over IR8. Their yields are much more stable in the presence of insects and diseases than those of IR8 and they mature faster, thereby permitting intensification of land use, but there is no higher yield potential *per se*.

Wheat: 'Potential' yields for semi-dwarf improved wheat varieties developed by CIMMYT and the Mexican agricultural research establishment have been reported by CIMMYT (1985). Unlike the rice data, there is an indication that yield potential has continued to increase since 1965, but the increase has been rather slow and, in recent years, modest, compared to the 'breakthrough' in 1961–6. As in the case of rice, the difference between national average yields and the potential as measured in the experimental data has been eroding since the mid-1960s.

Maize: The maize story is more complex, especially in most developing countries. There was no dramatic spread of semi-dwarf or other 'new' maize varieties in the developing world, as with wheat and rice, even though many of the same institutions have been involved in maize and wheat research. Reasons for this difference are complex and beyond the scope of this discussion but, because the international research system has been active, there are data that can be examined to determine the performance of the available technology.

Briefly, there are many rather formal experiments indicating large gaps between available technology and farmers' practices. However, a growing body of data, often arising from farming systems research programmes, is revealing that such gaps are often modest indeed (for example, Harrington, Thiraporn and Wattanutchariya, 1984; Collinson, 1985; Pham. Waddington and Crosa, 1986).

It is hazardous to generalize about complex and diverse phenomena from data sets that are still limited in many ways (especially over time, space and ecology), but some on-farm trials suggest that improved maize varieties may be expected to add no more than 1 t/ha yield and often less, and that improved varieties, together with fertilizer and other inputs may add no more than 1.5 t/ha and often less in on-farm trials. Further, economic analyses of the yield increases obtained from high levels of fertilizer and other inputs show that often they are not profitable. This was the case for trials in Haiti and Thailand. Such data support the hypothesis that there is little evidence of large exploitable yield gaps for maize.

Sorghum and Millet: It is perhaps not widely appreciated beyond South Asia that high-yielding hybrid varieties of sorghum and millet were developed for the semi-arid areas of India during the 1960s, and that by the 1980s they had spread quite widely. These hybrids, like semi-dwarf rice and wheat, were more highly responsive to fertilizer than were local varieties (Parthasarathy and Ryan, 1983), and were attractive to the many Indian farmers who adopted them.

Analysis of the adoption of hybrid sorghum shows low use in areas where sorghum is mainly grown as a winter crop. Also, the adoption process seems to have been largely completed by the late 1970s in the sense that adoption had reached a plateau, although at significantly less than 100 per cent (Walker and Singh, 1983).

For Africa, the situation is very different. New appropriate cultivars must be developed within the ecologies of that continent. 'Among some 7000 sorghum introductions screened by ICRISAT in Burkina Faso, nine cultivars were found sufficiently promising in on-station trials to warrant on-farm tests. Of these, only two cultivars have been found to be generally superior under farmer conditions. Among some 3000 millet entries screened, five cultivars have been advanced to on-farm tests but no superior cultivars have yet been identified' (Malton, 1985). A programme to develop improved sorghum and millet varieties that will raise production and be acceptable to West African farmers is under way at the ICRISAT Center in Niger, but it will require considerable time to produce superior varieties.

NEW PIPELINES?

The discussion thus far has focused on increasing production by using technologies that already exist, that is, technologies that can be examined in experiments, although they are not all widely applied on farmers' fields. What of the potential for developing still more productive technologies? Past yield gains have come through improving plants and the environment in which they are grown. A large share of the environmental improvement has been in the form of water and nutrient control that requires capital investment or current expenditures. Economic incentives to apply fertilizer nutrients, improve management or invest in water control are interrelated with the capacity of plants to make productive use of the 'improved environments', a capacity that has been generated through plant breeding.

Potential medium-term productivity increases

What of the future potential for developing still more productive technologies? Does plant biotechnology hold out the prospect for dramatic new yield breakthroughs? If so, will these breakthroughs come in the next 5, 10 or 25 years? Will innovations designed for the developed world have transferability to the developing world? These questions are difficult to answer, especially because science is just now being converted into technology and it is unclear what the limits of the technology will be.

Biotechnology provides new ways to improve plants and animals and new products with which to treat plants and animals. But biotechnology applications must be specifically designed for the target organism, and may require intensive research to refine the many separate techniques required by a process. For these reasons significant resources are required to bring new production technologies to farmers via biotechnology research.

The impact of biotechnology will be modest in the developing world this century for several reasons. The simplest is that few resources are being devoted to biotechnology research on crops of importance for the developing world. Aside from the rice biotechnology programme being funded by the Rockefeller Foundation, there is little research being done to develop the knowledge and tools of biotechnology for developing country crops such as cassava, sorghum, millet and yams. Second, even for crops that are important in both the developed and the developing world (for example, maize and wheat) there is likely to be little contribution to the developing world from ongoing industrial country research because of the location-specific nature of so many agricultural innovations.

In the case of rice, where there is a programme aimed at the developing world, the prospects over the next decade for genetically incorporating the capacity to resist the attacks of specific insects or plant diseases, and for genetically engineered products such as biological insecticides to lower production costs, are relatively bright. However, the analogous prospects for dramatic increases in yield potential or for engineering plants to endure adverse environmental conditions are modest. The prospects for things such as incorporating nitrogen fixation capacity are even more remote. Why the differences? The first characteristics are controlled by a few relatively well-understood genes, while the factors that control yield potential and nitrogen fixation are complex and involve many genes that are difficult to isolate and identify.

These differences in outlook arise from what biotechnology is and can do. There are biotechnological tools to clone segments of DNA, tools to insert segments of DNA into organisms, and tools to determine whether a particular sequence of DNA exists in a particular organism. These can help speed the genetic improvement of crops, but the determination of what genetic characteristics will increase the productivity of a crop, the isolation of the genes for those characters, and the identification of a segment of DNA with a gene requires the growing of crops in the conditions for which they are intended and determining how they perform. The activities variously require the disciplines of plant genetics, entomology, plant pathology, and plant physiology. One biologist puts it this way:

> At present, however, our attempts to engineer plants can be likened to those of an electronic engineer who attempts to modify a computer for which there is no circuit diagram. He or she might know how parts of it worked, but would have no way of understanding how it is functionally integrated. There are far too many gaps in our knowledge of biochemistry and physiology to make it feasible to think in terms of planned and directed changes in all but the simplest of plant characteristics (Arnold, 1987).

There is a disagreement among plant scientists over the extent to which further yield potential increases can be achieved through genetic manipulation. H. K. Jain has examined the record of wheat, rice, barley and sorghum yield improvements during the past 80 years of crop breeding and concluded that most of the genetic basis for the yield gains has come from redistribution of dry matter between vegetative and reproductive plant parts and that 'there is little evidence to show that biological yield or the dry matter production has seen a significant increase during this period' (1986). He saw little possibility for further gains from this source and so concluded that the prospects are bleak. Other authorities do not agree. After enumerating five physiological routes to greater productivity, Evans (1987) concluded that he 'can still envisage many possible avenues to greater yield potential in wheat, and there is no reason to suppose that it is near its limit'.

The reason for the implicit disagreement seems to rest in the assumptions about what can be changed and how it contributes to productivity gains. Jain identified past gains in productivity with plant height and biomass partitioning. The genes controlling these characters have long been recognized, can be manipulated by plant breeders and could be modified further using genetic engineering. The potential sources of productivity gains identified by Evans are much more complex traits such as photosynthesis, timing of the reproductive cycle, and growth regulation. Many genes affect these processes and only a few have yet been isolated, so the route to their manipulation is likely to be much longer and will require much improved understanding of their basic nature (Arnold, 1988).

Notwithstanding such controversies, harvest index for wheat and rice can probably be lifted from around 50 per cent to more than 60 per cent but, especially in developing countries where the straw production of a crop can be very valuable, the lifting of harvest index for maize, sorghum and millets from usually rather less than 30 per cent will be difficult and slow. However, the potential for improving the maize yields in developing countries through genetics appears to be considerable, judging from advances made in the developed world.

An examination of the trends in yields of major crops in the industrial world provides some indications of the inherent capacity of crops to respond to plant breeding. Table 1 shows such a comparison. Average grain yields in North America and Europe increased by 100 to 300 per cent between 1950 and 1980 (leaving aside rice in Europe, which began with a very high yield. Yield gains for other major crops seem to be somewhat less than for the major grains, although dry bean yields in Europe and peanut yields in North America increased by over 150 per cent, and soybean yields in Europe increased by nearly 100 per cent, showing that yields of these crops have been increased, but not as dramatically as yields of cereals. In the developing regions, by contrast, yields of most crops have increased much less, suggesting that there is genetic capacity for yield improvements if the appropriate research is done. However, the examples of millet and sorghum in West Africa and maize more generally show that, in most cases, there is no easy transfer of technology from one agricultural ecology to another, even when the ecologies appear superficially to be similar. Research seemingly must be conducted in the agricultural ecology for which a technology is intended.

Taking a global view, our optimism derived from viewing the success in industrial nations and many developing nations more than balances the pessimism that can come from reflecting on the problems of parts of the developing world. There may be many important (often life-threatening) distributional problems to be overcome, but our growth scenarios suggest relative ease for the human world to feed itself quite adequately into the next century.

CONCLUSION

There is relatively little under-utilized technology simply waiting for farmers, especially in the developing countries, to adopt. Existing proven technologies such as semi-dwarf wheat and rice will spread further only at a modest pace. There is no apparent further 'break-through' just over the horizon. Maize, sorghum and millet technologies that would be substantial improvements over

TABLE 1 *Yields (t/ha) and yield changes of major crops 1948–52 to 1981–3, in two developed regions*

	North America			Europe		
	1948–1952	1981–1983	% Change per year	1948–1952	1981–1983	% Change per year
Wheat	1.16	2.32	2.2	1.47	3.77	3.0
Maize	2.49	6.37	3.0	1.24	5.05	4.5
Rice (paddy)	2.56	5.26	2.3	4.27	5.07	0.5
Sorghum Millet	1.24	3.59	3.4	0.85	1.45	1.7
Barley	1.45	2.69	1.9	1.68	3.41	2.2
Potato	15.58	29.17	2.0	13.78	19.02	1.0
Sweet potato	5.88	13.57	2.6	15.10	10.67	–1.1
Dry beans	1.19	1.58	0.9	0.22	0.58	3.1
Chickpeas				0.43	0.58	0.9
Soybeans	1.43	1.96	1.0	0.64	1.26	2.1
Peanuts (in shell)	0.94	2.49	3.1			

Sources: FAO (1966), *World Crop Statistics: Area, Production and Yield 1948–64* Rome, and FAO (1984), *Production Yearbook 1983*, Rome.

what exists will take quite some time to develop for the vast developing dryland regions where they are so important. Biotechnology's promise is somewhat uncertain, and it is clear that much time must pass before that promise can be realized.

Prices, and hence enforced price policies, can have significant effects on incentives to adopt new technologies, so it is conceivable that some technologies that are not sufficiently profitable to be attractive under current policies could be more attractive. However, policy changes cannot compensate for the lack of technical (yield) advantages, which appears, for example, to be the explanation for the difference between the observed rates of technical change for wheat and maize. Potentials for new technologies exist on every front, but all require further investment of research time and effort (OTA, 1985).

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DISCUSSION OPENING – EDUARDO VENEZIAN

This paper is essentially an intuitive analysis of future trends in yields (productivity) of the major cereals on a worldwide basis. As such, it mainly deals with scientific and technological issues that determine yield levels and trends. Except for passing recognition and comments on the importance of economic variables in determining observed yields and eventual adoption rates, it does not discuss these economic issues. I understand this has been a conscious choice by the authors, given the extensive discussion of economic aspects in the commodity projection models presented earlier in this Session. On the other hand, the authors, though economists, have a thorough knowledge and understanding of plant science research issues that give them a special advantage to engage in this broad review and speculative analysis of productivity trends. Unfortunately, I am not in a position to question their assessment and judgement of technical issues, although I find their review of evidence well done, imaginative in approach, and generally convincing in its conclusions that: (a) prospects are mildly favourable for continued productivity increases in cereals; and (b) investments in research and technology development must be maintained and/or increased to ensure this outcome. Given the intuitive nature of the study, the latter conclusion comes as somewhat of an act of faith, but I happen to fully agree with it so I will not question its soundness.

The main question that I would pose is the following: what does this analysis tell us about future productivity trends that are useful for policy decisions (economic and scientific); that is, what insights and guidance do we get for better allocating research resources?

With this question in mind, I will concentrate my comments on four points:

(1) The analysis is far *too aggregative* in nature, as it considers all cereals together, for the whole world. I suggest that at least two breakdowns would be quite useful to consider:

Geographic subdivision: This could be done by major agro-ecological zones of the world (thus recognizing that rapid yield improvements, say, in the temperate areas for wheat or corn do not have the same implications as if yield improvements also occurred for cereals in the tropics); or by some grouping of countries by economic criteria. For instance, an early study by IFPRI comes to mind, in which countries were subdivided according to income level and food surplus or deficit situation. Clearly, the nature of the cereal production and yield problem would be rather different in a rich exporting nation than in a poor food-deficit one. At least from the standpoint of allocating investment for research, the two cases are likely to lead to quite different decisions.

Commodity subdivision: Although the discussion in this paper is conducted separately for each cereal crop, the projection is fully aggregate. Since research and technology development proceeds largely on a commodity basis, it would obviously be useful for decision making to consider commodities separately. This separation should also make distinctions between these grains with respect to their use (food or animal feed), especially since industrial and less-developed countries are included in the analysis. The problems faced by the maize research of CIMMYT and its forerunner the Rockefeller Program in Mexico are well known: the high-yielding varieties, bred following the hybridization techniques developed in the US, have not been successful, in good part because of quality problems associated with the fact that maize is mostly for human consumption in Mexico.

(2) The authors emphasize the *role of crop breeding* or genetic research almost to the exclusion of other forms of research that might be quite significant in increasing cereal production and productivity. In this perhaps they show an implict bias or concern with research by the International Centres – although the paper (also implicitly) covers much more ground than that in the domain of the IARCs. The point is that other types of research, especially in the soil/water disciplines can contribute significantly to expanding the area (and sometimes yields) suitable for cereal production.

(3) The exclusion of *economic variables* (especially relative prices) from

the analysis subtracts a lot from the realism of the discussion and the usefulness of the main prescription. There is no doubt, as the authors clearly themselves recognize, that yield levels likely to be achieved in the years ahead will depend not only on technological factors, but very largely on economic ones as well. Thus, a cereal yields scenario for the next decade or so that implicitly assumes a *ceteris paribus* condition in regard to economic conditions represents a very restricted scenario. The example of wheat in Chile over the last four years provides a good illustration of the impact of pricing policies; the area sown to wheat nearly doubled, while at the same time yields increased by about 70 per cent just through correcting price distortions that had penalized wheat production for a long time.

(4) Finally, the highly aggregative model shows an approximate rate of growth of average cereal yields up to the year 2000. This rate looks quite high, compared with the historical rates experienced by developed countries and shown in Table 1. Also, the 'reserved optimism' that transpires from the discussion in the paper does not seem to warrant such a high rate. I suggest the data and/or parameters used should be revised.