equilibrium, but goes on forever. But if we look only at the total populations of the various states and ignore the identities of the molecules which compose those populations, we see a smooth, efficient, and inexorable drive to a determinate equilibrium condition. If the initial conditions are such that more of the molecules are in A states and fewer are in B states than is the case at equilibrium, what we see at the macroscopic level is the chemical reaction $A \rightarrow B$.

A very serious question which is begged rather than answered by the stochastic model is: What are the real origins of the transition probabilities per unit time? Once we assume them to exist, then Eqs. 1, and the irreversible approach to equilibrium which is implied, are immediate consequences; yet the system is composed of interacting molecules which satisfy time-reversible dynamical laws, so it is far from obvious how there could have arisen the fundamental distinction between past and future which is implied by the approach to equilibrium. While many aspects of this phenomenon are now understood, the general question, in one or another of its guises, is one of the continually recurrent problems of modern science (*10*).

References and Notes

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7. B. Widom, *Advan. Chem. Phys.* **5**, 353 (1963).
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9. Explicit formulas are known (see *3*, *6*, and *7*) for the differences $k_{eq} - k$, $k_{eq} - s$, $k_{eq} - r$. The first two of these differences, in particular, depend on how rapidly reactivity varies from one reactant state to another—more specifically, on the mean square deviation from the mean of the reactivity. As yet, this variation of the reactivity is not well enough known to allow any really quantitative statements to be made about the extent to which the constants k_{eq}, k, and s differ from each other. The best guess at the moment is that they do differ substantially, though all are of the same order of magnitude.
10. A beautiful and lucid account of this problem has been given by G. E. Uhlenbeck, in *Plenarvorträge der Physikertagung Düsseldorf 1964 (im Auftrag der deutschen physikalischen Gesellschaft)*, K. Hecht, Ed. (Univ. of Cologne, Cologne, 1964).

Megaloscience

Because of massive organization and large budgets, scientists are heavily involved with governments.

J. B. Adams

I have chosen the title "Megaloscience" for this discussion of scientific research and its interaction with governments and universities in order to convey the impression of very large-scale scientific research with just a hint of underlying mania.

Scientists who have grown up with this activity and who are still involved in it cannot pretend to be unbiased, but we can try as objectively as possible to analyze the problems which our activities have raised and to find reasonable solutions to them. We must address our minds to these problems now, if only because governments have become very much concerned with scientific research. Partly their concern is due to the rising cost of research and partly it is due to a growing realization in political circles that scientific research and development are the mainspring of our type of civilization. This concern must ultimately lead to decisions being taken by governments, and if we are to take an effective part in the decision-making we must first clear our own minds.

Even if our thinking does no more than dispel that public image of scientific research so well summed up by Academician Artsimovich, "Scientific research is a method of satisfying private curiosity at the public expense," it will not have been in vain.

Limiting Scientific Research Budgets

To the man in the street the impressive thing about megaloscience is its apparently insatiable demand for money. Where it all goes and how it is used is a mystery to most people. What results come out are by and large incomprehensible to almost everybody, including even scientists in other fields of research.

To the astute civil servant a far more ominous characteristic is the growth rate of scientific activity. Ever since the 17th century, we are told, the number of scientists has doubled every 15 years and the cost of scientific research has doubled every 5 years. We should not, of course, accept these statements without some investigation, particularly on such points as the definition of scientist used in the statistics, but during my own professional lifetime these doubling times seem to be about right. Extrapolation of these growth rates gives the fascinating and unlikely result that all the national incomes of our countries will be spent on scientific research in the year 2000 and everybody will be scientists a few decades later. Clearly, between now and the year 2000 something must occur to limit the growth of scientific research, and our problem is to determine what the limit should be and how it can be reached without unstable oscillations.

Such figures as exist show that in countries such as the United States and

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Britain about 2½ percent of the gross national income is being spent on civil research and development. About a tenth of this, that is, ¼ percent of the total, is spent on research and the rest on development. These may appear rather small percentages compared with what is spent on seemingly trivial things such as alcoholic drink and tobacco, which between them account for nearly 10 percent, but unfortunately, if people want to spend 40 times as much on smoking and drinking as on scientific research, there seems to be very little that anybody can do to stop them. As we all know, a short life and a gay one still has its attractions, even to physicists. In my own country only about one quarter of the national income is directly spent by the government, and that goes on such items as military defense, national insurances, and other public services. If we are to determine some limit for scientific research expenditure it is probably more profitable to consider what governments do with their money than what the people at large do with theirs.

Now some of the larger countries spend as much as 10 percent of their incomes on military defense, and as a starting point it does not seem unreasonable to imagine that they could attain the same percentage for civil research and development. If the same fraction of this goes to scientific research as at present, namely one tenth, then the upper limit for scientific research would be 1 percent. Since we are currently spending ¼ percent and the doubling time is 5 years, it would only take another 10 years to reach this limit. But if we are to avoid oscillation we must approach the limit asymptotically by means of an S-shaped or logistic curve, and the exponential rise must stop at the halfway mark. In other words, we must arrest the exponential growth in 5 years' time, when the expenditures will have reached ½ percent of the national income, in order to approach the 1-percent level smoothly.

This is a very simple and perhaps naïve example, but it yields an important result, namely that if the limit is 1 percent and we want to avoid uncomfortable, if not disastrous, oscillations, we must take action in the next 5 years to stop the exponential growth of scientific research budgets. Even if the limit is 2 percent, we can only delay decisions another 5 years, and if it is less than 1 percent we must act very soon. All this is the result of the very fast growth rate of scientific budgets and it is the reason why I said earlier that we must think about these problems now.

The percentage figures I have been quoting come from published government statistics, but as we all know, there is considerable confusion in the definitions of the various forms of scientific activity, and certainly in my own country I doubt whether our present figures are a sufficiently reliable basis for action. For example, what is called scientific research, as distinct from development, is very ill-defined and differs markedly among the sciences. Also, I know of no justification for the present 1-to-10 ratio between research and development or whether this ratio should be perpetuated in the next two decades. In fact we know far too little about the whole matter, and it will take a year or so to gather reliable statistics, even given government support for national surveys. To decide on such a serious matter without these facts is surely unthinkable, at least for scientists.

Just in case my example strikes terror in the hearts of the military, I should add that the figures I have been using of 1 or even 2 percent of the national income for scientific research could easily be reached by steadily allocating, year by year, a small fraction of the normal annual increase in national incomes which most developed countries now enjoy—for example, the American gross national product is increasing at 4 percent per annum. Thus we do not need to abandon military defense in order to find money for scientific research, although if peace broke out it would be a way of absorbing military research-and-development potential into the economy.

The Organizational Scientist

Let me turn from these weighty matters and divert your attention for a moment to another remarkable aspect of megaloscience. I refer to group activity and multiple authorship of papers in scientific journals. This is particularly noticeable in the leading megaloscience of high-energy nuclear physics research, where the motto seems to be "United we publish, divided we languish." It is not only that papers have many authors but that the authors of a single paper come from many laboratories. For example, in one of the September 1964 issues of *Physical Review Letters* there are two papers, one from Brookhaven on the Ω^- hyperon with 31 authors, and the other from the European Center for Nuclear Research (CERN) on π^- meson interactions with nuclei, with 25 authors from 6 different laboratories in 5 different countries. I notice that one author of the CERN paper, the work for which was done in Geneva, explains in a footnote that his affiliation is Berkeley, California, although he is actually on leave of absence from Milan University.

Those of us who work in large laboratories know that the authors listed on a paper are by no means the only people involved in the work. The ratio of research physicists to total laboratory staff is about 1 to 7, so a piece of research with 31 authors involves on the average something like 200 people in the laboratory, and the whole effort costs the laboratory about £1 million a year. Usually what one gets for this large investment of men and money is just another small piece of a vast jigsaw. Of course one tries to plan the research so that it is a vital piece, but one cannot always be successful, and sometimes someone else puts the piece down first. Very often the vital pieces turn out to be cheaper ones and the stroke of genius which first delineates the whole pattern is usually the cheapest act of all. Nevertheless, without enough of the jigsaw pieces it is beyond even a genius to see the pattern, and so we must go on prising them out of nature, each one costing more than the last. I must emphasize that megaloscience is not different from other science in this respect; it is only that it is further along the exponential growth curve, where bits of information apparently cost more. How long we can afford to go on collecting them while waiting for a pattern to emerge is another question.

I have remarked earlier that the number of scientists has apparently been doubling every 15 years, ever since the beginning of modern science. I doubt whether the number of scientists of, say, the caliber of Newton, Einstein, Schrödinger, Rutherford, and Fermi is increasing at this rate, and if the growth of research budgets were dependent only on men of such high ability and deep insight, it is unlikely that the doubling period of 5 years in research expenditure could have been maintained in the last few decades. What seems to have happened is that megaloscience has maintained the growth rate in recent years, first by becoming highly organized, and second by making the maximum use of whatever genius naturally arises in any decade.

In fact two distinct types of scientist have emerged in this process of scientific evolution, the Manager Scientist and the Pilgrim Scientist. Whereas the Manager Scientist spends a great deal of his time in his own laboratory, the Pilgrim Scientist is rarely to be found at home. While the Manager Scientist is responsible for large groups of people and for large laboratories and is familiar with the ways of governments and treasuries, the Pilgrim Scientist eschews all such contacts and responsibilities. Indeed, he is more in line with the popular image of a scientist, and he goes around fertilizing research in many laboratories. The Manager Scientist is mainly a postwar phenomenon, although some existed before. His job is to create the conditions in which good research can be carried out, and his reward is seeing it flourish about him.

I have used the term Pilgrim Scientist because it suggests a parallel with medieval times. The medieval pilgrim had a definite itinerary—certain holy places and religious houses to visit on his pilgrimage—and his itinerary depended on whether he was a Franciscan or Dominican or belonged to some other order. He was also the bearer of news, religious and otherwise, as we can read in Chaucer. The modern pilgrim scientist also has his shrines and religious houses to visit, depending on his branch of research. In high-energy nuclear physics, for example, the equivalents of the old religious houses are Berkeley, Brookhaven, CERN, and Dubna. It is as rare nowadays to find a scientist attaining pilgrim status in more than one research field as it was to find a medieval pilgrim belonging to more than one order. And just as it was customary for the medieval pilgrim to be fed, housed, and looked after by the monasteries, so the modern research laboratory must set aside funds to pay foreign pilgrim scientists and to send its own on tour.

Perhaps in medieval times there was a problem with pilgrims settling down in particularly attractive monasteries. Nowadays any pilgrim scientist who is captured more or less permanently in a foreign laboratory is said to be part of a national Brain Drain. Luckily, drains were far less common in medieval times, so no doubt the medieval pilgrims were mercifully saved from that simile.

Of course scientists have always traveled around. For example, right at the beginning of modern science there was the case of Tycho de Brahe, the Danish astronomer. He traveled quite extensively in Europe and at one time planned to settle in Basle, where he found the scientific community most congenial. This did not please the authorities back at home and finally Frederick II, King of Denmark, sent him a letter—it is dated 23 May 1576, a few years after New York Bay was discovered by Verrazano—which reads as follows:

> We, Frederick the Second, make known to all men, that we of our special favour and grace have conferred and granted in fee . . . to our beloved Tycho de Brahe, Otto's son . . . our land of Hveen, with all our tenants and servants who thereon live, with all rent and duty which comes from that . . . to use, hold, quit and free all the days of his life as long as he lives and likes to follow his studia mathematices.

The land of Hveen was an island of 2000 acres on which Tycho de Brahe built a castle and an observatory at Denmark's expense, and, what with the sinecures and grants, he became one of the richest men in Denmark.

You will observe that this letter contains all the ingredients to stop a Brain Drain: promise of money and staff and, above all, the personal touch in the letter of appointment—"our beloved Tycho de Brahe, Otto's son." You will not find that nowadays, not even in offers from American firms.

To Choose and How To Choose

Let me return again to money matters. The notion that there must be a limit to expenditure on scientific research naturally raises the problem of choosing among the different fields. We who are committed to the megalosciences must necessarily consider this problem very seriously indeed. Dr. Johnson once observed, "Depend on it, Sir, when a man knows he is to be hanged in a fortnight, it concentrates his mind wonderfully," and, indeed, a recent exchange of letters in *Physics Today* on this subject shows a power of concentration. All sorts of criteria for making choices have been put forward, such as scientific merit, technological merit, and social merit, as well as the degree of fundamentality of the research. National prestige is also clearly playing an important role in this matter, and so is international competition. We may yet find ourselves involved in the Pythagorean Games, as our athletic friends are now engaged in the Olympic Games. After all, the cost of the Tokyo Olympic Games is about the same as the cost of a 300-Gev accelerator laboratory for nuclear physics, and we already have our Gold Medals.

But before we get too involved in this matter, I think it is essential to be clear as to the motivations of scientific research. To my mind there are two basic motivations; one is the desire to do something and the other the desire to know something. The first is the motivation of applied research and development, and the second is the motivation of basic research. Because the motivations are different, the criteria for choice in these two types of scientific activity are different and should not be confused. To illustrate my point I can take an example from my own subject of plasma physics and fusion research. The motivation of the work of the Culham Laboratory is to see whether or not a controlled thermonuclear reactor can be built. In pursuing this aim we will of course learn a great deal about the plasma state of matter—in fact we must, if we are to make progress—but this is not the motivation of the work and it is not the reason why the British Government is spending £4 million a year on the Culham Laboratory. Such scientific activities as these must be judged on the basis of how successful they are in reaching their goals, and choices among them must be made on the basis of the values of the different goals to the sponsors at different times. It is quite conceivable that a laboratory such as Culham could have been motivated by a desire to know about the plasma state of matter. In this case it would fall into the basic research category and would be judged on a quite different basis from and in competition with the pursuit of other knowledge, such as that sought through research in high-energy nuclear physics or molecular biology.

Because the motivations of applied research and development are different from those of basic research, the two activities are not directly comparable, and lumping them both together in a single research-and-development budget has caused a great deal of confusion, particularly at the government level. In practice it is probably easier for a country to decide what it wants to do than what it wants to know. What I shall now discuss is the second of these two dilemmas, namely, how to choose between the basic scientific researches.

My starting point is simply that basic research, as I have defined it, is part of scientific education. It is the pursuit of new knowledge about nature, and the other two parts of education are the preservation of this knowledge and the handing of it on to future generations. My thesis is that the three parts must be held closely together at all times because, once the unity of education is destroyed, I fear that the whole system will slowly but surely deteriorate. For hundreds of years this unity has been preserved by our universities, but the advent of megaloscience and the creation of large basic research laboratories remote from the universities can easily disrupt it. It was to counteract this danger that the founders of CERN insisted that the research physicists using that laboratory must not be given permanent contracts, since these would encourage them to settle down at CERN and cut them off from their universities and from teaching. To this day very few research physicists have permanent contracts at CERN—just enough to guarantee the scientific management of the laboratory.

This concept of the unity of education can also give us a rough way of judging the extent to which the various basic researches should be supported by a country at any time. Suppose, for example, we first determine the number of university scientists actively engaged in the different fields of basic research and then calculate the amount of money needed per year to maintain a research scientist in each field at maximum efficiency. Obviously the cost per research scientist per annum is not the same in all research fields—it depends on the scale at which operations have to be conducted. At the megaloscience stage, for example in high-energy nuclear physics, it costs about £30,000 a year to maintain a research physicist efficiently, and it is rather a waste of money to maintain him otherwise. This figure is obtained by taking the total annual budget of a laboratory, such as CERN, and dividing it by the number of research physicists working in that laboratory. Other basic research fields not needing such large equipment cost less per scientist. The basic research budget is then composed by multiplying the cost per scientist by the number of active university scientists in each field, which gives the individual budgets for each research field, and then adding the lot together to give the total budget. At least this system of determining basic research budgets is constructive and avoids subjective judgments about the relative merit of the various research fields. Surely in trying to determine what a country should know it is safer to base the support on what its active research scientists find most challenging and worthwhile and to which they are prepared to devote their lives.

Sooner or later, of course, the total basic research budget calculated in this way will exceed the limit which I discussed earlier, and this is likely to happen first in the most developed countries. We must therefore consider what will happen in countries which have not yet reached this limit and which are making available less money for basic research than is calculated by the method I have just described.

The first reaction of an active research scientist who cannot obtain the necessary research facilities in his own country is to seek them elsewhere. Thus the first result of a financial limitation of basic research is the emigration of research scientists—a phenomenon with which we are only too familiar in Europe. A study of the pattern of scientific emigration can give clues as to what is wrong with the support for the basic researches. For example, if the emigration is confined to scientists in one field of research, it probably means an unbalance in the distribution of funds. If it covers all fields, then the total funds are probably inadequate in comparison with those provided by other countries. In my experience scientists do not emigrate for trivial reasons, and it takes several years of neglect to drive them that far. Thus the emigration figures are at best a very delayed manifestation of an unbalance.

The serious consequence of scientific emigration is not that a country cannot obtain the results of basic research, for they are all published and available to anybody. It is that fewer active scientists are available in the country to teach and inspire the next generation of scientists and the whole system of scientific education begins to run down. Hence my insistence on the importance of the unity of education. Also, since the best scientists can most easily find jobs abroad, the damage to the education system is far greater than the numbers emigrating indicate.

Clearly this emigration only continues so long as one country is further along the exponential curve of scientific expenditure than the others, and ever since the war the attractive country in this respect has been the United States. However, it is reasonable to suppose that that country will reach the limit of expenditure on scientific research first and so give the other countries the opportunity to catch up. In other words, scientific emigration need be only transitory if countries recognize its causes and try to reach the common limit as soon as possible. Nevertheless, in the megalosciences the absolute size of a country, and therefore the size of its investment in basic research, becomes important. For example, in high-energy physics the sheer size and cost of modern multi-Gev particle accelerators make it impossible for a small country to build them alone, however advanced that country may be in its support for science on a percentage basis. The solution in these cases is for a number of countries to combine together in a joint project, as was done in the case of CERN for high-energy physics. The advantage of CERN, quite apart from its contributions to physics, is that European high-energy nuclear physicists no longer have to emigrate to America in order to continue their research and hence they tend to remain in Europe as a vital part of its scientific education. Ultimately, as the cost of individual pieces of equipment in the megalosciences mounts, even the largest countries or groups of countries will be driven to unite if the research is to continue, and this is already being discussed for the 1000-Gev stage in high-energy physics.

It might be thought, and I have seen it proposed, that the smaller countries should use their limited resources for applied research and development and give up basic research, particularly at the megaloscience level. I think this notion is as dangerous as it is tempting to such countries. The active and original minds in science in these countries will not be satisfied with technology and applied research and will simply emigrate, thus reducing the standards of scientific education to a level where even the quality of applied scientists and development engineers may become inadequate for their tasks.

I have also heard the allocation of funds for basic research described as "dividing up the national cake." As I have tried to show, the method of determining research budgets should be additive, not divisive. Research budgets should be built up from the ingredients of research, which are the active scientists, and their proportions should be determined from what such people find most challenging and worthwhile in research. Also, basic research is not cake—it is bread and the staff of life of our type of civilization. We must get away from the idea that basic research is only a cultural activity or that its value to the community is some kind of "fallout" in technology and industrial processes. Of course our modern technology is a direct result of past basic research. The electronics industry is a result of J. J. Thompson's discovery of the electron, and the nuclear energy industry is a result of Rutherford's discovery of the nucleus. The cost of all the basic research that has ever been done is barely equal to the current year's increase in the gross national product of the larger countries, and without all that research it is doubtful whether they would now be enjoying any increases in prosperity. Nevertheless it is difficult to use such arguments for planning research expenditure in the future, however completely they justify research in the past. The true place of basic research is as a part of scientific education, and no industrial country these days can afford scientific illiteracy, whether it be in its universities, its industries, its government, or its people. We must therefore seek our guidance from this latter connection and merely accept the former as the natural consequence of enlightenment.

In Conclusion

Let me now try to draw together the threads of my discussion into some simple statements. I believe, for the reasons I have given, that we must be within a few years of the end of the exponential growth in scientific research which started in the time of Kepler, Galileo, and Newton and has been going on steadily during the last 400 years. Up till now this growth has been free and similar to the increase in populations which are not severely limited by food supplies or disease. I believe that we as scientists have an important part to play in the next most difficult phase in the growth of our subject, which is to bring the exponential phase smoothly toward a limit without oscillation or discord. We must use our skills as scientists on the growth of science itself.

I have mainly discussed basic scientific research as a vital part of the whole of scientific education. Even in the applied researches with definite goals which are supported by our countries because of these goals, we must continually examine our purposes. In nuclear fusion research, for example, we must be sure that nuclear fusion reactors remain worthwhile to the community and that we are making progress toward their realization. At the moment I think they are worthwhile and that we are making considerable progress, but if the time ever comes when their value is minimal and our progress questionable, I hope we will have the courage to speak out first and not wait until other people outside science find out and take appropriate action. As you know, the reason why populations do not continue to grow exponentially is that they either become diseased or exhaust their food supplies.

Megaloscience as the last phase in the long history of the growth of modern science has certainly brought a great number of problems for our generation, but it is only fair that I should end by briefly mentioning some of its less obvious blessings. Like technological fallout, they are perhaps incidental and were certainly not foreseen, but they have their importance.

Simply because it has grown so big, megaloscience has caused countries to act together in joint enterprises which, owing to their nonpolitical nature, have enabled methods of international behavior to be worked out far more quickly than has been possible in more controversial fields. CERN is a very good example of what international cooperation in scientific research can offer to small countries like the European states. The massive organization and large budgets of the megalosciences have brought scientists into headlong involvement with governments and treasuries, and although this interaction has not always been blissful, I think everybody has benefited from it. Also, the Manager Scientists and the Pilgrim Scientists have certainly opened up new channels of communication between the nations which even in nonscientific matters have remained remarkably direct and effective.

Perhaps future generations, looking back at our struggles with the growth rates of science and the limits, may well rate these incidental achievements as highly as our research results, and in terms of human welfare they may even find them to have been of greater significance.