craft spends in the magnetic field—or, more precisely, by the orbit which is flown, since the energies and intensities of the protons vary with position in the magnetic field.

From the point of view of shielding, the most significant feature of both the solar protons and the trapped protons is the fact that they have intensities and energies requiring, at least for missions contemplated in the near future, shield thickness of much less than 1 collision mean free path—a relatively thin shield. This means that the cascade does not develop appreciably either in the shield or in the man behind the shield, and thus is composed predominantly of primary particles. A large fraction of the dose that a man will receive in a typical spacecraft is due to the primary particles, and only a small fraction is due to the secondary particles. This is important because the uncertainty in calculating the dose from secondary particles is much larger than the uncertainty in calculating the dose from primary particles. Thus, for the case of spacecraft in which weight is a very important factor, relatively accurate calculations can be made.

In contrast to the shielding of spacecraft, the incident-particle energies of interest in accelerator shielding may extend into the multi-billion-electron-volt region, depending on the energy of the accelerator being considered. In general, whether we are dealing with an accelerator of modest energy of a few hundred million electron volts or with one of energy in the multi-billion-electron-volt region, the shield thickness required is such that the nucleon-meson cascade goes through its complete development and is well into the equilibrium region before the end of the shield is reached. Consequently, adequate design of an accelerator shield requires a very extensive treatment of the cascade.

Summary

The designing of radiation shields for manned space vehicles and for high-energy accelerators requires a knowledge of the nucleon-meson cascade that develops when a high-energy particle enters matter. The accuracy with which calculations of the nucleon-meson cascade can be made is to a large extent determined by the available information on particle production from nuclear reactions. The accuracy with which an effective shield can be designed is also determined by the available information on the biological effects of radiation.

In the case of present manned space vehicles and those contemplated in the near future, the secondary particles from nuclear reactions contribute only a fraction of the total radiation hazard, and relatively accurate design calculations for radiation shields can be made. In the case of high-energy accelerators, the secondary particles from nuclear reactions contribute the entire radiation hazard, and only very approximate design calculations for shields are at present possible.

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Agrobiology: Specialization or Systems Analysis?

The world-wide phenomenon of accelerated pace in growth and change suggests a new look at problem solving.

Neal F. Jensen

In the United States today, there is an increasing public awareness of the problems associated with world population growth. One of the problem areas directly involves agriculture; clearly it is the role of agriculture to provide the basics for food and clothing. The near disappearance of certain of our national food reserves (previously known as *farm surpluses*) in massive aid infusions to India and other less fortunate countries has sharpened the focus on the relationship between population and food supply. John Platt (*1*) and Max Ways (*2*) recently have called attention to the exponential nature of change endemic in the world today. In this paper I raise the issue of whether the approach of agricultural scientists to research directed toward most problems of the future is consonant with the accelerated pace of growth and change. The question is: can research remain arithmetical and confined to the present when all else is geometrical and directed toward the future?

The development of hybrid corn by geneticists and plant breeders stands as perhaps the best-known success story attributable to agricultural science, but it is generally forgotten that about 40 years elapsed from the time when the first critical papers were published until hybrid corn began to "move" as a commercial product. Some say this span was necessary to absorb and inte-

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grate the complexities of the new art. Was it? Cytoplasmic male sterility and fertility restoration are the foundation of modern hybrid corns (replacing the hand-detasseling procedure), but in 1933, while the double cross hybrid that made seed production economically feasible was still being developed, Rhoades published an account of the discovery of cytoplasmic male sterility in corn. This information languished for almost 20 years before serious attempts were made to adapt it to a hybrid corn system. The successful system, of course, was not completed until workable restorer systems were added in the 1950's and 1960's.

Recently a remarkably productive wheat variety was bred and introduced by Washington State University and the U.S. Department of Agriculture. The performance of Gaines wheat has been dramatic, setting world yield records that stand now at more than 200 bushels per acre. Thus I am sure it will not detract from the outstanding accomplishment of the breeder, Orville A. Vogel, to ask why the development of this kind of super star in the wheat world was not possible at an earlier date. Why did the pieces fall together in 1960? Why not 1930? We might also ask why plant breeders devoted three-quarters of a century to the improvement of wheat before serious attempts were made to breed for strong-strawed dwarf types—characteristics that today are absolutely crucial to field crop standability and high yields? The answers to these questions are easy but they do not satisfy. A gnawing doubt lingers: could not these developments have been anticipated and goals adjusted as time passed? The implications of these and similar questions bear importantly on the rate of future developments in agricultural research. Science has an infrastructure all its own; is it possible that our ways of looking at things—particularly, things of the future—need examination and, in the light of this, possible change?

The Futurists

The first deduction we can make from the questions is this: agricultural scientists do not comprehend the future well enough to use their knowledge effectively for planning. Particularly is this true when there exists a necessary time lag between having a piece of knowledge and translating it into a useful form. Recently there has been a development which *Time* calls "the new art of futurism," an activity wherein intelligence is brought to bear upon the problems and values of tomorrow in an attempt to visualize the real world in advance of its time. Practitioners of the art have been dubbed "futurists." The increased seriousness with which these highly intellectual deliberations are being regarded, is, I think, a reflection of our present expectation that the pace of change in the world today practically guarantees a future greatly different from the present and the past and we want to know as much about it as possible. I must point out that this has not always been so; before 1700, to choose an arbitrary example, the accumulated changes of a century might have been roughly equivalent in effect upon life to those changes that today take place within a decade or less. A glimmering of the art of "futurism" is now seen but it is not yet a significant factor in agricultural planning.

In my own field—cereal genetics and breeding—the inability to perceive the shape of the real world at a given point in future time has been and is a principal deterrent to advancement. In cereal breeding the minimum length of a particular problem-solving period is 10 years; it is often 15 years or more. This means that plant breeders today must direct their research towards goals that will seem reasonable by mid-1980 standards. I believe the record will show that we have not had this vision. The simple explanation of why wheat breeders single-mindedly devoted three-quarters of a century to the development of tall-strawed varieties is that they did not conceive of a real world in which these would no longer be suitable—a world with inexpensive and abundant nitrogen supplies, improved machinery, higher yielding varieties, better disease and weed controls—in short, our whole modern agricultural technology. Not perceiving, they did not initiate work with the proper lead time allowance.

One might say, with reference to the semi-dwarf wheats such as Gaines, that the critical parent-plant lines (from Japan) did not become available until after World War II, but they were available in Japan a decade before the war. Why did Japanese but not U.S. breeders have the dwarf prolific lines? The answer is that the future which we could only dimly see was already Japan's present. A large population forced to live off a fertile but meager arable land resource meant intensive cultivation and fertilization and practically hand care of plants for maximum production. Under these conditions there was no alternative to strong dwarf wheats, and that is why Japan had such types even in the early 1930's.

Systems Analysis

Now I turn from our narrow vision to a second point that will affect agriculture's rate of progress in the years ahead—narrow specialization. Another relatively new development outside the agricultural world is used in corporate and business planning, in the military, and increasingly in the federal government. This is systems analysis. The forerunner of modern systems analysis was operations analysis in World War II which led to the postwar formation of the Rand Corporation with its heavy emphasis on military strategy and weapons research. The advent of the computer helped spread operations research concepts through governmental circles and at this same time business began adapting systems analysis techniques to corporate procedures. Systems analysis permits a whole view of a problem; it takes into account means and ends, choices and alternatives; it makes use of prediction and advanced testing to suggest objectives and course of action. A likely first consequence of systems analysis is a restructuring of the problem itself. But, most importantly, systems analysis is a four-dimensional approach: it states explicitly that the problem is future-oriented.

In cereal breeding all problems are future-oriented. We have a minimum time lag of a decade from the perception of a problem to its solution. Systems analysis would be of inestimable value in that we would have from it an estimate of what the future total environment is likely to be at a given point in time and how the changes in the separate components of the environment might affect a chosen course of action. The following examples from my work with cereal breeding illustrate ways in which systems analysis could aid in this research:

1) New wheat types in the Cornell project and elsewhere are more efficient in light utilization for photosyn-

thesis, because of (i) a reduction in height and aboveground plant parts, which lessens shading; and (ii) a different shape, size, and angle of leaf. This suggests that a given area of soil could sustain a higher population of plants, each getting as much light as old-type plants, with a resultant increase in yield. But research on the cultural practices needed for narrow rows has been inadequate, and in any case there is no way to plant higher populations for better space utilization (that is, 4-inch rows) because farm planting drills in the United States have a fixed-row interval of 7 or 8 inches. Systems analysis would have forced an earlier comprehensive consideration of these factors.

2) A hardy winter barley of good malting quality would be a promising commercial crop for extensive areas of eastern and northeastern United States. The development of this hardy winter barley in the Cornell project is essentially complete, but production "know-how" and market development lag behind; in fact, there is no market structure at all in the Northeast. Systems analysis would have caused these critical activities to be foreseen and meshed together.

3) The protein level desired by industry for New York white wheats is about 8 percent. But, we believe that protein levels of nearly double this amount are attainable through breeding and selection. The cereal grains generally are low in total protein and in important amino acids; is it not strange that this desirable goal of more and better quality protein for a hungry world has been so long overlooked? Systems analysis would certainly have pinpointed this nutritional area decades ago as important enough to justify a dual-objective approach to variety development: one type of wheat for the milling and baking industry and another for other human and animal uses.

Hybrid Wheat

Hybrid wheat is an excellent illustration of an area of current high research interest that would benefit from systems analysis. Clearly we believe we are wise enough to avoid the mistakes and delays of the past. Indeed, this awareness and determination to make hybrid wheat a commercial *fait accompli* is most encouraging. One might ask, how are we doing with hybrid wheat? My assessment is that we are in fact doing promptly many of the things that must be done; my only criticism might concern an undue narrowing of research and a tendency to think along consensus lines. Since the discovery in 1962 of genes that restore fertility in the commercial farm crop, hybrid wheat has received great attention and research support. Regional and national conferences have been held. A conference is an admirable way to share and disseminate information, but it does not have the purpose or direction, the breadth of participation (particularly outside biology), the duration or the scope necessary for depth studies implicit in systems analysis. If, for example, hybrid wheat were subjected to systems analysis, I would first call attention to three areas of study in the hope that their consideration would lead to useful recommendations for action.

First, a strong program to discover or create sterile-restorer systems in addition to the one on which all hybrid wheat prospects are now predicated. This is vitally important. Already it is evident that the future of the hybrid wheat industry is viewed by many as resting on the very limited first breakthroughs when, in fact, there may be many possible systems, some potentially superior. Male steriles occur rather frequently in breeding and genetic studies; heretofore they have been eliminated as undesirable and useless products of a breeding program. Now—rather than 20, 30, or 40 years from now—is the time to press a search for other systems while there are still in existence many large conventional pure-line breeding projects.

A second recommendation would be research for a solution to a basic problem of hybrid seed production: inefficient land use because of the necessity of planting the cytoplasmic male sterile (seed producer) and male fertility restorer (pollen donor) seeds in alternate strips across a field with the result that less than one-half of the land area is actually producing hybrid seed. Pfeifer has proposed mixing the male fertility restorer seeds with the cytoplasmic male sterile seeds in a proportion of 5 : 95, respectively, and dispensing with alternate strips (*3*). This scheme would double the efficiency of hybrid seed production relative to land requirements but there are theoretical implications of somewhat lower hybrid crop performance and suitability because the selfed seeds of the male fertility restorer are harvested and included with the hybrid seeds, forming perhaps 5 to 10 percent of the total. A satisfactory solution to this problem requires a male fertility restorer wheat having some characteristic that would result in subsequent self-elimination of its own seeds from the harvested crop; the male wheats could then be mixed for planting with the cytoplasmic male sterile seeds. Theoretically, the random spacing of pollinator plants within the population should be more efficient in pollen dispersal than the alternate strip method because the wind-borne lateral movement of pollen would be augmented both by gravity and the jarring effects of "head bumping." The solution to this might be found in any one of such diverse fields as genetics, cytogenetics, plant morphology, plant physiology, seed technology, or agricultural engineering. As examples of possible solutions, the wheat could be self-sterile in a particular environment, or if fertile, possessed of some plant characteristics, such as shattering, that would result in self-elimination of seeds prior to or during harvest, or a seed characteristic that would permit separation of selfed from hybrid seeds after harvest.

My third recommendation would be to subject the design of hybrid varieties to relevant restrictions only. This may sound platitudinous but it is not; it is essential to give imagination free rein in designing the new varieties. Concepts appropriate to pure-line varieties should not be applied to hybrid wheat, where they may not be valid or useful. For example, the field uniformity of a pure-line crop serves a useful purpose in seed and crop promotional activities, because such uniformity is proof to the grower of the implicit claim by the seed vendor that trueness to type has been retained over many annual sequences of seed production; high uniformity has thus become synonymous with high quality. This concept has limited application to the new hybrid wheat. Plant breeders have come a long way in accepting the premise that a population may have visible uniformity and also great genetic diversity. Breeders have not, however, been willing to accept both genetic and phenotypic diversity—an arrangement that would add to potential productivity through: (i) a greater genetic diversity by the removal of possible genetic linkages with the characters affecting uniformity and through making possible a wider parent choice; and (ii) greater phenotypic diversity by removing the restrictions on variability in plant morphology, height, color, or types of heads. Research has shown that nonuniformity in a properly designed population of cereals can contribute to productivity through "bonuses" in environmental plant-to-plant competitive situations. Hybrid wheat opens many technical and procedural opportunities for variety development. Actually, breeders need satisfy only two aspects of uniformity in variety development: a maturity range with an all-ripe cut-off point to permit proper harvest procedures, and uniformity of product quality commensurate with intended use of the product. Beyond this, in my opinion, nonuniformity (genetic *and* phenotypic diversity) should be established as a desirable goal; performance and not appearance of the crop should be established as a value criterion. Recently I published a design proposal for a nonuniform hybrid wheat variety (*4*); to my knowledge this is the only such proposal that does not adhere to the concept of phenotypic uniformity.

Organized Problem Solving

Systems analysis should not be thought of only in terms of a wider examination of a problem; more importantly it should be thought of as bringing the resources of a broader group of skilled professionals from other disciplines to bear on the problem. I feel certain that a physicist, an evolutionary geneticist, a city planner, or an engineer would have insights into a problem that would be of value to a plant breeder. As an example, winter oats (fall-sown) is potentially higher yielding than spring-sown oats because the cool moist weather prior to spring oat planting time is especially favorable for cereal growth. The expected gains, however, are negated through winter killing because winter oats lacks sufficient cold resistance to northern winters. The genetic solution of increased hardiness appears distant. Searching for a way to obviate this difficulty I considered encapsulating the seeds in a waterproof coating that would prevent germination in the fall but, through the action of soil acids, would disappear and permit the seeds to germinate and take advantage of the early spring growing season. In 1966 I mentioned this problem to Robert A. Plane, now departmental chairman of chemistry at Cornell University; he at once suggested using a plastic that would harden to a brittle shell which would be cracked by alternate freezing and thawing during the winter. This illustrates how the solution to a problem in one discipline may be immediately apparent from the experience of another.

A practical approach, adequate to many situations, would be the employment of systems analysis on a modest scale, using locally available professional opinion; particularly since the implementation of recommendations from such an analysis would rest with the local research group, lacking foundation, governmental, or other outside financial support.

An exception and, hopefully, a trendsetter may be found in the proceedings (*5*) of a conference held in Chicago in April 1965. The conference examined the question of whether marketing, research, and educational agencies serving agriculture were responding properly to feedback from current and future structural changes in farming. A modest time projection was involved (to 1980). Whether the projections prove completely accurate is beside the point; they have provided one picture of the future world. It is a revelation to note the wealth of information generated by this conference—information that research scientists might well absorb as background for their own work. When systems analysis is applied to biological situations, another world of the future will be projected, which may prove as interesting to the economists as theirs does to us.

Finally, a third point: Gordon R. Ball recently made this comment (*6*) in *Fortune* magazine, "Many people who are referred to as scientists are actually engineers. Engineers deal between known minimums and maximums while scientists should be directing themselves to open-ended questions." Ball has put his finger on a crucial issue concerning the future of agricultural research. While related to the two points we have been discussing—narrow vision and narrow specialization—it is different; what we are talking about here is a narrowness of approach, grasp, and scope of a problem. The typical approach to problem-solving is to accept the problem as it exists and work directly towards a solution. This can be a narrow process and may lead to a workable but still unsatisfactory solution, as for example, in the use of alternate field strips in hybrid wheat production. We allow our conventional concepts to guide us. Instead, let us turn our attention more to an analysis of the problem itself to see whether it can be broken down into its component parts. This puts one into the world of theory and hypothesis. We should entertain the possibility that the end of a problem may come through the development of new means. For example, Adrian Srb, a biochemical geneticist, has proposed a novel tissue culture genetic system for plants, wherein nuclear fusion and genetic recombination would occur outside the sexual cycle (*7*). Such a system would open a whole new range of hybridizing opportunities for plant breeders, and perhaps make mutation breeding feasible. This hypothesis has not been tested. One would be well advised, however, not to ignore this or similar proposals merely because they deal with the unknown or the unfamiliar.

We might borrow an idea from the manufacturers of small gasoline motors who include a disassembled display of the engine in the instruction book. This "exploded parts" analogy is useful in biology; such a display of a problem may suggest other solutions. The satisfactory solution to the hybrid wheat problem, both from a biological and economic standpoint, is to devise a way to incorporate the male restorer plants in the sterile population without their seeds appearing in the harvested hybrid wheat. Encapsulating winter oat seeds has little to do with cereal genetics or plant breeding but it might furnish a useful temporary or even permanent solution to the problem of winter survival; note that this possibility stems from a redefinition of the problem to include artificially delayed germination.

I am aware, of course, that in this article my choice of many illustrations from my own specialization evidences narrowness, but it should be clear that the examples I have given are general phenomena pervading agricultural research. Indeed, I do agriculture an injustice by singling it out for criticism; our society's general approach to problem-solving tends to equate hindsight and corrective measures with planning and a forward look. Those like myself who work in narrow specializations are often aware of gaps in the process, but are reluctant to leave the comfortable boundaries of specialized research to become involved in the hurly-burly of group action. Yet it may be that our ability to keep food production in step with population will rest to some extent on the degree to which we approach problem-solving on a wholly integrated basis.

In conclusion, I should be sorry if this article implied that agricultural scientists must climb down from their ivory towers and enter the real world. Quite the opposite. I believe that they are already in the real world, but it is the world of the past and the present and not sufficiently of the future. Among the different paths to the real world of the future is one that leads through the door *to* the ivory towers. The future pace and accomplishments of agricultural research may well be determined by the extent to which agricultural scientists use this path.

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NEWS AND COMMENT

Philips: International Company Cultivates Basic Research

Eindhoven. A sharp commercial sense and a readiness to look to new horizons have helped the Dutch to overcome the limitations of a small country meagerly endowed with natural resources. With such international enterprises as Shell, Unilever, and Philips, the Dutch anticipated the European Economic Community's quest for a bigger market. And Philips provides an example of an advanced-technology industry with a research effort which is highly institutionalized and big by any standard.

Philips, which celebrated a 75th corporate anniversary last year, was started by Gerard Philips, a Dutch engineer who finished his studies at the polytechnical school at Delft just as the arc light and carbon-filament lamp were making electric lighting a commercial possibility. In 1891 Philips established a lamp factory at Eindhoven. The young entrepreneur at first carried out the necessary research himself and later started a chemical laboratory within the Eindhoven factory. By 1914, however, he had decided there was need for a physical research laboratory outside the factory; he established one, and employed a young physicist from Leyden University, G. Holst, to run it.

In the 1920's this laboratory's research in gas discharges led to early Philips success in bringing onto the market sodium-gas-discharge lamps for road lighting and, later, high-pressure mercury lamps. The same interest aided Philips development of x-ray tubes and radio tubes. Philips engineers and scientists were quick to grasp the commercial potential of radio.

The laboratory continues to follow Holst's original lead and concentrates on basic research rather than development. But, at the same time, research follows lines which offer hope of ultimate applications—that is, products for the company. The unexpected sometimes happens. In the 1930's, synthesis of vitamin D for livestock led to the establishment of a flourishing pharmaceutical products branch, which now has its own separate research arm.

During World War II, Philips facilities in Europe were caught in the German occupation. After the war came a great expansion for Philips into the fields of television, telecommunications, and electronic systems and a corresponding expansion of the research organization, including the growth of major research laboratories in Britain, France, and Germany and a smaller one in Belgium. The main fields of research, according to one executive, are those concerned with materials, devices, electron physics, x-ray and nuclear equipment, and electronic systems. The research laboratories at Eindhoven remain by far the largest, employing 2200 people, 400 of them holders of master's or doctor's degrees. (The Dutch master's degree in science or engineering requires 6 years of university work, and the doctoral degree, about 9.)

These "graduates," as the Dutch call them, are the object of personnel policies which are at the heart of the Philips system. Young men are brought into the company at the age of about 30, when they have received their degrees. After 7 or 8 years in the research labs, by a process which apparently includes a strong element of self-selection, decisions are made which in most cases set the direction of the individual's career. Some remain with the laboratories. Others go to the operating divisions or to the development labs attached to the factories. Others may return to the universities; a fair number of Philips labs alumni are professors.

Departure from the laboratories is no disgrace, nor does it blight a man's career. Chances of preferment—of becoming a senior scientist or director—are said to be somewhat better in the factories. The company has a big stake in seeing that posts throughout the company are manned with good, technically trained men, and assignments and rewards are said to be under constant review, so that opportunities inside and outside the research labs will be kept on a par.

The whole system, from the original selection on, is a highly personal one. Holland is a small country, and close contact is maintained with faculty in the ten institutions—universities and technical universities—which produce the candidates. Representatives from the laboratories visit the universities each year, and young men completing their studies are discussed. Prime prospects are invited to the labs, and the visits are taken very seriously. The lab's deputy director for chemistry, for example, last year saw 90 prospects; nine of them ultimately joined the labs. The visitors also go to the company's central per-