UNITED STATES GOVERNMENT

DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE
OFFICE OF THE SECRETARY

Memorandum

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Building 1, Room 20

TO : William Gorham
Robert Grosse
Jeffrey Weiss

DATE: August 31, 1967

FROM : Richard Zeckhauser

SUBJECT : Future Growth of NIH

We do not believe that there now exists, or that there ever will exist, a formula which will set optimal growth rate for a scientific area. However, we do believe it is both possible and reasonable to compare different scientific areas to see whether one is <u>relatively</u> over-supported. It was in this spirit that we conducted our investigation. We believe that biomedical research should grow in a competitive manner with other areas of science.

Studying this field for a summer gives one some feel for the area. Our projections are in the nature of educated, but intuitive guesses.

<u>Extramural</u>: As best we can determine, the extramural program has grown too rapidly in the past. A growth rate of eight to ten percent in the near future would seem adequate. — on what basis?

<u>Intramural</u>: Most indications are that intramural research will grow somewhat slower than the extramural program. To keep its relative pace at an optimum, it should grow at a six to eight percent rate.

<u>Manpower Development</u>: NIH training and fellowship expenditures seem to be somewhat high at the present time. We see no reason why they should grow any faster than the current inflation rate per man produced, no more than four percent per annum.

<u>Collaborative</u>: The collaborative programs present a more difficult problem for projection. These programs can easily be held down in periods of tight funds, but can profitably expand when resources are plentiful. At present, we suspect there may be some serious misallocations within this area. However, we would not wish to make any specific recommendations without some cost/benefit analysis of current collaborative efforts. As the payoffs from these projects are relatively concrete, we would think that an informed Congress could do a good job in setting their appropriations. We emphasize once again our belief that developmental efforts should not compete for funds with programs to promote additive research.

also quality analysis

GPO : 1965 O—737-657

Construction: Over the long-run we would expect construction to grow at approximately the same rate as the extramural sector. We did not examine construction grants on an interim basis.

Review and Approval and Program Direction: This area should likely grow as the sum of intramural and extramural research.

Regional Medical Programs: We did not examine the Regional Medical Programs. Their growth should be treated independently of the growth of the rest of NIH. However, it may be useful to compare them on a cost/benefit basis with some NIH sponsored developmental efforts.

Ad additional word about the NIH approach to the growth question might be in order. NIH materials usually attempt to present growth as the residual after the removal of any inflationary factors. There are two misleading aspects to this approach. (1) They are usually referring to inflation in costs per investigator. Part of this so-called inflation reflects changes in the conduct of the science (e.g., better instruction) and hopefully increased productivity. (2) We usually think that as the per unit cost of an item increases, we will purchase less units. NIH likes vertical demand curves.

or tackling harder + more fundamental problems

1967 APPROPRIATIONS (INCLUDING ENVIRONMENTAL, EXCLUDING MENTAL)
(millions)

Total NIH	$1,112
Grants	
Research Grants*	598
Regional Medical Programs	43
Research Fellowships*	51
Training Grants*	134
Direct Operations	229
Direct Research*	79
Collaborative Studies	110
Biologics Standards, Professional and Technical Assistance, Training, Computer Research and Technology	13
Review and Approval, Program Direction*	28
Construction Grants	56

Total with * = $946 million

		Research Grants and Construction	Fellowships and Training	Direct Research	Review and Approval, Program Direction
Growth Rate	High	10	4	8	9.8
	Low	8	3	6	7.8
	% of Total with *	61.9	19.8	8.3	3.0

High growth rate of programs with * 8.65%
Low growth rate of programs with * 6.85%

NIH PROJECT

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Disclaimer

At the outset, we best mention the limitations of this report. Data in the medical research area is difficult to find and hard to employ. Throughout this report, we use sample or representative data. We often find ourselves in a position from which we must extrapolate from limited, at best suggestive, information. For example, there is no unambiguous measure of quality for medical researchers. To handle this topic, we had to turn to surrogate indicators. One such indicator was rates of "approval for scientific merit" of projects submitted to NIH.

No one would claim that the results we present should be regarded unquestionably. No doubt, some are misleading, and others may be completely wrong. However, we should state that we entered this study without prejudice and presented as best we could our honest appraisal of NIH and its programs. We believe that the primary thrust of the inferences derived from this study would not differ greatly from that derived from a much more comprehensive investigation of the biomedical research area.

Orientation

In his August 1966 speech at the National Institutes of Health, Secretary Gardner identified critical issues relating to Federal support for biomedical science.[1/]

> First, has there been a major change in the basis and nature of the federal relationship to fundamental research, graduate training, and expansion of the academic and scientific resources of this country? Has there been a major policy decision to shift resources from the support of the individual scientists on a long-term basis to directed, short-term, research programs aimed at specific targets and to application?
>
> The second critical issue that must be dealt with is considerably more difficult. What are the essential conditions for maintaining, and the rate of growth needed to maintain, a healthy fundamental science component in the fields related to health and medicine? What rate of increase in funding can the scientific community expect? What constitutes stable support?
>
> The third critical issue is: How should one view the allocation of resources among (i) basic research, (ii) applied research, and (iii) application of knowledge in a health services setting?[2/]

This report is directed to these issues.

1/An address given in Bethesda, Maryland, 23 August 1966, to the Consultants of the National Institutes of Health. Reprinted in Science, Volume 153, Number 3744, 30 September 1966, pp. 1601-3.

2/In his address, Secretary Gardner identified a fourth issue which we consider only in passing.

> The final issue to be dealt with is this: How will the scientific and university community be affected by the growing government interest in delivery of health services?[2/]

Cost/Benefit Analysis

The national expenditure for medical research in 1967 will be approximately $2.275 billion. This is about $11.50 for every man, woman, and child in the United States. The per capita figure seems almost insignificant if we think of the benefits derived from such recent medical innovations as polio vaccines, birth control pills, and tranquilizers. Only a few significant medical discoveries per decade would seem to justify our current level of expenditure. This does not mean, however, that the dollars we spend on medical research are well spent.

For example, with a change in emphasis or direction in our medical research programs we might be able to garner much more medically useful knowledge while our current level of expenditure. Or, it may be the case that a small fraction of our current expenditure produces the great bulk of useful information, and the productivity of what economists would refer to as marginal expenditure may be minimal.

We also should point out that a straight dollar measure of inputs to the biomedical area will not give a true indication of the cost to society. There are presently nearly 70,000 professional workers engaged in biomedical research, many of them talented and highly trained scientists. The pool of capable scientists in this country is a vital national resource. We will argue below that the salaries of these individuals are not a true representation of the opportunity cost of employing them in a specified field.

The biomedical research area is also distinguished by the fact that nearly two-thirds of its total support comes from the Federal Government. It is an unfortunate fact, but true, that the Government cannot support every project whose dollar return is positive. Government dollars must do double and triple duty.[1/] In judging the appropriate level of expenditure for biomedical research we must look not only to the returns to be derived from our efforts, but also the returns from rival projects which compete for funds with biomedical research.

In this report, we make a continual effort to employ the alternatives foregone, opportunity cost approach to allocation problems. In many ways this apparently complex treatment actually simplifies our task. We do not consider for example such questions as how much money we as a Nation should spend on biomedical research, or for that matter on scientific research in general. Rather, we look at the question, how can we tell whether biomedicine is receiving sufficient support within a particular program for support of the sciences.

It is not solely the intellectual appeal and validity of the opportunity cost approach which dictate its use. We were also in a position in which we could not find suitable measures which would enable us to develop a simple benefit/cost analysis (see Appendix B).

There are sectors within the NIH program which are not too poorly adapted to cost/benefit analysis. Given a large margin of error, the costs and payoffs of developmental programs and those aimed at the delivery of health services can be estimated. The payoffs are of three varieties: (1) dollars saved in treatment, (2) decreased morbidity, (3) decreased mortality. It is difficult to give dollar values to the last two payoffs; however, DHEW has made past efforts in this direction.

1/For example, the Disease Control Memorandum prepared by this office dated November 15, 1966, lists 14 programs ranging from motorcycle helmets to tuberculosis with benefit/cost ratios of 4.4 and higher. It seems likely that some of these programs will not be undertaken.

Cost/benefit calculations would be a useful tool in comparing these projects against each other. There is little evidence of past comparisons, particularly between the developmental and delivery areas.

Manpower as a Resource Constraint--An Economist's Argument

Many analysts assert that brains, and not money, are the scarce resource which must be allocated to national research efforts. The assumption inherent in this argument is that a scientist's income does not adequately reflect his marginal productivity to society. This assumption might be justified on either of two grounds. (1) Scientific knowledge is a public good and as such is not totally appropriable by its producer. Therefore, the market demand price for scientists will not reflect their total value to society. This justification would not be relevant if Government support created an optimal level of demand for scientists.[1] (2) The market for scientists is imperfect in that salary differentials do not adequately reflect differences in scientific capability.

If marginal scientists are just paid their marginal value, then society is reaping a substantial surplus from its most capable scientists. There are, no doubt, some scientists whose value to society may well be in excess of a million dollars a year, who receive as income only a small fraction of that amount. If we lure them into a new field with a salary increase, the cost is not the new ~~salary~~ salaries but rather the opportunity cost resulting from taking them from old fields.

At the present time, we do not pretend to have the knowledge which would enable us to carry out an analysis comparing the productivity of scientists in different fields. If scientific research were supported

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1/In determining the optimal level of demand for scientists, the Government should account the pecuniary diseconomies it receives as it expands the field. That is, the Government should consider only the salaries it must pay to attract additional people to the scientific field. It should not consider the inflation of the salaries of individuals already resident in the field. This inflation is merely a transfer of wealth, and is not of relevance to efficiency considerations. (Cost/benefit analyses often mishandle the concept of pecuniary diseconomies.)

by the market mechanism, we could let the market carry out the allocation. For reasons outlined above, the market cannot handle the task. However, we might expect that a rudimentary form might be helpful. For example, we might agree that all scientists of a given level of capability should be supported by the Government. We could then leave it up to the scientists themselves to choose their field of endeavor. If there were a new fields (oceanography is a recent example) which had not received sufficient emphasis in the past, it would be reasonable for the Government to give it additional support in the hope that it would in a short period of time be able to generate a self-sustaining intellectual atmosphere which could attract talented scientists in the future. However, the basic Government position would be one in which the scientists would be free to choose their own fields. The basis of this procedure of course is that scientists can choose profitable fields well and that scientific profitability to some extent goes hand in hand with desirability to society.

...

The past 20 years has witnessed a most remarkable growth in the biomedical profession as the statistics presented in this report show. Biomedical science is a thriving area at present, and in that the words of that oft-cited phrase, it has reached its maturity. We would expect therefore that Government support for biomedical science should put it on roughly competing grounds with other fields of scientific endeavor. In the next section, we present some historical and current data relating to support for biomedicine and other scientific fields.

Research: Applied/Basic and Other Distinctions

In recent years, the distinction between applied and basic research has received a great deal of attention, and has resulted in no small amount of confusion. It is debatable whether such classifications mean very much in the biomedical sciences. Alvin Weinberg, a scientist and administrator, has written:

> The dogma of protein synthesis--DNA, messenger RNA, transfer RNA, protein--seems to be valid in almost every life form. The same 20-odd amino acids build proteins in bacteria, in mice, and in men. This unity suggests that most of what we learn about biological mechanisms in almost any animal is likely to have ultimate medical applications, whereas the same degree of relevance to application cannot be claimed for large parts of modern physics, or astronomy, or mathematics. In the biomedical sciences the distinction between pure and applied is rather irrelevant.

Operationally, applied research is often defined in terms of the objectives of the person performing it. But this engenders some ambiguity, for a scientist may be working on a project which he regards as basic--i.e., done only to satisfy scientific curiosity--while the administrator, in the context of the total research effort, sees it as mission-oriented. In such a case, we can talk about mission-oriented basic research and avoid confusion.

We have developed a concept of research which attempts to avoid the ambiguity of the applied/basic distinction. Possibly, it will pose new problems of its own. The economic approach is to look at the type and time distribution as well as the quantity of payoffs from a research project. One project, for example, might look at the structure and properties of cellular proteins. Such a study, if successful, would add to our knowledge and understanding of the life process--perhaps in a very fundamental way. Such research is termed "additive" because its primary value is that it will add to basic scientific knowledge and provide a basis for future research. A typical "non-additive" research project would be an investigation into the best surgical treatment of breast cancer. Research of this variety may save many lives, but it is not likely to add much to our understanding of scientific questions.

In general, we would expect the payoffs from non-additive research to be concentrated in the near future. In contrast, a practical payoff from additive research may not be visible for many years or even decades. There are exceptions of course. Additive research may turn out to be of immense short run practical significance, as in the case of the research on nuclear fission in 1938.

There is a difference between non-additive and focused research. Research is focused to the extent that it looks at a particular problem which is also an institutional goal. While non-additive is generally more focused than additive research, some additive research is highly focused on fundamental problems, the solutions of which are important goals.

Our analysis suggests that comparisons between different additive research projects can be made primarily on the basis of scientific merit. Their probable benefits lie in contributions to scientific knowledge, the material benefits of which are difficult to estimate and will hopefully continue for many years in the future. Non-additive research can be evaluated in terms of potential practical benefits. Any comparison between additive and non-additive research, however, will be difficult or impossible to make because the nature of their benefits differ greatly. Such a comparison would involve estimating the benefits from a certain domain of scientific knowledge, the contribution of a given piece of research to that knowledge and an appropriate rate of discount. It is hard to see how the requisite estimates could be made in a meaningful way. Our major conclusion, reached also by Weinberg for applied and basic research as defined by him, is that additive and non-additive research should not compete for funds; they are simply not comparable under ordinary circumstances.[1/]

1/In time of war, it may seem obvious that the welfare of future generations depends upon current welfare--that is, military victory. This sense of national urgency results in a high discount rate, and everyone does applied research.

Current Characteristics

The biomedical research establishment has grown from a $45 million level in 1940 to $2.275 billion in 1967. This tremendous rate of growth reflects, to a considerable extent, the increasing Federal involvement in the field. Federal support, of negligible importance to the field in 1940, is the main force in the biomedical area today, eclipsing industry, State and local government, and the private foundations. NIH (excluding mental health) provides 55% of Federal funds for biomedical research. Appendix A gives detailed basic data on NIH and on biomedical research.

Scale of Research

Although the biomedical area has entered the period of big funds, it has remained in the province of "little science." Most research is done by individuals or small teams. For fiscal year 1965 the median size of a research grant was only $20,328, and only 5.2% of their grants were over $100,000 (although they made up about one third of the dollars awarded) Since 1955, the size of an average NIH grant has risen steeply from $11,000 to over $40,000*, but the scale of research projects remains small in comparison to premier "big science" fields like astronomy and experimental physics. Current thinking about the problems and potential of "big science" in biomedicine is summarized in <u>The Advancement of</u>

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*In part this reflects a trend toward the practice of combining two or more research efforts into single research proposals.

Knowledge for the Nation's Health.**

"It is generally agreed that many areas of medical science, given sufficient manpower, facilities, and operating funds, are amenable to such an approach. For example, bioengineering, tapping the bast potential of industry, could be applied much more extensively in the development of artificial organs, diagnostic instruments, and patient monitoring systems Such research is highly expensive and makes use of wholly new skills. Consequently, it is not a substitute for the present mix of fundamental and applied research which characterizes the present scene."

The best approximations of "big science" now being performed in the biomedical area are the NIH collaborative programs. Cancer chemotherapy, the oldest and best known of these programs, currently expends $30 million a year to screen thousands of chemical compounds for effective activity against cancer. The related Etiology collaborative program spends a like amount investigating the causes and prevention of cancer. The relatively more modest Artificial Heart-Myocardial Infarction project spends roughly one quarter the total of the two major cancer programs. It is somewhat misleading to label these efforts big science. They are centrally administered, but conducted on a decentralized basis under numerous small individual contracts.

** A Report to the President on the Research Programs of the National Institutes of Health, p. 292, November 1966.

The Federal Role

The government needs to support additive research because scientific knowledge is a public good. The market system will not produce a nearly sufficient amount. However, the close relationship with the production of goods, together with patent protection make non-additive, developmental efforts considerably less public in nature. This would seem to offer the argument that the government, in contrast to industry, should concentrate on building a base of scientific knowledge and do relatively little in an applied or developmental way. Past government support in the biomedical sciences has been primarily for additive research.

Unfortunately, the present structure of the biomedical research field is not well-suited to commercial developmental efforts (We discuss this further in our section on Collaborative Research.).* The result is that as a Nation we have made insufficient efforts in the past to bring medical knowledge to the developmental stage. This area must now be given Federal support.

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*The drug industry is an outstanding exception. The following quotes are from the highly sympathetic report of the American Medical Association Commission on Research.

> "Most of the research conducted by drug companies is applied research although increasingly drug companies have had to expand into areas of fundamental research. The commitment of drug companies to medical research has been increasing annually. . .
>
> "From 1959 to 1965, the research and development staffs of drug companies were increased from 11,400 to 16,400, a rise of approximately 50%. An additional increase of 2,500 was projected for the end of 1968. . ."
>
> "Most of the specific medical advances of recent years have been in the chemotherapeutic area. These have come in the main from the drug industry. Although academic contributions have had their place in this innovational process, the ultimate development of drugs and the bringing of them to the public depended largely upon the exercise of industrial creativity and know-how."

Study Sections

The study section system has functioned effectively in the past because research funds have been liberally available. It now seems evident that this bountiful situation may not be maintained. Indeed, the last few years have seen a slowing in the growth of funds, and a reduction in the percentage of approved projects which are funded.

NIH RESEARCH GRANT AWARDS AND NATIONAL ADVISORY COUNCIL APPROVALS† 1963-1967

Council Year	Council Approvals		Institute Awarded		Funding Ratios: Ratio of Institute Awarded to Council Approved*	
	Number	Dollars	Number	Dollars	Number	Dollars
1963	6,396	172,608,334	5,959	157,733,532	97.4	95.6
1964	6,279	163,251,461	5,882	150,437,541	97.4	93.9
1965	6,444	180,171,207	5,536	155,620,800	90.4	90.1
1966	6,544	200,287,778	4,881	153,576,562	84.2	85.9
1967	6,431	224,674,428	4,560	149,137,288	93.3	94.3

may not prove full effort - prob. includes non-competitive renewals

	Withdrawn After Council Approvals		Pending Institute Action	
	Number	Dollars Approved by Council	Number	Dollars Approved by Council
1963	278	7,572,288		
1964	135	2,982,808		
1965	302	6,134,984	15	1,254,604
1966	575	12,849,615	175	8,668,837
1967	454	10,501,512	1,088	55,997,357

*Awards as a percent of council approvals less approvals on withdrawn applications and applications for which final action has not yet been taken by the Institute.

NOTE: The ratio figures for 1967 may be misleading in that there is a large quantity of pending proposals.

In a period when the overwhelming majority of approved projects are funded, the primary function of the study section is to approve and disapprove projects. With the exception of marginal cases, the scores of approved projects are of no relevance. However, during a period of tight funding, the scores play a significant role. As in most distributions, the scores are densest around the median numbers. Thus, for example, if approximately one-half of the approved projects were funded we would expect to find the maximum number of projects with scores within a few points of the cutoff. Such a situation would be likely to lead to unhealthy competition between study sections, each attempting to improve its share of funded projects by giving better and better priority scores to its approved projects.

The table below gives some hint that the competition we fear may be developing.

TABLE

Mean Study Section Priority Scores
(A Lower number means higher priority)

Aug-Oct '64	Dec '64-Feb. '65	Aug.-Nov. '65	Nov.-Feb. '66	Mar.-June '66	Aug.-Nov. '66
258	255	251	243	241	239

First two numbers for PHS. From August 1965 on for NIH only. Difference between averages is always very small.
for PHS and NIH

We should note that these priority scores were improving during a period in which approval rates were falling. Thus, the argument that projects were on the whole improving seems doubtful.

The study section system faces another danger during a period of tight funding. Some worthwhile research areas may not be investigated. This will be particularly true if different study sections grade at different levels of difficulty. The correlation between NIH study section scores for August-October 1965 and August-October 1966 was .79. For November 1955-February 1966 and March-June 1966, the correlation was .68. The sample of study section means shown in the table below shows the consistency of high or low scoring which we mention.

SAMPLE NIH STUDY SECTION PRIORITY SCORES **

	Nov.'65-Feb.66		Mar.-June '66		Aug.-Oct.'66	
Study Section	Score	Dev.	Score	Dev.	Score	Dev.
Overall Mean*	243	0	241	0	239	0
5. BBCB	235	-8	233	-8	227	-12
10. CBYA	237	-6	224	-17	246	+7
15. CVB	295	+52	276	+35	278	+39
20. EPB	249	+6	268	+27	251	+12
25. HEM	267	+24	255	+14	245	+6
30. MNHA	269	+26	273	+32	264	+25
35. NTN	244	+1	252	+11	243	+4
40. PMY	269	+26	260	+19	252	+13
45. SGYB	276	+33	300	+59	274	+35
50. VR	232	-11	245	+4	225	-14

* The average score for a project approved by a study section durint the time period, obtained by weighting the average study section priority scores.

** Data from Research Grant Applications Reviewed by PHS Study Sections and Committees, prepared by DATA PROCESSING SECTION, NIH, Bethesda, Maryland.

Even if all sections graded with equal difficulty, or if study section scores were normalized there will be the danger that minor fluctuations in cut-off points can significantly alter the balance of research programs within an institute.* Similarly, the Council may find it difficult in the short period it has for review and allocation to identify those projects which will complement or compete with each other. Funding difficulties may be further aggravated because Councils can commit funds from future years. This means that fluctuations in current funds create much more significant percentage flucuations in the funds available for new projects.

not in legal sense ?

NIH has been rather successful in its funding efforts over past years. Now that its program is larger, and resource constraints on the Federal budget more severe, funding shortages are more likely. We think that it may become necessary, and certainly will be desirable for the institutes to identify priority areas through their Advisory Councils.

Fortunately, NIH has already had some experience with this approach. A few years back, the Allergy and Infectious Diseases Institute was forced to function on a very limited budget. Prior commitments put them in a position where they could fund but few new projects. Straightforward

Dr. Colbert

*Advisory Councils are empowered to take program balance into consideration to decide the priority order for funding. In actual practice the Councils follow the study sections priority ratings in all but a few cases. Similarly, the Councils can allow for easy and hard study sections. Past scoring averages and like data are available at Council meetings. However, it is much simpler not to attempt to correct for this facor, and we fear the Councils usually follow the easy way.

allocations on a priority score basis would have awarded many grants in the easily graded tropical diseases area. To deal with this situation, NIAID developed their Special Emphasis Research Programs. They have described their programs as follows:

"In previous years the structures of limited research grant funds encouraged this Institute to develop, with the help of Council, the concept of "areas of high program relevance." This concept permitted the intrinsic scientific merit to be supplemented by a fiscal priority of payment when the grant application was judged to have high program relevance. The experience gained in developing program priorities and the current estimate regarding FY 68 funding now substantially lessens the importance of fiscal priority and stimulates the Institute to take an even more active programming position in certain areas."

Under the Special Emphasis Research Programs, the NIAID Council was empowered to swing two deciles in its funding of projects. We think the NIAID approach is a most promising one. We include an appendix on its procedures and programs.

In our section on projected future growth rates for extramural programs, we discuss an elaboration of their scheme.

Collaborative Programs

Probably the most significant recent development in the biomedical research field is the turn toward large scale developmental efforts. Most of this work is carried out in the collaborative programs on a contractual basis. NIH has administrative responsibility for these efforts which are directed toward specific goals.

Well known NIH collaborative projects include the cancer chemotherapy program, the development of the artificial kidney and the artificial heart, and the rubella vaccine development project.

This portion of the NIH budget has grown rapidly in recent years. It has moved from a $35.9 million level in 1962 to $91.5 million in 1966 and $109.7 in 1967 (the latter figure excludes mental health)

Three fundamental questions should be asked about the collaborative program:

1. Is the program well conceived?
2. Can NIH administer it successfully?
3. Are there organizations or institutions which have the capability to carry out the research?

I. NIH has summarized the prerequisites for a targeted research program:

> ". . . a clear demonstration that a solution for the problem is attainable (e.g., a vaccine can only be said to be attainable if it is known that the disease is caused by an infectious agent);
>
> ". . . a reasonable assurance that both the theoretical method and the physical means for working out the

solution are available;

". . . a rational set of procedures--or a pathway--
to the solution of the problem is discernible."

NIH has not been bold in discussing the possibilities for this form of research.

"These conditions can be met in a number of physical science problems that have a sound and fairly comprehensive theoretical base. They usually cannot be met in the majority of biological problems where this most essential of all conditions for large-scale targeted research is missing."

"In general, targeted research in the life sciences must be undertaken cautiously. It must proceed with the realization that the approach is substantially unproved and that caution is called for. If experience shows that too much caution has been exercised, or that success is fairly well predictable, an accelerated effort will be justified. Too ambitious a beginning or too ready an abandonment of the methods by which success has been achieved would be foolhardy. The result might well be the expenditure of valuable resources andscarce talen on an ill-founded presumption of success."

However, we should mention that administrators of collaborative programs at NIH have been much more optimistic in their predictions.

One of the disadvantages of much targeted research is that it tends to be of a non-additive nature.

Cancer Chemotherapy as an Example

NIH has commented on the cancer chemotherapy program: "These considerations, in view of the state of biomedical knowledge today, are major constraints on the eager development of targeted programs, most of which, for some time to come, are likely to remain a gamble with not very favorable odds. Indeed, the cancer chemotherapy program is such a gamble. Over the past decade, it has paid off with many useful drugs and much knowledge about drug action, but has not yielded, and may never yield, any basic insight into the nature of cancer leading to cancer control." Dr. Howard Hiatt has questioned the scientific basis which underlies this project:

> ". . . Indeed, what remains to be answered about all the agents presently is use is not why they do not cure, or why their therapeutic usefulness is not broader but, rather, why these drugs work at all, for, as I feel impelled to emphasize the quest for metabolic pathways peculiar to the cancer cell has been unsuccessful.*"

In the absence of fundamental knowledge, the chemotherapy project seems to be a shot in the dark--albeit a well-intentioned one.

Similarly, should we ever be so fortunate to be able to prevent or cure heart disease or kidney failure, we will render obsolete the artificial organ programs in those areas. Most of the dollars

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* New England Journal of Medicine, pp. 157-166, January 19, 1967

and effort that went into those programs will have been wasted in the research sense in that they will not be a base on which future research will build.

The additivity or non-additivity of a research program is not a basis on which it should be approved or rejected; it is merely a factor which influences its effectiveness. Not unrelated to the additivity concept is that of spinoffs.* Quite often a developmental research effort will yield information on techniques which may be applicable in other areas with different end goals. Thus, for example, knowledge we gain about the introduction of one artificial organ will assuredly have many uses when we attempt to develop another. Similarly, it is clear that there are striking similarities in the techniques used to develop vaccines. ~~Information gained while producing one might well be valuable when we attempt to produce another.~~

We would expect in general that the directed nature of collaborative programs would tend to minimize the potential for spinoffs.* However, this factor must be taken into consideration in any cost/benefit analysis of these programs. They are difficult to quantify, and are usually omitted.

2. II The Woolridge and Ruine reports were pessimistic about the ability of NIH to administer its large collaborative programs. The National

* It would seem for example that the cancer chemotherapy program would offer very little in the way of spinoffs.

Institutes of Health has been exremely fortunate in obtaining and developing a few outstanding program managers. However, the supply of experienced, competent program managers in NIH, and generally in the biomedical community, is scanty relative to the foreseeable needs, and steps should quickly be taken to make such ~~occupations more~~ attractive as to pay, challenge and responsibility, career opportunities, and professional status. A pattern of incentives of this type may activate suitable talents and interests now latent within the community." We have made no attempt to update the findings of these reports (the Ruina report is dated March 29, 1966). However, interviews with some of the individuals concerned with their preparation indicated that they were not familiar with any developments which led them to believe that NIH now has the then lacking managerial ability. We have some indication that all parties at NIH are not fully behind some of the collaborative efforts. This factor, combined with the salary limitations of the government pay scale make it all the more difficult to secure competent program managers for the collaborative efforts.

A principle finding of the Ruina Report was that: "If NIH is to be responsible for program of directed research or development, a strong management structure, distinct from the intramural research activity and from the mechanism for administering grants, should be established."

The current NIH collaborative programs are handled separately by the institutes. However, there does not exist any NIH-wide organization for dealing with collaborative programs. If, as has been emphasized in the past, NIH's primary difficulty in the extramural area is administration and planning, it would seem valuable to have cnetralized co-ordinating agency which could give advice and guidance developed from the experience of all collaborative effoets. For example, there are many problems peculiar to government industry relations which all collaborative programs are likely to encounter. How helpful it might be if there were an organization which could pool knowledge of past treatment of such problems.

Furthermore, we would suspect that in some cases there are scientific questions which interest two ormore collaborative programs; that there is a potential for spinoffs between collaborative programs. An example might relate to the development of power sources for artificial organs. The existence of such possibilities gives further reason for the development of a collaborative agency.

Finally, it is apparent that collaborative programs have been forced to become involved with institute politics. Some programs have done quite well by themselves. But on the whole, whatever the results we would consider this an unfortunate situation. A centralized collaborative organization might be able to reduce the sensitivity of collaborative efforts to political considerations.

We should emphasize and state explicitly that we do not believe that collaborative programs should be removed from the institutes. We are merely stating that it might be beneficial to develop an NIH agency which could bring together all NIH collaborative experience.

3. III At present, few industrial firms (excluding drug companies) have the capability to carry on high level medical R and D. In part this is a reflection of lack of past demand. In part, a reflection of the fact that the industrial environment is not one talented medical researchers find particularly congenial. Further, there is the fact that non-industrial research has been well-supported.

It is difficult to predict future trends in this area. Some people with whom we spoke have warned that large scale collaborative efforts could swallow up the resource talent currently dealing with less directed research efforts. Others have despaired of the possibility of developmental efforts attracting the talen which they will require.

At this point we might contrast the experience of biomedicine and chemistry. Outside of the drug field, there is little opportunity for the commercial exploitation of developmental research in the biomedical area. Thus, there is little incentive for industry to bring biomedical knowledge to the developmental stage. In chemistry, by contrast, there are industrial firms which regularly and profitably turn new knowledge into new products. Part of the success of the chemical industry may be explained by the size of its corporate units. They are sufficiently large to make it feasible and profitable for individual companies to carry on their own R and D projects.

Despite the lure of higher salaries, many, if not most, of our most talented and original chemists stay within the academic community. Their more workmanlike brethren, on the other hand, can take industrial positions in which they can bring new knowledge to the point of application.

At present biomedicine lacks this incentive structure and corporation organization. The unfortunate result may well be that unoriginal, but capable researchers are attempting to generate new knowledge when they could more profitably be employed carrying on developmental efforts.

Effectiveness of NIH Training Programs

A systematic evaluation of the effectivness of NIH training programs has never been done. Good statistics on the number of people supported on these programs are not available before 1963. A report entitled The Development of NIH Training Programs to Meet National Needs for Research and Teaching in the Biomedical Sciences is now undergoing final review at NIH and should be available shortly.

The National Institute of General Medical Sciences is the primary source of NIH support for training programs. Their experience has been summarized in an NIH report:

> To obtain this output of PH.D's it is necessary to provide support for approximately six times as many students as will receive the degree in any given year. Again, the data are relatively consistent over the short span in which information is available. . . It should be pointed out that this ratio of one Ph.D to six graduate students is considerably better than the national average in the biosciences which is one to ten; thus, the non-NIGMS ratio is 1 in 15. Thus, it appears that NIGMS support is twice as effective as other programs.

There are of course many factors which influence any effectiveness measure. For example, if many graduate students are not Ph.D candidates any Ph.D. support program (80% of NIGMS trainees are seeking PH.D) will look quite attractive on a Ph.D. per graduate student basis.

The chart below shows the relative performance of NIH supported graduate students as a function of the level of NIH support in the

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field.

FIELD	PH.D.'S TO NIH TRAINEES 1964-5	NIH PH.D.'S AS % OF TOTAL 1965	% OF TOTAL IN FIELD SUPPORTED BY NIH 1963	(3)/(4)
(1)	(2)	(3)	(4)	(5)
Anatomy	34	40.0	29	1.38
Biochemistry	80	27.6	36	.77
Biophysics	25	64.1	75	.85
Microbiology	91	40.4	31	1.30
Pathology	14	42.4	12	3.53
Pharmacology	47	59.5	55	1.09
Physiology	64	50.0	40	1.20
Biology	9	5.0	3	1.67
Genetics	28	32.6	35	.90
Nutrition	10	47.6	28	1.70

Column (5) gives some sort of effectivness measure for NIH programs. The chart indicates that NIH programs are most effective in those areas in which it supports the smallest percentage of the field. Put in the jargon of the economist, there appear to be diminishing returns to increasing support in a field. The five fields to which NIH gave the smallest percentage of total support had an average effectiveness rating of 1.92. The five fields which NIH supported the most heavily had an average rating of .96. In all cases a small percentage support field had a higher effectiveness rating than a large percentage support field.
Underlined fields are those in which NIH supported the greatest percentage of the field.

Interpretation can be that the five fields to which NIH contributed most heavily are those for which other support is also available.

INDIRECT TRAINEES WHO RECEIVED FINANCIAL SUPPORT
FROM NIH TRAINING GRANTS
FY 1961 - 1965

Fiscal Year	Total Number	Predoctoral 1/ Number	Postdoctoral 2/ Number
1961	12,473	7,528	4,945
1962	14,417	8,988	5,429
1963 3/	18,902	12,847	6,055
1964 3/	22,216	15,531	6,685
1965 3/	23,337	Not avail.	Not avail.

1/ Includes trainees at prebaccalaureate level.
2/ Includes trainees holding a doctoral degree and seeking another degree.
3/ Includes trainees supported under NIMH Undergraduate Training Grants. (NCI and NHI undergraduate training grants do not support trainees.) Data for previous years not available.

NOTE: This material was received by DHEW August 31, 1967. We did not have an opportunity to include the figures in our analysis.

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Quality of Personnel in the Biomedical Field

We were not able to make a direct evaluation of the quality of people in the biomedical research field. This section discusses those indirect measures which we could find.

From 1954 to 1967, the pool of professional manpower in biomedical research expanded from under 20,000 to approximately 65,000 individuals. Inevitably, the quality of such a flood of people moving into the biomedical area has been questioned. Such a question cannot, of course, be answered without a detailed investigation. All we can look for are indications one way or another; in which spirit, the following information may be of interest. Of 1964 college seniors majoring in biology, 93 percent of the top fifth of the class was going on to graduate school. This is not very surprising, what is striking is that more than 84 percent of the students in the next three decades were going on for graduate study, most of them under Government-sponsored programs.[1] The

[1] We might note a statistic which we cited in our section, Comparison with other Fields. "For those whose career field was the biosciences, 55 percent of 1961 college seniors of both sexes and two-thirds of the men were in graduate school in 1964."

figures suggest two hypotheses:

1. Little of the NIH support to institutions of higher learning trickles down to expand the biosciences on the undergraduate level. Indeed, many observers have made statements in support of this hypothesis.
2. It is extremely easy to go on for graduate study in the biosciences.

Another interesting statistic is the approval rate for research proposals reviewed by NIH. An approved project is simply one which has been judged to be of some scientific merit; if funds are available, it can be supported.* Since standards of scientific competence have probably changed little during the period of NIH's existence, it is possible to regard the approval rate as an indication of the quality of researchers, in a given year. As Table 6 illustrates, the approval rates for new NIH applications dropped sharply in the years 1957-65, from 73.7 percent to 51.5 percent.

Table 6
APPROVAL RATES FOR NEW NIH APPLICATIONS

	1957	1958	1959	1960	1961	1962	1963	1964	1965
NUMBERS	73.7	67.0	63.7	58.4	56.1	52.0	53.6	51.2	51.5%
AMOUNTS	68.0	53.3	52.1	48.7	44.8	48.0	44.7	42.7	43.7%

This trend suggests that the overall quality of biomedical researchers declined with the major influx of newly trained manpower in the late fifties and early sixties. However, such a conclusion would require a knowledge of how new researchers compared with older ones, with respect to approval rates, both now and in years past. Such information is not at present available, but could presumably be obtained by NIH from their data on applicants and project evaluations.

*NIH traditionally refuses to fund approved projects with priority scores which place them in the lowest decile of approved projects.

Age Distribution of Manpower

Most psychologists and scientists agree that creative scientific research is the domain of the young. The Special Subcommittee on Investigation of DHEW heard testimony to this effect:

> Dr. Kenneth Endicott, Director, National Cancer Institute, informed the subcommittee that in his opinion research was a young man's game. He stated that often a good investigator will commence to "run down" in his forties and become unproductive in his fifties. He further stated that this was one of the reasons why lifetime awards under the Research Career Program were considered not desirable and were terminated.

Many would disagree

Given such conclusions, the research career awards warrant some investigation. The value of such awards has been questioned by the Woolridge Committee, and subsequently by the Rogers subcommittee:

> The subcommittee made a check of career awards under this program and noted that 23 of 24 recipients were over 40 years of age at the time they received the award--3 of these awardees were over 50 years of age and 1 awardee was over 60 years of age. A check of career development awards under this program also was made, and it was noted that 32 of 47 recipients will be over 40 years of age at the completion of their first 5-year award. Moreover, as there are no age restrictions for grantees, it must be assumed that the age factor will not preclude the granting of follow-on awards for an additional 5 years to most, if not all, of these same grantees. Therefore, it appears these scientists were given awards to develop them into proficient researchers at a time of life when it is believed that their age is a deterrent to such proficiency.

Of the institutes reporting in 1960, only Mental Health had a mean age of grantees below 40 (see table II). The median ages were somewhat lower; but even if one restricts his attention to <u>initial</u> grantees, the lowest median age is 36. This figure appears to be a little high in view of the accepted notions about the connection between research productivity and age. We hasten to remark, however, that it is certainly not a cause for alarm, and may represent the best compromise available in the context of existing manpower in the biomedical sciences. More ought to be known, however, about NIH funding of investigators in various age groups, on which there is little or no data at this time. Are young investigators at a disadvantage in the competition for funds, perhaps because of a lack of experience in the art of grantsmanship, or perhaps because it is difficult for study sections to have confidence in their unproved capabilities?

The distribution of NIH support to faculty members of the medical schools, on the other hand, seems to favor the younger people. Table III shows that while only 14.4 percent of all professors have more than

Table II

Mean and Median Ages and Age Range
of Principal Investigators, by Institute
(in Years)

Item	All Institutes	Institute A	B	C	D	E	G	H	M
Initial and Repeat Grantees									
Mean age	40	40	40	42	43	42	41	41	39
Median age	39	38	37	39	40	39	38	39	37
Range									
Youngest	25	26	28	27	27	25	26	25	26
Oldest	81	81	68	70	66	80	73	67	69
Initial Grantees									
Mean age	39	38	39	40	42	41	39	40	38
Median age	37	36	36	38	39	38	37	38	36
Range									
Youngest	25	26	28	28	27	25	26	25	26
Oldest	77	70	68	70	65	77	67	71	69
Repeat Grantees									
Mean age	43	43	41	44	45	44	44	42	42
Median age	41	41	39	41	43	42	42	40	41
Range									
Youngest	27	27	29	27	33	28	28	31	30
Oldest	81	81	62	67	66	80	73	67	63

Table III

% of salary paid by federal funds

Position	100%	50-99%	1-49%	0%
Professor	8.4%	6%	23.6%	62%
Associate Prof.	15.7%	15.5%	22.8%	46%
Assistant Prof.	19%	14%	22%	45%
Instructor	25%	9.7%	12.5%	53%

half their salary paid by Federal funds, 34.7 percent of the instructors receive such support. And although fewer instructors are funded than associate and assistant professors, a higher percentage of instructors (25 percent) receive their total salary from Federal funds than any of the other three groups.

Our primary conclusion is that more should be known about the effectiveness of and current funding levels for different age groups in the research sector. At present, there is little concrete data. In the absence of contrary evidence, we can only surmise that NIH may not have obtained an ideal distribution of support between investigators at different levels of age and experience. It has sometimes been suggested that all first time applicants who have completed training in an approved program should be funded.

Distribution of Research

In our sections on intramural and collaborative research we discuss trends in those areas. The location of extramural grant research has changed but little over the past dozen years.

PERCENT OF NIH RESEARCH GRANTS BY GRANTEE INSTITUTION

	1954	1960	1964	1966
Colleges and Universities	78	72	72	73
Schools of Medicine	53	48	49	49
Other Health Prof. Schools	4	5	4	4
Univ. (Exd. Health Prof.)	16	19	20	21
Hospitals	16	15	17	14
Private Non-Profit Org.	4	7	6	10
Foriegn	1	3	2	2
All Other	2	3	3	2
Total Dollars (Millions)	$28.9	$198.8	$462.9	$556.2

The only trend worthy of mention is the increase in the percentage going to the non-health professional part of universities. This no doubt reflects the increasing reliance of medical research on the sciences. We see no reason why this trend should be reversed.

Comparison with Other Fields

There is no theoretical base which would allow us to establish the appropriate fraction of total national research and development expenditures which should go to biomedicine. Roughly 7 percent of our GNP goes to health and medical care. The medical share of R & D expenditures is 10 percent, although it makes up only 9 percent of Federally-supported R & D. There is no reason to assume that fractions of GNP and fractions of R & D should correspond. Defense expenditures are, quite naturally, heavily oriented to R & D. However, even some of the outstanding growth areas in our economy such as the services sector engage in very little research and development. *A priori* it is not possible to state a desirable relationship between the percentages cited above.

It may be relevant to compare R & D support between related fields, or among related situations. If the current 10 percent figure is appropriate for the medical share of the Nation's R & D expenditure, it might seem unlikely that the appropriate figure for 1975 would be 5 percent or 20 percent. However, changes within a field (or less likely within a great many other fields) might well alter the appropriate figures. The most likely significant change in the biomedical field in the near future is movement into large scale development expenditures. At this time, it is difficult to predict the size or the time pattern of this movement.

In addition to prediction difficulties, time extrapolations suffer because there usually is no assurance that present expenditure patterns are optimal. In fact, it may not be easy to decide what the characteristics of the optimal pattern would be. The non-functioning of the market mechanism prevents consumer preferences from determining allocations. However, there is the possibility of allowing producer preferences to help guide expenditures. To some extent, the motivation of scientists is to develop new information which society will value highly. In theory, we might agree to support all scientists of a certain level of capability, allowing them to choose their own fields of research, relying on their scientific motivation to correspond roughly to consumer preferences.

There are, of course, many difficulties associated with any scheme of this sort. The value of the output is not the only factor which determines the attractiveness of a research field. Scientists follow fads. A research area may be attractive because good scientists are working in it, or because it presents intellectually stimulating problems.

It would be unwise to follow any *laissez-faire* scheme to the extreme. Some fields will be under-investigated (the best example is oceanography which recently has been the beneficiary of large scale Federal encouragement), as was biomedical research before the advent of NIH. However, among mature research areas there should be some element of parity among the financial inducements they offer to enter the field.

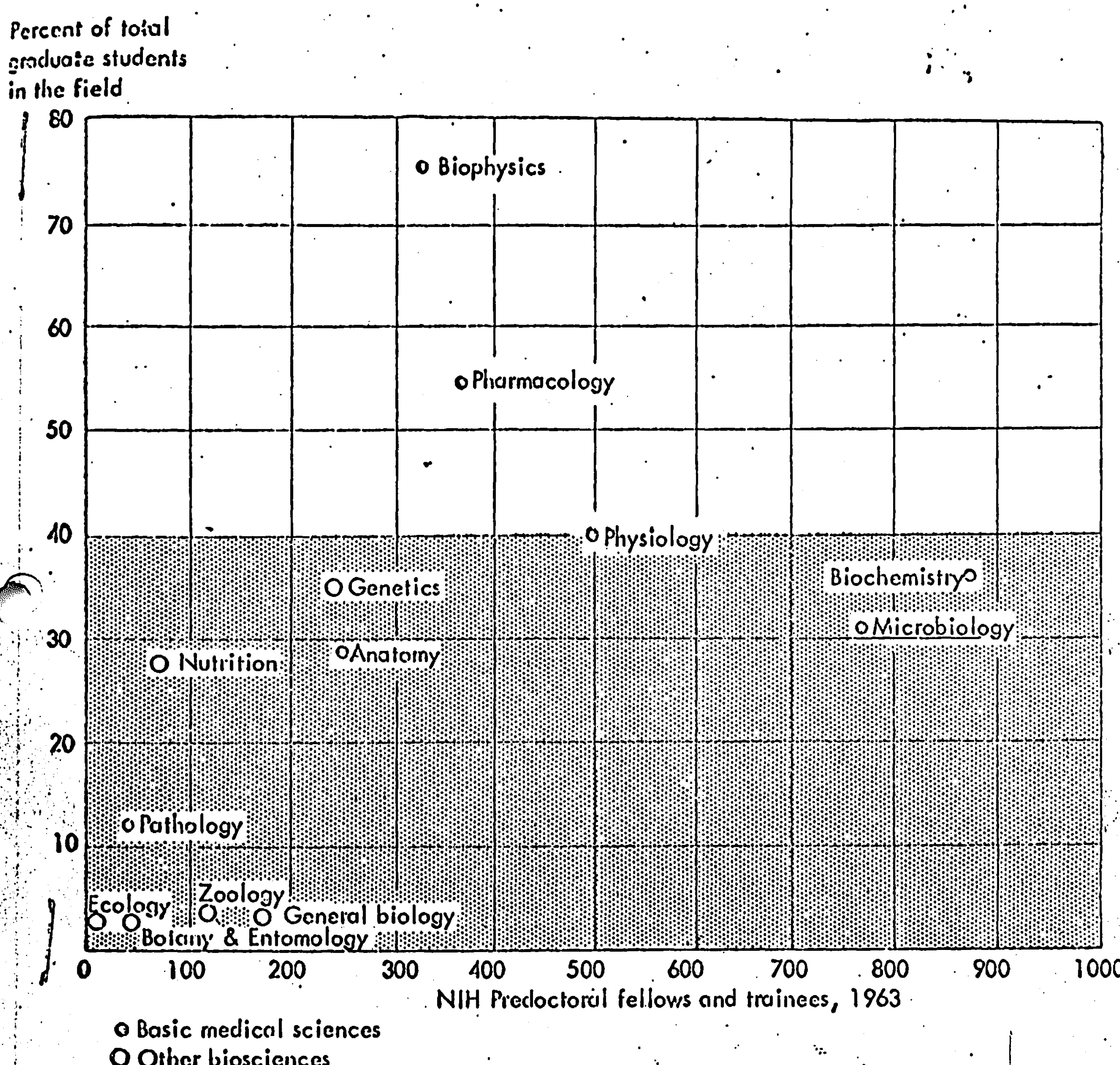
<u>Support of Graduate Study</u>

Biomedicine as a whole is the most heavily supported major area of science on the graduate level. The table below gives some relevant figures:

Table 4.10 Stipend Income for Graduate Students, By Field of Study, 1963

Field	(1) Percent Receiving	(3) Expected Value
Physical Sciences		
Astronomy	85	2300
Chemistry	81	2000
Physics	76	2100
Geology & Geography	72	1700
Oceanography	87	2600
Meteorology	81	2000
Mathematics	67	1500
Agriculture	80	2200
Engineering		
Civil Engineering	66	1700
Metallurgy	61	1700
Chemical Engineering	71	1900
Electrical Engineering	56	1100
Mechanical Engineering	60	1100
All other	71	1800
Biological Sciences		
Anatomy	84	2700
Biological Science, general	71	2100
Biochemistry	92	2600
Botany	87	2200
Biophysics	91	3100
Genetics	91	2700
Microbiology	87	2100
Pathology	75	3400
Physiology	86	2600
Zoology	84	2000
All other	84	2400
Psychology	64	1500
Anthropology	66	1700
Economics	65	1600
Sociology	62	1500
English	46	900
History	46	800
Geography	61	1300

It is instructive to compare the material shown in the graph below and the table which precedes it:



The seven fields most heavily supported by NIH in terms of numbers of students are also the seven for which they support the greatest percentage of total graduate student enrollment. These areas fared very well in their degree of support on the graduate level, as the following table shows:

GRADUATE FIELDS WHICH LEAD IN SUPPORT BY NIH, 1963

Field	# of Students	% of Students in Field	Percent on Stipend	Expect Value of Stipend
Biochemistry	880 (1)	36% (4)	92% (1)	$2600 (5-7)
Microbiology	770 (2)	31% (6)	87% (4-6)	$2100 (12-14)
Physiology	500 (3)	40% (3)	86% (7)	$2600 (5-7)
Pharmacology	370 (4)	55% (2)	n.a.	n.a.
Biophysics	420 (5)	75% (1)	91% (2-3)	$3100 (2)
Anatomy	250 (6)	29% (7)	84% (9-11)	$2700 (3-4)
Genetics	240 (7)	35% (6)	91% (2-3)	$2700 (3-4)

Numbers in parentheses are ranks out of the 32 fields represented in Table 4.1.

The average expected stipend for a field listed in Table 4.1 is $1970, for the sciences it is $2260. The average for the fields which receive the greatest NIH support is $2630. Perhaps more important than the expected value of support is the percentage receiving support. Here again, NIH's major fields did very well. In comparison to the 32 field average of 73 % and the science average of 705%, the NIH fields average was 88.5%.1/

We unfortunately do not have the comparative stipend and percentage support figures for years later than 1963. However, it is important to point out that graduate study in the biosciences continued to increase rapidly due, in part, to generous NIH encouragement.

1/The NIH major fields are important in getting overall averages. Given the mathematical properties of averaging, the disparities are reduced because the NIH fields were included in the overall averages.

Field	Percentage Increase in Graduate Enrollment	
	Average Annual Increase, 1960-61 to 1965-66	1964-65 to 1965-66
Total, Selected Science Fields	9.8	9.5
Biosciences	14.0	14.8
Basic Medical Sciences	12.4	11.4
Other Biosciences	15.0	17.0
Mathematics and Statistics	11.9	10.8
Physical Sciences	7.0	7.2
Selected Social Sciences	9.8	16.6
Engineering	9.5	5.9

In a field which is expanding rapidly in response to support on the graduate level, we would expect that large percentages of under-graduates in allied fields would be going on to graduate study. This is, in fact, the case in the biosciences.

One-third of 1961 college seniors were in graduate school three years later in 1964. For those whose career field was the biosciences, 55 percent of 1961 college seniors of both sexes and two-thirds of the men were in graduate school in 1964. We discuss the implications of this situation of exceptionally wide-spread support in our section on the quality of personnel in the biomedical research field.

Comparison in Approval and Funding Rates

It is also instructive to compare the support given to different fields of science. In the tables below we compare NSF and NIH experience.

EXPERIENCE OF NSF AND NIH SPONSORED RESEARCH PROJECTS

	Proposals	Awards	Rate
NSF 1966			
NIH Related Sciences including Cell Biology, Molecular Biology, Physiological Processes, and Psychobiology	1761 for $154.4million	907 for $33.1million	51% 22%
Other Sciences including Environmental and Systematic Biology, Biological Oceanography, Astronomy, Chemistry, Mathematical Sciences, Physics, Atmospheric Sciences, Earth Sciences, Physical Sciences	5015 for $357.1million	2740 for $1274 million	55% 35%
NIH 1965		Approval Rate $Awards Rate	51.5% 43.7%

This table indicates that on the whole, NIH related sciences did not fare nearly so well as other sciences in the competition for NSF funds. To the extent that the NSF standards employs standards of scientific quality across fields, it would appear that the scientists in NIH related fields are of generally lower quality. There are, of course, other factors which might help explain these figures.

No definite conclusions can be drawn comparing the statistics for NSF and NIH. The NSF system does not have the two step, award then fund, procedure. Thus, it is impossible to state whether a greater or lesser percentage of projects of scientific merit get funded at NSF.

It is instructive to note that the amounts percentage as a fraction of awards percentage (approval percentage for NIH) is much lower at NSF than it is at NIH. If anything, the fact that some NIH approved projects go unfunded would have led us to expect the opposite situation. Possible explanations include:

1. NSF cuts its approved applications more than NIH.

2. In comparison to NSF, NIH approves and funds more projects at the larger end of its spectrum.

3. NIH funding rates (approved projects funded/approved projects) is sufficiently high not to affect its amounts percentage/awards percentage significantly.

NIH funding information for 1966 is shown below:

1966 research grant applications, approved, financed, and unfinanced

[Amounts in thousands]

Appropriation	Approved		Financed		Unfinanced	
	Number	Amount	Number	Amount	Number	Amount
Cancer	1,608	$59,745	1,453	$54,228	155	$5,517
Heart	2,197	82,351	2,171	81,710	26	641
Dental	425	13,428	339	11,263	86	2,165
Arthritis and metabolic diseases	3,087	82,804	2,796	75,204	291	7,600
Neurological diseases and blindness	1,874	59,710	1,676	54,495	198	5,215
Allergy and infectious diseases	1,620	45,348	1,348	38,271	278	7,077
General medical sciences	2,134	67,643	1,684	53,612	450	14,031
Child health and human development	1,065	34,136	1,065	34,136		
International research	129	384	129	384		
Environmental health sciences						
Total	14,145	445,549	12,661	403,303	1,484	42,246

The overall funding rates are 89.5 percent by number and 90.5% by amount. Funding figures for past years are in Appendix __. ~~Percentage percent cut in funded.~~

<u>Income Comparisons</u>

Income comparisons between fields may easily be misleading. The following table shows estimated lifetime earnings for different scientific areas.

ESTIMATED LIFETIME INCOME BY FIELD OF PH.D. SPECIALTY 1960[1/]

Field	Income (000's)	4-Year Stipend[2/] (000's)	Stipend as % of Income
Physics	$168.1	$ 9.7	5.44
Chemistry	158.9	9.4	5.60
Pharmacology	154.8	12.9	7.68
Geology	144.5	8.2	5.35
Psychology	140.4	6.7	4.52
Biochemistry	132.9	12.6	8.65
Agriculture	132.1	10.6	7.40
Microbiology	131.6	9.9	6.96
Physiology	126.2	12.2	8.83
Genetics	123.1	12.9	10.21
Botany	118.5	10.3	7.99

Underlined fields are those of major NIH support. Biophysics and anatomy notavailable.

1/Stipend and income figures discounted at 6 percent, $ are 1957-9 dollars.
2/Stipends are for 1963.

SOURCE: Dissertation in progress by Richard Freeman of Yale University on scientific manpower. Note stipend numbers are somewhat greater than those shown elsewhere in this report.

It is clear that stipends are a larger portion of estimated income in biomedically-related fields. This is accounted for in large part by the fact that they are better supported on the graduate level.

The biomedical fields do not do well with respect to estimated lifetime income. Out of 11 fields they rank third, sixth, eighth, ninth, and tenth. On a non-weighted basis, their estimated income is 7 percent lower than the other scientific fields. Richard Freeman, whose dissertation is the source of this data, suggests that the relatively low income expectation might well reflect the fact that the biomedical research field is over-populated at present. Further investigation would be required before we could accept this rather sweeping conclusion.

Patterns of Support in Other Nations

A comparison of U. S. experience with that of other Nations may be instructive. The table below compares Federal R & D allocations among four countries.

SUPPORT OF VARIOUS GOVERNMENTS FOR HEALTH AND MEDICAL R & D

Country	Category	Expenditure	% Total Gov't Funds R & D	% of Total Excl. Defense
Canada	Health and Welfare 1959-60	4.3 million Canadian $	2.0	2.9
France	Health 1961	13 million francs	.5	1.1
United Kingdom	Health and Medical	6.3 million pounds	1.6	4.4
United States	Health and Welfare 1961-62	$451 million	4.8 1/	16.7
United States	Medical and Health Related Research 1967	$1475 billion	8.9	15.9

1/Education and NSF not included.

Source of original figures: *Basic Research and National Goals*. National Academy of Sciences, March 1965. 1967 figure from NIH Basic Data.

The data suggest that the United States grants a relatively larger portion of its R & D expenditures to biomedicine than do other countries.2/

2/The United States does finance some research in other countries, but the amounts are not so significant as to alter any implications drawn from this data.

In concluding this section, we note that by all measures we could discover, biomedical research in this country is as well or better supported as other areas of science. To the extent that it is better supported, individuals in the biomedical area can receive financial encouragement that more capable people working in other areas would be denied. Without a demonstration that biomedicine is an inherently more important area than other areas of science as a whole, we must regard this as an inefficient and inequitable situation.

Medical Schools

In the five-year period from 1960-61 to 1965-66, the full-time faculties of the Nation's medical schools increased 54 percent, from 11,111 to 17,149. During the same period, the number of medical students increased only 8 percent, from 30,288 to 32,825. The net result was a decline in the student to faculty ratio of 30 percent, from 2.7 to 1.9. But the faculty also may be turning out PhDs.

This dramatic increase in faculty clearly reflects the large amounts of money which have been poured into the medical schools. The percentage of full-time faculty receiving all or part of their salary from the Federal Government has increased from 27 to 49, in the same period. In the period from 1958-59 to 1964-65, medical school expenditures increased from $319 million to $779 million, an increase of 144 percent. The percentage of total medical school expenditures paid by Federal funds increased from 30 to 54. Federal funds to medical schools multiplied nearly four and one half times in this five-year period.

The most rapid growth within the medical schools came in the sponsored programs (218 percent). The regular operating programs increased only 83 percent over this period. And these figures do not even ~~present~~ represent the true extent of the disparate growth rates. Nearly $50 million in overhead on contracts and grants, $44.7 million Federal, was budgeted for regular operating programs in 1964-65. No such funds were available in 1958-59.

Table
MEDICAL SCHOOL EXPENDITURES (IN MILLIONS)

Sponsored Programs	1958-9	1964-5	Change
1. Federal contracts and grants for teaching and training	$ 20.8	$ 95.8	+361%
2. Total contracts and grants for teaching and training	$ 25.4	$104.0	+309%
3. Federal contracts and grants for research	$ 74.1	$280.6	+278%
4. Total expenditures for sponsored research	$113.8	$342.9	+202%
5. Total expenditures for sponsored programs (2 + 4 + misc.)	$144.2	$459.0	+218%
Regular Operating Programs			
1. Overhead on Federal contracts and grants	0	$ 44.7	+
2. Total expenditures for regular operating programs	$178.8	$319.7	+ 83%

MEDICAL SCHOOL FINANCIAL DATA

	1958-9	1963-4	1964-5
10. Percentage of total medical school expenditures applicable to regular operating programs (excluding sponsored programs)	55%	41%	41%
11. Percentage of total medical school expenditures for all sponsored programs	45%	59%	59%
12. Percentage of total medical school expenditures for sponsored research	36%	45%	44%
13. Percentage of total medical school expenditures paid by federal funds	30%	54%	54%
14. Percentage of sponsored research paid from federal funds	65%	81%	82%
15. Percentage of sponsored research paid from non-government funds	35%	19%	15%
16. Percentage of sponsored research paid from state and local government funds		4%	3%
17. Percentage of total medical school expenditures paid for federal research	23%	36%	36%

This data provides obvious justification for the frequently-voiced conclusion that the Federal Government has become the primary source of support for the Nation's medical schools. It is significant, however, that only a limited portion of this support goes directly into regular operating programs. This is true. We were training faculty for new schools & should get them there.

New Medical Schools

The Nation is now confronted with a significant shortage of physicians. In partial response to this need, a number of new medical schools are in the planning and developing stage. As of November 1966, 16 new schools had received provisional accreditation from the Association of American Medical Colleges (see Table 1). These schools are all scheduled to open their doors by 1971. It is reasonable to hope for an additional ten schools by 1975.

Table 1.—Medical Schools in Development 1965-1966

School	Chief Administrative Officer	Tentative Starting Date	Maximum Enrollment Planned
University of Arizona[*] School of Medicine	Merlin K. DuVal, Jr. Dean	Fall, 1967	64
Brown University[**†] Program in Medical Science	M. V. Edds, Jr. Director of Medicine	Fall, 1963	50
University of California[*] School of Medicine (Davis)	C. J. Tupper Dean	Fall, 1968	128
University of California[*] School of Medicine (San Diego)	Joseph Stokes, III Dean	Fall, 1968	96
University of Connecticut[*] School of Medicine	Joseph W. Patterson Dean	Fall, 1968	64
University of Hawaii[†] School of Biomedical Sciences	Windsor C. Cutting Dean	Fall, 1967	50
Louisiana State University[*] Medical College (Shreveport)	Edgar Hull Interim Dean	Fall, 1969	100
University of Massachusetts[*] School of Medicine	Lamar Soutter Dean	Fall, 1970	112
Michigan State University[*] College of Human Medicine	Andrew D. Hunt, Jr. Dean	Fall, 1966	50
Mount Sinai[*] School of Medicine	George James Dean	Fall, 1970	100
University of New Mexico[*‡] School of Medicine	Reginald H. Fitz Dean	Fall, 1964	48
State University of New York[*] School of Medicine (Stony Brook)	Edmund D. Pellegrino Director of Medical Center	Fall, 1971	150
Pennsylvania State University[*] Milton S. Hershey Medical School	George T. Harrell Dean	Fall, 1967	64
Rutgers—The State University[*] Rutgers Medical School	DeWitt Stetten, Jr. Dean	Fall, 1966	64
University of Texas[*] South Texas Medical School (San Antonio)	F. C. Pannill Dean	Fall, 1967	100
Toledo State[*] College of Medicine	Glidden L. Brooks	Fall, 1970	100

[*] Planning complete curriculum for MD degree.
[†] Planning programs to meet requirements for the first two years of medical curriculum.
[**] Six-year combined premedical-medical program.
[‡] First class enrolled September 1964.

It is important that the personnel exist to staff these new schools competently, meanwhile allowing for the expansion of those now in existence. The problem does not seem to be critical, however, at this time. For this, as Dean Thomas Turner points out, we must thank NIH:

> In another context, the large NIH program of research support has had a most important but perhaps largely unanticipated result. For almost solely as a result of this program, during the past fifteen years it has been possible to bring about a substantial increase in the yearly number of medical graduates in the United States (from 6,135 in 1951 to 7,677 in 1966, a 25 per cent increase); and further increments are projected. We now have the medical and scientific manpower to staff the enlarged medical schools and the new schools the nation needs so badly. Had this program not been operative, significant expansion of educational facilities could have been accomplished only at the risk of a decline in the quality of medical education. As it is, since enough teachers are available, only money and facilities are required rapidly to increase productivity in terms of trained health manpower. The figures speak for themselves. It is estimated that in 1951 full-time faculty positions in 79 medical schools numbered about 3,575. Today, these positions number in the order of 17,000 in 88 medical schools, a pool quite adequate to furnish the cadres for new and enlarged schools. Moreover, in most medical schools preclinical departments have been transformed in their pedagogical effectiveness as well as in their research potential, and much the same can be said of clinical departments.

Even if we should be fortunate enough to raise medical school enrollment to 50,000 students by 1975, our 1967 faculty would be large enough if we returned to the 1959-60 student/faculty ratio (2.9). This is not, of course, a very likely eventuality. It seems highly improbable that student/faculty ratios will return to that level in such a short period of time. However, all evidence seems to point to the fact that continued increases in the research orientation of medical schools will not be feasible if we expect dramatic increases in our output of trained physicians.

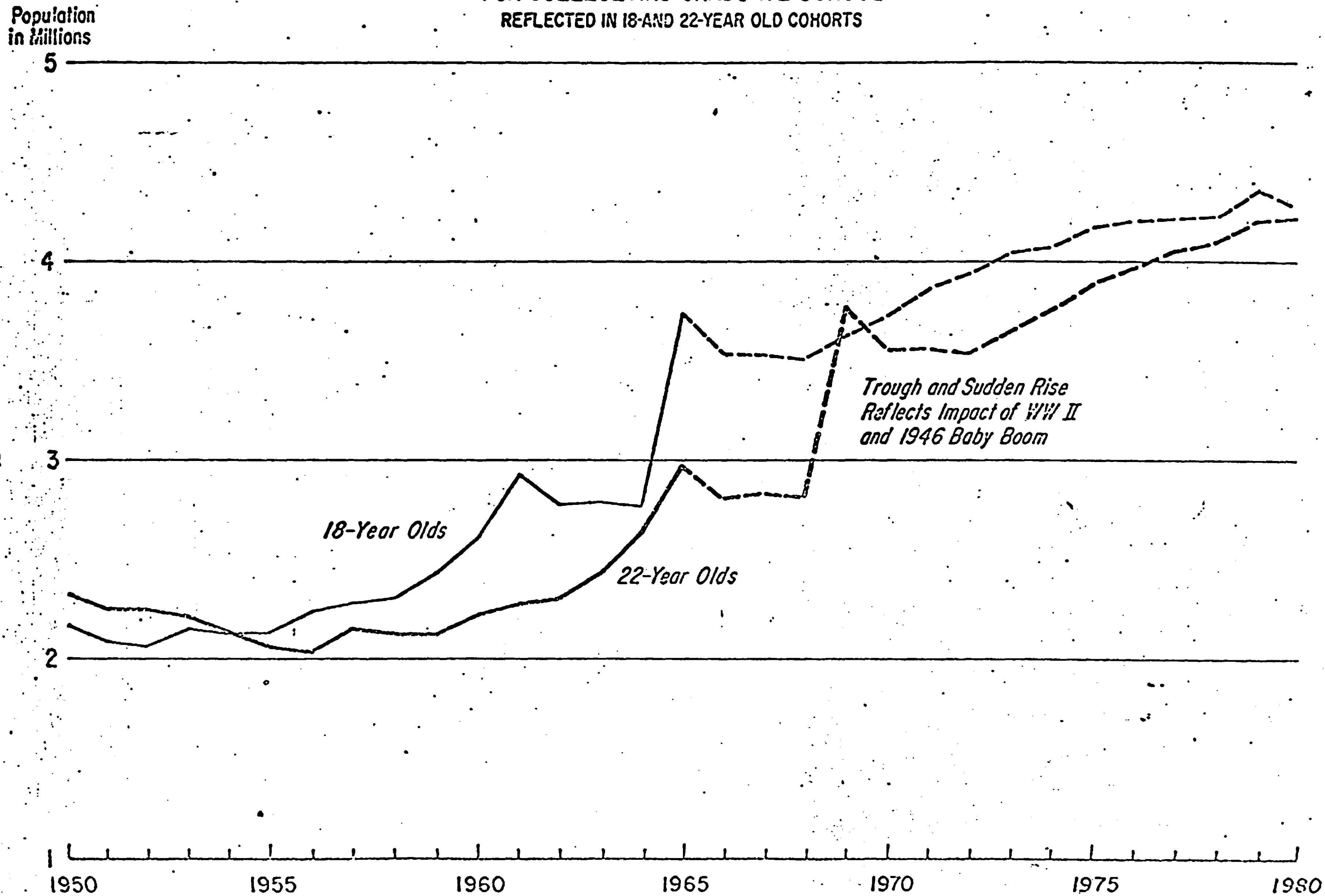
Training More Physicians

Such an increase in the output of physicians would unquestionably require some Government financing. Much discussion has arisen about Federal loan programs and possible grant programs for medical students. However, the bottleneck does not seem to lie in the supply of potential medical students.

	Number of Students Applying	Number Accepted
1959-60	14,952	8,512
1965-66	18,703	9,012

The ratio of applicants to places has gone up in the last six years, and there remains a large pool of students who are denied entrance because of a lack of space. With the expected increases in the number of bachelor degrees over the next decade, we should not expect any deficiency of people desiring a medical education (see Chart 2). Between 1967 and 1975, it is estimated that the number of 22-year olds in our population will increase from 2.8 to 3.8 million, and that college graduates will increase at least proportionately. In addition, we expect increasing incomes to combine with better loan and scholarship programs, making medical education a possibility for a larger number of college graduates. We don't mean to minimize the importance of improved Federal assistance to medical students. There are important arguments for such aid on both equity and efficiency grounds. We merely wish to point out that it does not seem that a lack of qualified applicants will be the restraining force in expanding medical education.

Chart 9
EXPANSION OF POPULATION BASE FOR COLLEGE AND GRADUATE SCHOOL
REFLECTED IN 18-AND 22-YEAR OLD COHORTS



What will be needed, then, is increasing Federal assistance to medical schools for regular operating programs, for teaching rather than research. Neither increased research support by itself, nor significant assistance to medical students will be adequate. Our reasons for this conclusion seem clear. Looking back at Chart 1, we observe that expenditures for regular operating programs increased 83 percent over the five-year period from 1959-60 to 1964-65. Excluding overhead on Federal contracts, the increase is only 55 percent. Tuition income kept pace with this lower rate of increase, but endowment income and gifts lagged far behind. The big source of increased support for regular operating programs were State expenditures for defrayal of medical college expenditures, which nearly doubled, so that they now account for 30 percent of the total spent on regular operating programs. The 83 percent increase came during a period when medical student enrollment increased only minimally. Any dramatic increase in enrollment will have to be accompanied by a dramatic increase in regular operating expenditures. It is not within the scope of this paper to assess the Federal Government's role in supporting this increase. We merely wish to point out that increasing research contracts will not be the solution.

NIH Practice of Separating Considerations of Distribution and Scientific Merit

Much of the concern about distribution centers around the question of support for small medical schools and those in the development stage. The General Research Support Grants Program disperses funds on a formula basis which favors the smaller schools.[1] The following data taken from

1/Each school is eligible for $25,000 automatically. Above that, each school is eligible for 5 percent of the first $1 million of Federal research money expended by the school in the prior fiscal year; 3 percent of the second $1 million of Federal research funds; 10 percent of the (continued)

Not [illegible]

a recent Budget Bureau study shows the breakdown in support given to quartiles of medical schools ranked on size, budget, and general excellence.

	NIH-NIMH Research	General Research Support	($ in Millions) Other Agency Research
1st Quartile	$139.4 (51)	8.1 (37)	34.4 (51)
2nd Quartile	66.1 (24)	5.7 (26)	16.2 (24)
3rd Quartile	42.2 (16)	4.8 (22)	9.2 (14)
4th Quartile	23.5 (9)	3.2 (15)	8.1 (12)
TOTAL	$271.2	21.8	67.9

Figures in parentheses are percentages

However, as the table indicates, these grants are of relatively minor importance accounting for less than 15 percent of the support for 4th quartile schools. We emphasize this program because we think it shows that NIH has drawn an important distinction. Programs designed to ensure the survival of underfinanced or new institutions, or to improve the geographical distribution of funds should be separated from those whose aim is to support the most talented scientifically. The advantage of such separation should become increasingly clear as the projected new medical schools come into being.

The Woolridge Report was in general enthusiastic about General Research Support Grants. The following quote is from their Administration Panel Report.

The materials below from FY 1968 appropriations hearings show the financial history and present structure of this program.

1/ (continued) first $1 million of private funds and 6 percent of the second $1 million of private funds. These funds can be used at the discretion of the institution to open new fields, adjust to fluctuations in research support and provide services.

"General Research Support Grants: The Panel was especially interested in learning how institutions have been using the funds allocated to some of them, in the past few years, under the program of general research grants. We found a diversity of approaches. In each case, a method seemed to us to be appropriate to the immediate, local situation, and we believe these funds are generally being applied prudently and wisely. We see much merit in the suggestion, made in several of the institutions we visited, that general research support funds be increased by a factor of two or three and that some more latitude be allowed in their use. The Panel wonders whether GRSG support might not well be offered also to appropriate divisions or departments of institutions other than medical and dental schools. Incentives of thos sort might help expedite the current NIH campaign to bring institutions generally into their proper role, as the Panel sees it, in the total administration of the grant program."

The materials below from FY 1968 appropriations hearings show the financial history and present structure of this program.

2. Funds

The amount available for distribution to this program constitutes "such uniform percentage, not to exceed 15 per centum, as the Surgeon General may determine, of the amounts provided for grants for research projects for any fiscal year through the appropriations for the National Institutes of Health . . ." (Public Law 86-798).

1962 obligations	$20,000,000
1963 obligations	30,000,000
1964 obligations	35,000,000
1965 obligations	45,000,000
1966 obligations	45,200,000
1967 estimates	[1] 51,700,000
1968 estimates	[1] 61,700,000

[1] An amount for NIMH *is included* here (1967—$6,434,000; 1968—$5,667,000) inasmuch as the total is obligated through NIH.

3. Present stage of development

In 1967 general research support grants will provide support for 296 health professional schools, hospitals, research institutes, laboratories, centers and certain other nonprofit research organizations heavily engaged in health related research. In addition approximately 100 academic institutions other than health professional schools will be supported. Approximately eight institutional advancement awards will also be made. These awards are intended to encourage the recipient institutions to enter the health research field for the first time or to expand and improve their present biomedical research activities.

The Distribution of Support for Medical Schools

Figures for percentage distribution of total funds are often not too informative. No one would expect that a school with a faculty of 50 should receive the same total funds as one whose five times that figure. The chart below gives what we believe to be a more relevant comparison.

MEDICAL SCHOOL SUPPORT RANKED BY SIZE OF FEDERAL CONTRACTS AND GRANTS FOR RESEARCH

	Federal Research $	% of Total	Faculty	Research Per Faculty Member ~~$22.3 Thousand~~
First 22 Schools	$154.5 m.	55	6,579	$22.8 thousand
(per school)	7.0 m.			
Second 22 Schools	66.9 m.	24	4,389	15.2
(per school)	3.0 m.			
Third 21 schools	38.5	14	3,013	12.8
(per school)	1.8			
Fourth 22 schools	20.5 m.	7	2,035	10.1
(per school)	.93			

Our figures were derived from data submitted by the Association of American Medical Colleges for medical schools.

The distribution of funds shown in the table above is significantly more skew than that of Table . However, the more relevant figure, research dollar per faculty member shows that the smaller schools are not neglected. Only two schools in 87 receive less than $5,000 per faculty member in Federal research funds, two more receive less than $7,000.

It would not be reasonable to expect that achools of varying quality would receive equivalent levels of research support. The figures cited above indicate that the funds are spread fairly well, certainly much more than most people believe.

Training funds on the whole are much more evenly spread than research funds. The 15 largest medical schools (in terms of faculty) receive roughly 10 percent more Federal training dollars per faculty member than do the 15 smallest. If anything, it would appear that these funds are not sufficiently concentrated. First rate training is best accomplished in an atmosphere of first rate research.

We are concerned with the distribution of funds at medical schools because we wish to ensure the quality of medical education, and promote the dissemination of medical knowledge. A further consideration is the distribution of physicians. The larger, better medical schools are on the whole located in those States which have higher overall levels of medical excellence and service. It is not the purpose of this report to study those factors which influence the geographical distribution of physicians. However, we might mention that Federal funding has not led to a situation in which the great majority of medical students are drawn from a very few States. The Nation-wide average is 4.2 entering medical students per 100,000 population. Alabama, Alaska, California, Maine, Nevada, New Hampshire, New Mexico, North Carolina, Rhode Island, and Texas were 20 percent or more below this average. Only Alaska, Maine and Nevada had 2.5 or fewer. It is encouraging to note that five of the 16 new medical schools are to be located in the ten States which send the fewest students per capita to medical school.

Competition between Biomedical Research and the Production of Physicians

People concerned with the inadequate supply of physicians in the Nation, often cite the outstanding growth of the biomedical research profession as a contributing factor. The following quotes are representative:

> And indeed, I believe there is evidence of competition between the demands of medical practice and the demands of medical research. I refer to the frequently quoted statistics showing that the relative number of A students in first-year medical school in the United States fell from 40 per cent to 13.4 per cent, during the period from 1950 to 1960. Although it is hard to document, I have always believed that at least part of this loss in quality was a consequence of the favoured position of the graduate student in biomedical research as compared with his counterpart in medicine. The United States Government has made fellowships available for the research student but, with few exceptions, not for the medical student.
>
> Alvin Weinberg

> After absorption of the post-World War II backlog of medical school applicants, a progressive decline set in which reached a low of 14,381 applicants in 1961-1962. Thereafter, there was an encouraging, although moderate, upturn in applicants.
>
> The National Science Foundation study brought out figures showing a gradual bleeding away of student interests from medicine as a field of research and practice into biological sciences, conventional fields have lost ground to biochemistry, biophysics, genetics, and microbiology, generally regarded as the glamour fields.
>
> The AMA, Report on Research

There is much contrary data and evidence. NIH proponents claim that high level research and first quality teaching go hand in hand. Moreover, they assert that without the increasingly sophisticated science orientation of medicine, numbers and the quality of medical students might have fallen considerably over past dozen years.

The tables below indicate that any claims of falling quality over the past 14 years will not be easy to validate. Although the percentage of A students has fallen, the percentage of C students has fallen as well.

(There is, of course, no way to know that college grading standards have not changed over this period.)

(Average Year)	A Students	B Students	C Students
1953-1957	17.5	68.9	13.6
1958-1961	14.2	70.3	15.5
1962-1963	12.4	73.5	14.1
1964-1965	13.3	76.2	10.5

A better comparison of quality would seem to be scores on the medical school admissions test:

Table 15.—Mean Medical College Admission Test Scores of Accepted Applicants During the Past 14 Years

Yr.	Verbal Ability	Quantitative Ability	General Information*	Science	Taking MCAT, No.	Total Accepted Applicants, No.
1952-1953	522	526	519	525	7,346	7,778
1953-1954	519	525	524	530	7,426	7,756
1954-1955	517	521	530	533	7,527	7,878
1955-1956	524	528	527	522	7,688	7,969
1956-1957	525	525	526	519	8,012	8,263
1957-1958	528	517	527	516	8,223	8,302
1958-1959	527	532	520	523	8,301	8,366
1959-1960	529	527	527	527	8,449	8,512
1960-1961	527	533	527	533	8,500	8,560
1961-1962	533	538	522	537	8,633	8,682
1962-1963	544	537	541	545	8,920	8,959
1963-1964	537	551	549	545	9,021	9,063
1964-1965	540	538	561	556	9,015	9,043
1965-1966	541	583	565	549	8,983	9,012

In our sections devoted to manpower we cite statistics which indicate that the supply of medical school applicants and medical school faculty have been and should continue to be adequate.

Biomedical Research as a Drain on Physician Manpower

Biomedical research also conflicts with the delivery of medical services because it engages licensed physicians. Good data are not available in this field. We have seen estimates ranging from 3 percent to 10 percent for the percent of the total physician population engaged in medical research. (This variation might be explained because different measures give different results; e.g., full-time, full-time equivalent, part-time). In a forthcoming report, NIH has made projections on the assumption that, "The proportion of M.D.'s entering research will remain constant at approximately 15 percent from each class." This figure seems high, and in isolation, somewhat misleading. Most researchers have clinical and/or teaching responsibilities. Further, entering statistics give high projections if, as we suspect, individuals devote less of their time to research as they grow older.

Dr. Turner, President of the American Association of Medical Colleges, does not believe the research drain on physician manpower is significant:

CRITICISMS

It is desirable at this point to deal with the commonly heard criticism that the large federal medical research program has diverted medical schools from their main mission of developing practicing physicians. I know of no data to support this contention.

For example, Sanazaro (3) in a recent paper gives data on career choices of medical graduates in the United States. Of 5,218 interns in 1964-65, only 2.7 per cent indicated research or teaching, or both, without clinical-care responsibilities as their career choice. In a study of Johns Hopkins medical graduates for the years 1948 to 1962 inclusive, Thomas found that only 3.9 percent were engaged in activities which did not involve patient care; and, of these, fewer than half were exclusively engaged in research.* Significant, too, was the fact that no trend toward solely research careers was noted over the period which spans the years of greatest build-up of the NIH research support program. Indeed, one may question whether full-time research is attracting an adequate proportion of American medical graduates. Fortunately, the federal research support has led to a great increase in the number of Ph.D. graduates who make full-time careers of health-related research.

*Personal communication from Caroline B. Thomas, 1966.

With no concrete evidence to the contrary, we tend to hold the same viewpoint as Turner.

Future Prospects

Alvin Weinberg states, in his article "Scientific Choice and Biomedical Science," that "We are, or ought to be, entering an age of biomedical science and biomedical technology that could rival in magnitude and richness the present age of physical science and physical technology." His optimism stems in part from observations on the changing nature of biomedical research. In the past, "biomedical research avoided expensive experiments even if expensive experiments were required to obtain reliable statistics." This tradition is already under attack, however, and it appears that an important part of future science will belong to "big biology." A major part of this "big science" will reflect the development of powerful and expensive, new instruments and technology for biomedical research. Instruments which may be developed in the near future include an electron microscope with one angstrom resolution (powerful enough to "see" individual atoms), bigger and better-adapted ultracentrifuges, and automated laboratories for chemical analysis. The use of computer technology will probably accelerate as biomedical researchers become more knowledgeable and sophisticated about the possibilities for exploitation.

Another trend, which corresponds well with the increasing use of instruments formerly used only by physical scientists, is the increasingly interdisciplinary nature of the biosciences. This may require new methods of reaearch organization. The best research teams may need to have men trained in several different fields. Indeed Weinberg feels that the interdisciplinary research institute is superior to the universities for the kind of mission-oriented research it will take to solve the tougher problems.* John Platt suggests that there ought to be institutional arrangements which would encourage, or at least not discourage, scientists who wished to change fields.** He believes that such changes of atmosphere are usually beneficial. The future will probably see increasing numbers of outside scientists and engineers moving into the biomedical sector.

A major factor in a future "biomedical age," however, will be results which are revolutionary, not merely in a scientific sense, but also in a human and institutional sense. Such science fiction themes as rejuvenation, artificial organs, and genetic engineering could become realities in the next few decades. Curing cancer would probably not alter the structure of our society, but many conceivable discoveries in the biomedical area could be profoundly influential, altering the very fabric of our lives. A small-scale current example is the psychedelic craze, triggered by the synthesis and release of LSD. Biomedical research will continue to grow because people have begun to realize its importance. Indeed, its potentialities seem so great that considerable wisdom and foresight will be required to deal with the discoveries it may give to an unwitting public.

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*This is current practice in France and England.
**Science. December 2, 1966, pp. 1132-9.

Attracting Manpower in New Fields

If as most analysts believe, we are entering an era of rapid progress in application of medical knowledge, we are also entering a period in which medical research will be forced to draw upon people in areas of expertise out of the biosciences. Most significantly, we expect that a great number of engineers will be engaged in work related to the development of medical techniques. To a lesser extent, scientists such as chemists, physicists and systems analysts will also be required for this work.

It is beyond the scope of this paper to give more than brief comment of the implications of this problem.

1. We have had much success in the past building up internally consistent fields such as biochemistry. It is by no means clear that we can repeat this success in bioengineering. The human body is not an engineering structure. The field of bioengineering appears to be more a bit of engineering and a bit of biology than a coherent field which lies between the two. I agree

2. The people currently engaged in bioengineering as best we can determine, are distinguished in neither engineering nor in their understanding of biological methods. There is a difficult problem involving incentives and professional orientation which will hinder efforts to attract people to this

This is a gratuitous slap at a group which is not homogeneous at all and which they could not sample! in 3 months. I suspect they are parroting somebody.

bastardized field.

3. The problems with which these scientists will be concerned are sufficiently complex, difficult, and important as to require the top minds in the respective fields. Fortunately, aspects of the problem lying outside a specific field can be explained to talented experts without an interdisciplinary background. Collaboration between biomedical specialists and scientists in other fields is a promising possibility.

The following quote from the Program Development Plan August 1967 for the artificial heart relates to this problem:

> It was therefore evident that if a truly satisfactory device or group of devices to aid circulation in the body were to be developed it would be necessary to involve the best biochemists, general physiologists, cardiovascular physiologists, polymerchemists, physicists, physicists and engineers that could be found. It was also evident that scientists of this caliber had the intelligence and usually the interest to learn the vocabulary and problems of other disciplines involved, and after an initial educational period could contribute a great deal to the overall program. (p. 9)

4. Probably the most significant problem relating to bringing people not trained in the biomedical sciences into the biomedical research area involves the organization of research. At present, centers with the appropriate interdisciplinary expertise do not exist.

(a) It seems unlikely that current levels of collaborative programs are sufficient to encourage industry to make the investments necessary to develop the capabilities required.

(b) Funding interdisciplinary training programs does not seem to be a promising avenue of approach.

(c) Allowing the medical profession to call on outside talent where needed is an expedient measure. However, it relinquishes the initiative to physicians. Innovations will be made where conventional techniques in other fields can be applied to medicine. It is also important that the leading innovators in other fields be given the incentive to look at the medical field to see where their new ideas can be put to work.

Alvin Weinberg recently commented on those areas within biomedical research in which the feasibility of direct application has been established.

"In this group he said he would place the application of engineering science to the development of the artificial kidney. 'To be sure, the artificial kidney is a cumbersome and awkward thing; yet artificial kidneys do work. We have passed the feasibility stage and what seems to be indicated is massive development . . . to reduce the technique to widespread practice.' Other examples, Weinberg continued, would be further development of medical scintillometry, automation of clinical chemistry, and development of zonal centrifuge and the 1-angstrom microscope." Science, November 4, 1966, p. 619-20.

The developments which Weinberg discusses require topflight talent in fields outside of biomedicine who have some acquaintance with specific problems in the biomedical field. A thorough knowledge of biomedicine would not be required.

Weinberg characterized these problems as "Prospects for Big Biology" and argued that national laboratories (such as Oak Ridge, his institution) were ideal institutions for undertaking huge costly programs that require multidisciplinary coordination. We are under the impression that some individuals involved with the collaborative programs at NIH could endorse this approach. ~~Thexxfollowingxquotesxfromxanx NIHxreportxinxprogressx~~
The following quotes from an NIH report in progress represent a point of view somewhat different from our own:

"It is estimated that a minimum of 2,000 bioengineers will be needed by 1975. With adequate support for training of this new breed of scientist, it is our conviction that this objective can be met. . .

"This area is of such critical importance that bioengineering training should be divided into two separate areas: The first dealing with the use of systems concepts in the study of human biology, the second dealing with appropriate segments of the scientific and medical complex in the design of systems for delivery of health care. It is our feeling that, because of the divergent needs of these two areas, separate programs of training may be needed although conceptually there will be a good deal of overlap between them. Of particular importance in the second category, i.e., the development of systems of health care, is the need to train not just bioengineers but a significant fraction of academic physicians in the respective disciplines who can use this knowledge in their day-to-day activities. Thus, it is not sufficient to have simply a bioengineer on the scene to set up a new systems approach method.

". . . Although the bioengineers will teach the teacher, we cannot wait for a long succession of events for the transfer of this knowledge to the effective delivery of health care; therefore, we need to find mechanisms which will rapidly cut through the usual sequences of events. We cannot afford to wait to train the bioengineer per se to train the teachers. Instead, we must find some way of utilizing people from industry and from other areas who can communicate this information to physicians who will be doing research on these techniques."

Projected Future Growth Rates

Much recent discussion has revolved around the question of how fast the NIH budget should grow in future years. Taken in isolation, there is no satisfactory answer to this question.

1. If, as we expect, there will be significant alterations in the conduct of biomedical research in the near future, we should expect equally significant alterations in the allocation of the NIH budget Some areas of support will no doubt grow at extraordinary rates, while others will undergo a relative and perhaps absolute contraction. It seems reasonable and desirable to project individual growth rates for different portions of the NIH program.

2. Throughout this document we have emphasized that the most profitable approaches to the problem of allocating funds to research involves the concept of opportunity costs. We therefore would argue against any attempt to determine rates of growth in support for biomedical research without considering at the same time the levels of support for other sciences, programs to improve the delivery of medical services, plans for feeding Federalfunds to the nation's medical schools, etc. We have not had the opportunity to examine prospects for these programs. However, we do believe that we have at least hinted how knowledge of these programs might be related to allocation considerations in biomedical research.

In considering future growth rates we would break NIH operations into parts --

Intramural Research
Extramural Research
~~Developmental Programs~~
~~Manpower~~ Development ~~Programs~~ of Manpower
Developmental Programs
~~Other~~ Other

NIH Approach

Before we turn to our analysis of specific areas, we present a section which attempts to replicates one NIH approach to future needs projections.

Estimates of future requirements can be made in either dollar or manpower magnitude. A projected inflation rate in cost per worker is, of course, the link that would be required to make these estimates consistent. Unfortunately, it is difficult to extrapolate past inflation figures into the future. Changes in the nature of scientific research work or in its location can radically alter expected rates of inflation. Recent experience has shown that costs per professional research worker have been increasing 6% per annum in government and 7% per annum in industry, where the preponderance of those engaged in biomedical research devote full time to such activities. Estimates developed by the National Science Foundation indicate that costs per researcher in academic institutions increased roughly 10% per annum between 1958-1964.

It is not improbable that the next decade will see more radical changes in the method of conduct of biomedical research than has the last. As we have mentioned above, biomedical science is now acknowledged to have moved into a state of maturity. In the coming years we will be devoting a greater percentage of our research effort to developing the ability to apply much of the new knowledge which has been generated over the past two decades. It would surely be folly to curtail governmental programs which support investigations into basic biomedical problems. However, it appears unlikely that basic research will grow as rapidly as applied and developmental research. Because of the nature of such efforts, and the probable introduction of "Big Science" into the biomedical area, costs might rise much more rapidly than expected in the next few years.

If we do decide to engage in more research of a big scientific nature, the biomedical fields will need to draw on increasing numbers of individuals from disciplines outside the biosciences, As NIH has put it

> . . . the new approaches to prevention, diagnosis and therapy will include sophisticated disciplines and agents, from molecular genetics to virus vaccines. Such operations will draw heavily upon emerging disciplines of a complex character, including biomathematics, bioengineering, computer science, physical chemistry, and molecular biology.

That is, we should expect more physicists, chemists, engineers, computer scientists and technicians to become involved in biomedicine. This trend will be even more significant if, as some people have predicted, industrial firms become seriously interested in biomedical projects.

The possibility of significant change in the conduct of biomedical research has two implications so far as the cost link between expenditures on biomedical research and the need for biomedical researchers:

1. If we extrapolate from past trends, we could significantly underestimate the expected cost increase per research worker in the field.

2. We could overestimate the number of people who ~~used~~ need to be trained in the biomedical sciences to conduct a given dollar volume of research[1].

(p. 297, The Advancement of Knowledge for the Nation's Health.)

Table I shows alternate possibilities for biomedical manpower in 1975 and 1985, given different assumptions about the percentage of GNP devoted to biomedical research and the shares of the government, non-profit, and industry sectors of research. The costs per worker have been extrapolated on the basis of a 6% per year rise for the government and non-profit sectors, and a 7% per year rise in industry. The GNP estimates are based on a deflated rate of growth of 4%.

It is evident that different assumptions give significantly different numbers with respect to the number of professional researchers who will be working in 1975 and 1985. At the present proportionate level of expenditures, biomedical research receiving .29% of the GNP, only 55,000 workers need be in the field in 1975, a decrease from current levels. With an increase in biomedicine's share to .50% of GNP, our estimate gives a possible 96,000 researchers. In 1985, our high estimate (Biomed = .01 GNP) is 152,000 workers, while the low figure, with biomedicine not increasing its share, is a mere 42,000.

Even if we could predict the desired number of researchers in years to come with responable accuracy, the implications for NIH training needs would not be clear. It would be necessary to estimate the number of individuals being trained in fields not normally supported by NIH training who would subsequently migrate into the biomedical area.* And, of course, one must be able to estimate the level of support which would induce the proper number of individuals to be trained in the biomedical sciences.

Biomedical Manpower in 1965

	Federal	Industry	Non-Profit	Total
Cost Per Worker	$25,900	$46,800	$26,100	
Number of Workers	11,800	11,900	40,300	64,000

*We realize, of course, that NIH may wish to support training in fields not currently being supported in order to get, let us say, engineers interested in the biomedical field. However, we would still expect there to be some migration from these fields into biomedicine. It is difficult to see how thepattern of migration could run to the detriment of biomedicine.

Estimates for 1975

GNP =$1,070 Billions

	Federal	Industry	Non-Profit Institutions	Total
Dollar Cost Per Worker	$46,400	$92,000	$46,900	
Percentage share (1)	14%	34%	52%	100%
Percentage share (2)	11%	40%	49%	100%

Biomedical = .0029 GNP =$3,103

	Federal	Industry	Non-Profit Institutions	Total
No. Workers, share (1)	9,400	11,600	34,800	55,800
No. Workers, share (2)	7,400	13,600	32,800	53,800

Biomedical = .0038 GNP = $4,066 millions

	Federal	Industry	Non-Profit Institutions	Total
No. Workers, share (1)	12,300	15,200	45,600	73,100
No. Workers, share (2)	9,700	17,800	42,900	70,400

Biomedical = .0050 GNP =$5,035 millions

	Federal	Industry	Non-Profit Institutions	Total
No. Workers, share (1)	16,200	19,900	59,900	96,000
No. Workers, share (2)	12,700	23,400	56,400	92,500

Estimates for 1985

GNP = $1,584 Billions

	Federal	Industry	Non-Profit Institutions	Total
Dollar Cost Per Worker	$83,100	$181,000	$84,000	
Percentage share (1)	12%	36%	52%	100%
Percentage share (2)	9%	42%	49%	100%
Biomedical = .0029 GNP =$4,594 millions				
No. Workers, share (1)	6,600	9,100	28,500	44,200
No. Workers, share (2)	5,000	10,600	26,800	42,400
Biomedical = .0050 GNP = $7,920 millions				
No. Workers, share (1)	11,400	15,600	49,000	76,000
No. Workers, share (2)	8,600	18,200	46,100	72,900
Biomedical = .0075 GNP = $11,853 millions				
No. Workers, share (1)	17,000	23,500	73,500	114,000
No. Workers, share (2)	12,900	27,300	69,100	109,300
Biomedical = .0100 GNP = $15,840 millions				
No. Workers, share (1)	22,700	31,300	98,000	152,000
No. Workers, share (2)	17,200	36,500	92,200	145,900

Intramural Research

The share of intramural research in the NIH budget has declined steadily over the years:

Table
(Total in Millions)

	1955	1957	1960	1962	1964	1966	1967*
Total NIH	81.3	213.0	430.0	771.6	974.5	1059	1413
Direct Research	22.9	34.1	49.9	69.7	71.1	82.7	90.5
DR/NIH	28%	16%	12%	9%	7%	8%	6%

*Including mental health, excluding environmental health

If the present structure of NIH intramural research is maintained[1/] we would expect to see its share of the total NIH budget continue to diminish as new programs gain significance. An unofficial NIH projection quantifies this expected trend; the over-all Federal share of money for the performance of biomedical research is expected to decline from 16 percent to 14 percent by 1975.

UNOFFICIAL PROJECTION FOR EXPENDITURES ON BIOMED RESEARCH

	Percent of Total			
Year	Total	Federal	Industry	Non-Profit Institutions
1967	100	16	30	54
1970	100	15	30	55
1975	100	14	34	52
1980	100	13	35	52
1985	100	12	36	52

The table below gives annual rates of growth for the three sectors--Federal, non-profit, and industry--given differing rates of growth for

1/In another section of this report we mention the possibility that NIH may find it desirable to carry on applied research efforts on an intramural basis.

the whole biomedical research industry. It is assumed that the projected 1975 distribution is in fact achieved.

Whole Industry Rate	5.0%	8.0%	15.0%
Federal Rate	3.2%	6.1%	12.9%
Non-Profit Rate	5.0%	8.0%	15.0%
Industry Rate	6.7%	9.8%	16.9%

We do not, however, mean to minimize the importance of the intramural program; it has received high praise from most observers. The relative decrease in its importance reflects the success of the NIH extramural experience, rather than any intramural shortcomings. And yet, partially because of the extramural program, the future of NIH intramural research is not assured.

One difficulty we foresee for the intramural program is that of maintaining the outstanding level of scientific competence of its investigators. From its early days, NIH has been able to attract top-flight scientists to its fine laboratories. The resulting high concentration of talented manpower itself became a magnet for scientific talent.1/ The development of other biomedical research centers has somewhat diminished the relative advantage of the NIH intramural program. This relative decline has been further accentuated because many of these other centers are now able to offer positions which concentrate heavily, if not exclusively, on research (see medical school section).

In this context, the erosion of NIH's competitive salary position is frequently cited as a source of danger. The chart below gives some suggestive statistics.

1/Many senior NIH investigators will be coming up for 20-year retirement in the next six years. We are informed that, given current practices, it will be most difficult for NIH to keep these individuals. If these scientists do leave, the intramural program will surely suffer.

COMPOUND RATE OF INCREASE PER YEAR IN SALARIES

		Period	Total Incr. Over Per.
NIH Direct Operations	5.6%	1956-66	73 %
Harvard Medical School			
Full Time Teachers			
Full Professors	2.3	1956-66	25
Associate Professors	3.4		40.5
Assistant Professors	4.4		53.5
Associate	3.4		40.5
Instructor	2.6		31
Ph.D.'s -All	4.9	1957-76	Not Comparable
Biology	6.1		
Chemistry	4.3		
Biology & Pharmacology		1957-65	Not Comparable
Starting Ph.D.	5.2		
Experienced Ph.D.	5.0		

We don't believe that these figures are conclusive in any way.[1/] However, they may indicate that the relative salaries of researchers probably have not changed too drastically to NIH's detriment over this period. Many analysts believe that the overall attractiveness of the research environment is more important to research scientists than is the salary incentive. It is in this area that the relative NIH position has deteriorated, though NIH is still the acknowledged leader in many areas of research.

The past dozen years have witness an enormous increase in our national capability for biomedical research. Where NIH was once predominant, there are now many well-equipped laboratories, staffed with

1/Good figures are not available for the extramural grants program. In a memo from Dr. Shannon to Dr. Philip Lee dated April 5, 1967, it was estimated that costs per employee and/or investigator in the grants program would approach 10% per year (versus 7.4% and 6.2% for the intramural program). Unfortunately, it is not possible to deduce salary figures, or % changes in salary, from this number. The contribution of other costs, % of time spent on NIH grants, and the academic rank distribution of grantees is not known.

talented researchers; and often, both laboratory and researcher are supported by funds from the NIH extramural program.

Yet, even today, the NIH intramural program occupies a position of special importance; the NIH Report to the President illustrates this point:

> The senior intramural staff is keenly responsive to Institute concerns over problem areas in its entire research program. Consequently, the intramural program serves in some measure to fill gaps in the extramural program. The intramural staff, for example, is conducting studies of bacterial species--a field in which interest has declined--because this work throws significant light on the mechanisms by which bacteria become capable of resisting antibiotics. The NIH intramural program has also played an important role in advancing the study of infectious viral agents that produce uniformly fatal disease and have incubation periods measured in years. This work requires large and expensive animal facilities, which few research organizations have available.

In other sections of this report, we have frequently suggested that biomedical research will involve a partnership with new fields--physics, chemistry, engineering--if the promise of the future is to fulfilled. The NIH intramural program must respond to this challenge if it is to retain its current important position in the biomedical research community.

Extramural Research

Our analysis above seems to indicate that the extramural program of NIH may have expanded so rapidly that the overall quality of the people in the field may have declined. Two major questions arise in connection with future funding of extramural research: (1) How fast should we expand the numbers of people in the field?

To be consistent with our philosophy we would have to say that the answer to this ~~first~~ question must depend in turn upon the amount of support given to other scientific fields, and the ability of biomedical science once put on a competitive basis to attract talented scientists. If, as we have indicated, biomedical research is somewhat overstocked at present, its relative share of the scientific labor market may decline somewhat in the short run.

This trend will be somewhat reinforced if we exclude from present consideration (we deal with them in our collaborative section) the increasing number of individuals, primarily in the industrial sector, who will be carrying out research on a contract basis.

NIH's extramural program has been remarkably successful. It is likely to go down in the history of science as a spectacular example of the ability of a government to secure on a continuing basis advice on scientific allocations from a larger percentage of the most talented scientists in an area. However, as the President, the Secretary of Health, Education, and Welfare and a host of analysts have emphasized the primary research program of NIH has come of age. The individual grant program has formed the core of past NIH research efforts. It is both natural and desirable that it receive diminished emphasis as NIH moves into new challenges. Why?

To a significant extent we are urging that the judgment on the appropriate portion of our scientific talent that goes to biomedicine be left to the decisions of the scientific community. There is little doubt that if future years witness a continued rapid growth of interest in biomedical research, Congress will respond by supplying the appropriate funds. However, we suspect that the growth rate of this field will be greatly diminished once it is put on a competitive basis with other sciences.

2. What percentage of the people in the field should be supported, and to what level?

This question gets us into all the intricacies of marginal evaluations. It is frequently said that in many scientific fields 90 percent of the good work is done by 10 percent of the people. The more abstruse and difficult the field, the more skew this distribution. In the cited example, the productivity per person of the most talented tenth is 81 times that of the remaining 90 percent. If one looks at only the weakest decile the number would no doubt be in the hundreds.

Unfortunately, there is no easy way to measure the output of different members of the biomedical research field.[1] We have asked some people involved in the area, and they indicate that the 90/10 illustration would not exaggerate the evidence with respect to the more basic work in biomedicine.

But Lord pick the pop?

However, the amazing fact is that the last, the most marginally funded researcher, receives nearly the same degree of support as does the individual whose project receives the highest priority of all projects considered by the Council. Both projects are checked to see if there is any area in which the request should be trimmed, but beyond this they are given equal treatment.

1/NIH, unfortunately, could not supply us with any studies relating, let us say, the success of projects with strong and weak priority scores.

**

PERCENTAGE OF REQUESTED AMOUNTS ALLOWED IN NEW RESEARCH GRANTS BY ADVISORY COUNCILS

	Allergy	Arthritis	Cancer	Child Health	Dental
1962	96.2	89.4	85.2	82.4	87.5
1965	88.1	88.7	80.1		85.3

	General Med. Sciences	Heart	Neurological
1962	87.8	91.6	94.7
1965		85.0	86.8

We believe that this is an unfortunate practice. Perhaps the Councils could make an effort to distribute some of their extramural funds on a restricted basis. Thus, for example, they might distribute the first 80 percent of their funds to the most attractive projects following current procedures. The remaining 20 percent of the funds could be allocated to the next 30-35 percent of the projects (assuming that that many had been approved), the projects in general receiving no more than two-thirds of their requested amounts. These reductions could be made in two ways:

1. Reducing the scope of the projects.
2. Reducing the period for which they are funded.

Quite likely, the Councils would usually wish to concentrate on the second procedure. This would, of course, have the beneficial effect of increasing institute flexibility in future years, but would not be so helpful in cutting current obligations. After "reduced grants" have run their shortened period, perhaps three year maximum, they could be resubmitted for further consideration following current procedure for renewals. This would mean further increasing the burden on the overworked study sections and requiring further paper work from grantees. However, we believe that these marginal grantees would likely benefit from the incentive of a second review and possible advice from the granting authorities.

The converse philosophy to the one now current is supported that our most talented scientists should be most heavily supported. This discrimination may well become a necessity with the increasing presence of Big Biology and the accompanying sky-rocketing costs for the latest equipment required for its conduct.

We have a further suggestion relating to future funding of extramural research. In our discussion of study sections we hinted at the desirability of an allocation system which would single out certain research areas as carrying high program relevance. These would be promising, underworked areas in which the advisory councils would want to encourage greater research efforts. This could be carried out through a formal bonus point system.

Projects in high priority areas would have points subtracted from their study section priority scores to put them in a better competitive position. Such a system might have the further benefit of directing our less talented scientists into high payoff areas, while allowing the most capable individuals to work where they wished.

One possible schema would establish the following guidelines:

1. Allocate 50 percent of funds without attention to priority areas.
2. Allocate the next 30 percent of funds, alloting bonus points to priority areas.
3. Allocate the remaining 20 percent of funds to 30-35 percent of the projects (bonus points counting).

The development of such a schema of this nature would mesh very nicely with the development of a program budgeting system. The underlying philosophies for the two are very similar.

Development of Manpower

In our section devoted to this topic, we point out that NIH has been extremely successful in its efforts to develop biomedical manpower. Unfortunately, the history of NIH experience in this area has not received the type of systematic analysis that would seem justified and could serve as a guide for future allocations. At present, the biomedical research area is better supported than any other major area of graduate study. High levels of support are necessary to bring people into a developing area. However, we would expect that an intellectually stimulating area should be able to attract an appropriate share of the graduate student market if it supported its students on a competitive basis.

Projections of graduate science enrollment over the next decade are quite rosy, it is expected to double from its 1965-66 total of 163,000. It seems unlikely that medical student enrollment will increase this quickly. However, it seems not too optimistic to hope that by 1975, m.d. production will be 40-50 percent higher than it is today. We should expect the research interests of medical students to continue to increase during this period. Further, increased programs of support for medical students may allow m.d. programs to compete for science-oriented students for whom medical school is presently prohibitive.

NIH, working in a tight budget, restricted its manpower training requests for fiscal 1968.[1] This looks like a wise and cautious move. We would urge continued restraint in this area until the basic medical and bioscience areas are put back on a competitive basis with other fields, We would also suggest that ~~into the training~~ careful study should accompany any decisions to move into the massive training of bioengineers.

Developmental Programs

It is xxxx quite difficult to make future projections about the growth of NIH developmental efforts. We consider this question in a separate Memorandum on the Future Growth of NIH, August 31, 1967.

Other

We did not devote attention to other parts of the NIH program. The principal components of the "other" section are construction, review and approval and program direction, and the Regional Medical Programs. We would expect all of these except for the last mentioned to grow at about the same rate as the sum of intramural and extramural.

In xx examining prospects for future growth of NIH, we believe that the Regional Medical Programs should be considered as a separate entity.

1/ There was no increase in the number of fellowships and a decrease in training grants.

Criteria for Decision

It is most difficult to measure, predict, or value the output of biomedical research. It, therefore, is not easy to employ any of the familiar decision making aids such as cost/benefit analysis or program budgeting. Quite understandably, NIH has been most hesitant to move into PPB techniques. This development, or perhaps better, lack of development is unfortunate in our estimation.

For the most part, allocation decisions in the biomedical research area have been made with the aid of quided intuition. The Nation has been most fortunate to have a truly outstanding administrator at the head of NIH. By all accounts, he has done an outstanding job of developing and running the NIH program. Given natural limitations, his institution has been well-guided. ?

By the same token, the very difficulties which make it difficult to apply PPB techniques to the health research area make it difficult to judge its performance. NIH has been very successful. However, there it no evidence that it could not have performed much better with the same funds, or perhaps nearly as well with significantly smaller budgets.

As best we can judge, the philosophy at NIH has been that where it is hard to apply analytic techniques they won't do you much good. The contrary philosophy is that analytic techniques have the most difficulty in the very areas in which non-analytic approaches are liable to break down completely. We would not suggest that anybody attempt to quantify non-quantifiable concepts, nor that quantifiable variables take precedence over those for which it is not possible to develop measures. However, we do feel that it is important that all allocation decisions be carried out in a climate of efficient choice. Concepts such as marginal analysis and opportunity cost rarely creep into NIH reports. agree

Many of their current requests and projections of future needs are made in the spirit of the requirements approach. It is also unfortunate that many areas of vital interest to NIH appear to have received very minimal study. For example, NIH has been the major force in training and supporting a manpower pool for biomedical research. However, they do not prepare figures or reports showing the past dynamics of the process.

It would seem that a powerful PPB organization at NIH could have a most beneficial effect, even if all of its reports were discarded before use. It might encourage the institutes and the NIH central administration to think about their programs in terms of efficiency, a factor which at present is not always considered.

Possibilities for Program Budgeting

NIH is currently employing a program budgeting system, but our impression is that it is more of a formality than a useful tool in program planning. The successful development and implementation of a program plan requires much thought and a thorough knowledge of the area. Lacking these, we can only present some limited thoughts on the subject.

Research

We would regard much of the research carried on at NIH as an overhead item, overhead to the biomedical research effort.1/ As such, its budget is liable to be somewhat aggregative and inexact. Any attempt to split all NIH research into disease categories would seem likely to meet with failure. Some worthwhile research projects would be

1/See Carl Kaysen's article in Basic Research and National Goals for further thoughts on the overhead technique.

difficult to categorize by disease area, much less by disease. We think a multi-level program budget might be a successful tool with which to classify research funds. The chart below shows how different projects could be programmed at different levels.

Disease Category	Disease	Facet
Major Disease Category I		
Project 1		
Project 2		
Project 3		
Project 4		
	Disease A	
Project 5	Project 5	
Project 6	Project 6	
		Facet 1
Project 7	Project 7	Project 7
Project 8	Project 8	Project 8
		Facet 2
Project 9	Project 9	Project 9
Project 10	Project 10	Project 10
	Disease B	
Project 11	Project 11	
Project 12	Project 12	
Project 13	Project 13	
Major Disease Category II		

Underlining shows primary category for allocation

In the chart shown, Projects 1, 2, 3, and 4 are of such general orientation that they must be programmed at the major disease cateogry level. Projects 7, 8, 9, and 10 are sufficiently focused so that they can be programmed at the facet level. Our program levels Major Disease Category, Disease, and Facet are illustrative and not suggestive. The point of our example is that like allocations (e.g., research grants) might profitably be programmed at different levels of aggregation. Indeed, we see no reason why the aggregation levels could not vary within different sectors of the budget. It would be most helpful if each of the institutes within NIH prepared a report which would suggest the appropriate divisions within its own sector. A coordinating PPB group could then develop these into an NIH program budget.

The multi-level approach is one way of getting at a two-dimensional program budget. It would make it possible to summarize data by funds allocated to different aggregation levels. In the cited example, this might give us an indication of the extent to which the research program is focused.

In our section devoted to distinctions between different forms of research, we emphasized the difficulties in distinguishing between applied and basic research in the biomedical area. For many purposes, we believe that those who wish to draw this distinction are really interested to discover the extent to which research is focused. Allowing NIH to program its research at different levels of aggregation would give us an indication of the extent of focus without involving them in the process of drawing difficult, perhaps impossible, distinctions. This would also get us away from the applied basic terminology which now alas has become loaded with unintended connotations.

There are other areas within the NIH program which suggest a two-dimensional or perhaps even multi-dimensional approach. Secondary questions of allocation, such as the division between laboratory and clinical, or among medical school, university, or hospital, may be handled via this technique.

It is difficult to adapt biomedical research to a program budget. Surely we should be willing to alter some current techniques of program budgeting to adapt the technique to the special needs of this area.

Elsewhere (see section on study sections) we discuss the problem of focusing research. Crudely stated, our conclusion is that freedom from focus should be a privilege for our most capable scientists. The development of a program budget which distinguished between more and less focused research, would seem to be of great assistance in this process.

The intramural and extramural programs of NIH are directed towards a common purpose. It would seem most reasonable that they should share a common program budget. Actually, the extramural portion might be somewhat more difficult to budget, since NIH has less control over the orientation of its projects and less day to day review of their progress. However, for these very reasons, it is probably more important to impose a program budget on the extramural research funds.

Collaborative Research

Before we have much practical experience, it is difficult to say how collaborative programs can best be budgeted. If the collaborative effort is small, and shares objectives with other portions of the research program they might well be incorporated into a common program budget. Even a large collaborative program which interacts with other institute projects in many areas might well be programmed with other research. However, we see no reason why a relatively self-contained project such as the artificial heart program need be incorporated into a program area with non-collaborative research efforts. It may be the case that some non-contractual research will be being conducted in a primarily collaborative field. In such a case, we believe that this research could be programmed in the primarily collaborative budgeting area.

Program budgeting, as such, is probably not so important for coherent collaborative programs. The general project may be sufficiently manageable that the day to day allocation of funds is not a problem of interest. What is essential is that the collaborative program have a well developed decision flow chart with which to plan and alter its allocations over time. We have been impressed by some of the documents the collaborative programs have developed along the PERT, decision flow chart lines. However, we believe that we might still learn a great deal about the dynamic planning of direct research if there were more coordination of the different collaborative programs.

Development of Manpower Resources

NIH bears the responsibility of overseeing and promoting the development of biomedical research manpower. More than a sixth of the NIH budget goes to its training and fellowship programs. The development of manpower resources is a dynamic, fairly long range effort. We think that it might well benefit from some form on centralized review planning procedure. To this purpose, we would think it helpful to have a budgeting area devoted to the development of manpower resources. It would, of course, still be essential to coordinate training programs with other parts of the NIH program budget.

Other Programs

NIH is engaged in numerous activities other than the ones we mentioned above. The most important of these are the Regional Medical Programs and the construction of health research facilities.

If, as in the Regional Medical Programs, the purposes are relatively distinct from those of other NIH efforts, we would imagine that they should be handled within a separate budgeting area. It is more difficult to say where support activities of the construction or review variety ought to be programmed. Some support programs might best be handled as separate entities; others might more reasonably be included within non-support program areas.

We conclude that program budgeting might well prove useful in allocating funds to biomedical research. There can be no doubt that research is not an ideal area for program budgeting. But, it is so unideal for any non-analytic approach that program budgeting is worth a try. Needless to say, much work and thought will need go into the development of a successful program budgeting format.

Suggested Future Studies

1. In the additive research field, a study might well be made which looks at the productivity of researchers of varying levels of ability, working on problems of different levels of applicability and importance. Quite specifically, we might wish to see whether our current research funds allocation system is optimal.

2. A thorough investigation of the dynamics of manpower development programs in the biomedical field should be undertaken. A cost/benefit analysis of different training procedures might be illuminating. This study might also look at NIH experience with investigators at different stages in their careers.

3. A cost/benefit analysis of the major collaborative programs at NIH should be undertaken. These should be compared with each other and with programs for the delivery of health services.

4. A detailed investigation should be made of the possibilities for developmental research, with particular attention to bioengineering. Topics to be covered should include manpower availability, industrial capability, and the need for Government support.

5. A separate, but related study should look into Federal mechanisms for supporting developmental efforts. Quite particularly, it would be of interest to examine:

   a. The potential for a non-profit research organization (à la RAND) or a Government operated laboratory for biomedical development.
   b. The desirability of establishing a centralized collaborative agency at NIH.

6. An effort should be made to develop a program budgeting system for NIH. This system need not employ individual diseases as the unit of allocation.

7. A brief effort on the relationship between Government programs and medical schools might clarify the needs of new schools and settle much of the uninformed controversy that goes on in this area.

Appendices

A. Satistics on NIH and Biomedical Research
B. Cost/Benefit Analysis in Medical Research
C. NIAID Special Emphasis Research Programs
D. Project Hindsight and Biomedical Science
E. New Sources of Knowledge*
F. Statistics on NIH Breakdown Among Basic Research, Applied Research and Development*
G. Materials on Training and Funding, NIH*

*Available only on first copy.

NATIONAL EXPENDITURES FOR MEDICAL RESEARCH, 1940-1967*
(In millions)

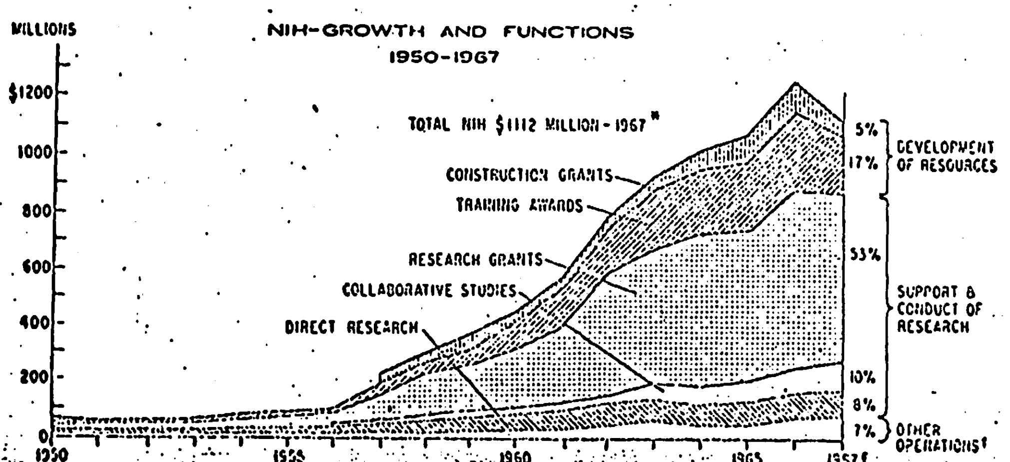
Source of funds	1940	1947	1957	1962	1963	1964	1965	1966 est.	1967 est.
Total	$45	$87	$440	$1,290	$1,455	$1,672	$1,841	$2,050	$2,275
Government	3	27	239	819	964	1,102	1,229	1,380	1,540
Federal	3	27	229	782	919	1,052	1,174	1,319	1,475
State and local	n.a.	n.a.	10	37	45	50	55	61	65
Industry	25	35	126	336	375	415	450	500	550
Private support	17	25	75	135	147	155	162	170	175
Foundations and health agencies	12	15	42	78	85	90	92	95	98
Other private contributors	n.a.	n.a.	5	18	21	22	25	28	29
Endowment	5	10	19	19	19	19	19	19	19
Institutions' own funds	n.a.	n.a.	9	20	22	24	26	28	29

*Covers only medical and health-related research; such activities as research training and construction are not included. Beginning with 1962, data for the non-Federal component have been improved and therefore are not strictly comparable with those for prior years.

CONSOLIDATED NIH APPROPRIATIONS, FY 1945-1967*
(In thousands)

Activity	1946	1950	1955	1957	1960	1962	1964	1965	1966	1967 Incl. Mental, excl. Envir.	1967 Incl. Envir., excl. Mental
Total NIH[†]	$3,020	$52,145	$81,268	$213,037	$430,003	$771,535	$974,454	$1,053,992	$1,244,435	$1,412,983	$1,111,833
Grants	850	30,979	54,331	133,541	304,433	573,993	753,569	787,197	920,853	1,019,337	826,471
Research grants	780	14,056	33,918	87,797	202,943	433,652	527,231	545,153	604,377	631,197	573,139
Regional medical programs	-	-	-	-	-	-	-	-	24,000	43,000	43,000
Research fellowships	45	1,412	2,562	5,397	14,370	29,050	45,735	48,965	56,330	60,123	50,990
Training grants	25	6,415	11,051	26,075	75,037	119,506	172,532	181,311	207,575	224,435	134,342
Mental health staffing	-	-	-	-	-	-	-	-	19,500	33,781	-
State control programs	-	9,050	6,800	10,275	11,875	17,750	10,950	6,750	6,750	6,750	-
Direct operations	2,170	15,372	26,937	47,453	95,570	137,547	159,835	183,793	217,553	257,646	229,417
Direct research	2,100	11,300	22,934	34,142	47,535	69,674	71,133	77,154	82,723	90,479	78,647
Collaborative studies[‡]	-	-	-	4,668	22,142	35,657	53,050	74,472	91,450	113,445	107,725
Biologics standards	-	-	454	1,672	2,805	3,050	4,787	4,959	6,806	7,934	7,934
Prof. & tech. assistance	-	2,533	1,850	3,137	11,555	11,953	3,935	4,192	5,637	7,370	835
Training	-	171	50	251	335	475	1,455	1,639	2,107	2,319	578
Review & approval	30	833	1,049	2,767	7,076	12,799	15,171	15,532	17,330	19,903	16,443
Program direction	40	383	600	784	1,769	3,779	5,265	5,835	8,763	12,195	11,353
Computer research and tech.	-	-	-	-	-	-	-	-	2,717	3,831	3,831
Construction grants	-	5,775	-	30,000	30,000	35,000	56,000	93,000	105,030	105,000[§]	56,000

*Data represent appropriations, not funds obligated. [†]Does not include direct construction or special foreign currency program. [‡]Data for years prior to 1962 represent chemotherapy contracts only. [§]Includes $50 million for Community Mental Health Centers.



* Excludes direct construction. [†]State control programs, biologics control, prof & tech asst., regional med. programs, training, review and approval, and program direction. [†]Includes Environmental Health Sciences, excludes Mental Health.

FEDERAL SUPPORT FOR MEDICAL RESEARCH, FY 1947-1967*

(Dollars in millions)

Agency	1947		1957		1962		1963		1964		1965		1966		1967 est.	
	Amt.	%	Amt.	%	Amt.	%	Amt.	%	Amt.	%	Amt.	%	Amt.	%	Amt.	%
Total	$27.0	100.0	$228.9	100.0	$782.1	100.0	$819.0	100.0	$1043.7	100.0	$1174.2	100.0	$1318.7	100.0	$1475.5	100.0
AEC	1.8	6.7	32.8	14.3	64.7	8.3	74.7	8.1	78.1	7.4	84.6	7.2	89.6	6.8	95.4	6.5
Agriculture	2.9	10.7	12.7	5.5	21.7	2.8	23.3	2.5	27.9	2.7	40.5	3.4	44.7	3.4	45.7	3.1
Defense	8.4	31.1	26.2	11.4	73.7	9.4	83.4	9.6	85.5	8.2	101.3	8.6	119.2	9.0	114.3	7.7
DHEW	12.2	45.2	142.3	67.2	562.1	71.9	541.5	67.8	752.5	71.8	825.2	70.4	928.3	70.4	1070.5	72.6
(NIH)	(8.3)	(30.7)	(125.4)	(54.8)	(415.4)	(53.3)	(506.0)	(61.6)	(651.0)	(62.1)	(715.1)	(60.9)	(790.9)	(60.0)	(813.9)	(55.2)†
FAA	–	–	0.0	0.0	1.8	0.2	2.9	0.3	2.9	0.3	2.8	0.2	2.5	0.2	2.7	0.2
NASA	–	–	–	–	13.6	1.7	34.2	3.7	41.2	3.9	60.0	5.1	75.4	5.7	79.9	5.4
NSF	–	–	3.4	1.5	15.3	2.0	21.1	2.3	23.1	2.2	19.7	1.7	13.8	1.0	14.4	1.0
VA	1.4	5.2	10.4	4.5	26.8	3.4	29.9	3.3	33.2	3.2	36.9	3.1	40.7	3.1	45.9	3.2
Other	0.3	1.1	1.0	0.4	2.3	0.3	2.9	0.3	3.3	0.3	2.4	0.2	4.7	0.4	5.4	0.4

*Covers only medical and health-related research; such activities as research training and construction are not included.
†Excludes mental health.

NIH RESEARCH GRANTS BY TYPE OF GRANTEE INSTITUTION, FY 1954-1966*

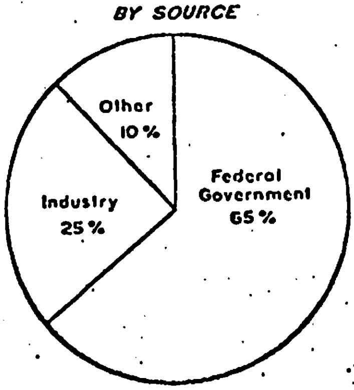
(Dollars in millions)

Type of Institution	1954	1956	1958	1960	1962	1964		1965		1966	
						Amt.	%	Amt.	%	Amt.	%
Total†	$28.9	$40.4	$101.3	$198.8	$352.1	$462.9	100.0	$494.8	100.0	$556.2	100.0
Colleges and universities	22.4	30.7	74.7	142.8	256.9	333.0	72.0	356.8	72.1	406.4	73.1
Schools of medicine	16.7	21.5	51.4	93.9	175.6	224.5	48.6	238.2	48.2	271.9	48.9
Other health professional schools	1.0	1.7	5.1	9.6	14.7	17.6	3.8	19.4	3.9	19.9	3.6
Universities (excluding health professional schools)	4.7	7.5	18.2	39.1	66.6	90.9	19.6	99.2	20.0	114.6	20.6
Hospitals	4.8	6.5	16.0	30.3	45.5	73.8	15.9	83.2	16.8	75.7	13.6
Private nonprofit organizations	1.1	2.6	6.7	14.2	25.2	30.6	6.6	29.5	6.0	53.7	9.6
Foreign institutions	0.2	0.3	1.8	5.9	14.1	12.9	2.7	10.8	2.2	10.0	1.8
All other‡	0.4	0.3	2.1	5.6	10.4	12.6	2.7	14.5	2.9	10.4	1.8

*Data for years prior to FY 1966 are based on reports of the Science Information Exchange. †Excludes general research support grants, $44.8 million for 1966. ‡Includes grants to government agencies, individuals, and industry.

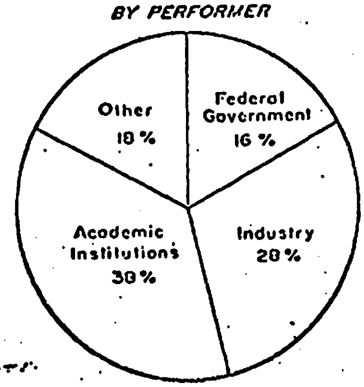
NATIONAL EXPENDITURES FOR MEDICAL AND HEALTH-RELATED RESEARCH 1967

BY SOURCE



$2.3 BILLION Est.

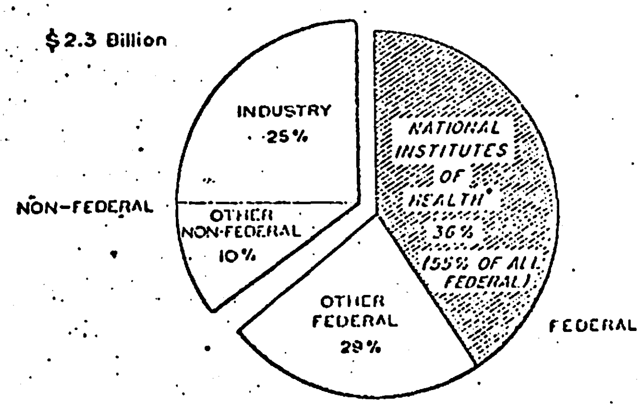
BY PERFORMER



$2.3 BILLION Est.

NIH FUNDS AS A PROPORTION OF NATION'S MEDICAL RESEARCH SUPPORT

$2.3 Billion



1967 Est.

*Excludes National Institute of Mental Health (4%).

APPENDIX B

Cost/Benefit Analysis in Medical Research

It is natural and desirable that cost/benefit analysis be applied to allocation decisions in the biomedical area. A common approach is that taken in A National Program to Conquer Heart Disease, Cancer, and Stroke, President's Commission on Heart, Cancer and Stroke. We present below a description of their approach elaborated in a recent monograph entitled, Federal Investment in Medical Research and Application.

The most obvious thing to do, it would serm, would be to relate medical applications to medical research, to specify some quantity of death and illness averted, and then place a value on the quantity of life produced by research. The value that suggests itself is the earnings from human productivity, for if medical research is capital, and the function of capital investment is to increase productivity, then human productivity made possible by research would appear to be a plausible measure of research capital.

This procedure has in fact been adopted by many economists working in the health area, and, for analytic purposes, the procedure will be used here. I have, however, serious reservations about measuring the value of life in terms of productivity for several reasons: it confuses ends and means; it is a fragile measure easily reduced or nullified by lowering retirement age or divorcing earnings from productivity; and for other reasons best left to the chapter on "Measuring the Federal Investment." Nevertheless, it may be worthwhile to assume that the measure of human value in terms of productivity has some validity, and then see what the assumption yields.

In the report of the President's Commission on Heart Disease, Cancer, and Stroke, the analysts estimated the direct and indirect costs of these diseases for the year 1962.[9] Direct costs include those for personal health services and medical supplies, and for impersonal services such as research and training. The summary table on cardio-vascular diseases and cancer is reproduced in Table IV-1. Of the estimated total cost of $43,085.9 million, the largest proportion of costs is that attributed to mortality in "previous years", which accounts for $33,075.5 million. The analysts explain their estimate as follows:

"The estimated total indirect costs to the Nation of cardio-vascular diseases and cancer amounted to $38.8 billion in 1962, rep-resenting 9.6 million man-years lost to gainful employment.

"Of the 9.6 million man-years lost, 4.3 million were for males and the remaining 5.3 million were for females. Due to the higher earnings for males, the dollar amounts associated with their losses were considerably higher---$24.8 billion compared with $14.0 billion for females. Losses among males were for those who have been in the labor force; among females the losses were mainly for those prevented from housekeeping. . . .

" Mortality in previous years comprises the largest share of the indirect costs of these diseases---approximately 85 percent of the total. Of the 33.7 million deaths due to cardiovascular diseases and cancer in the period 1900-1961, about 8.3 million would have survived through 1962 and worked or kept house if these major causes of death had been eliminated. In dollar terms, the loss of output amounted to $33.1 billion. In making these estimates, it was assumed that cardiovascular and cancer death and disability rates were zero while the rates for all other causes remained unchanged." (pp. 453-454)

It is not clear from the text whether the costs for previous years (earnings and imputed earnings) were converted to present-value dollars, and, if so, what discount rate was used. To simplify this analysis, I shall assume that the indirect costs of mortality and morbidity for 1962 are valid, and I shall exclude the costs for "previous years". To these are added the direct costs, $4,319.7 million, giving a total of $10,000.3 million. But since I am trying to determine the present value of research if these diseases were eliminated, the "nonpersonal services" under direct costs are subtracted....the entire amount, again for simplicity's sake. This leaves us with a cost of about $9,200 million in 1962 for cardiovascular diseases and cancer. Against this cost, which becomes a benefit if the cost is eliminated, is the cost of research for these diseases which is put at $244.5 million. Of this amount, the federal government accounted for about $195.5 million, and the NIH for $178.8 million of the latter. (p. 465, p. 488) (Earlier, I stated that allocations among diseases of research expenditures has little meaning because of the interdependencies of research; I am obviously relaxing this constraint in this analysis.)

Let us make the following assumptions: (a) that $9,200 million is the cost of these diseases in any future year; (b) that the elimination of these diseases is owing to the full success of research, as previously defined; i.e., research uncovers essentially costless preventives, so that there are no costs for diagnosis or treatment; and (c) therefore, the averted costs become the sole output of research, and all benefit values may be attributed to it. With these assumptions, we may then ask: In how many years would the full

Table IV-1. Estimated Economic Costs of Cardiovascular Diseases and Cancer, by type of cost, 1962, in millions of dollars.

Type of Cost	Total	Cardiovascular Amount	Cardiovascular % of Total	Cancer Amount	Cancer % of Total
Total	$43,085.9	$31,867.4	74.0	$11,218.5	26.0
Direct Costs	4,319.7	3,072.2	71.1	1,247.5	28.9
Pers. Serv. & Supply	3,500.4	2,579.7	73.7	920.7	26.3
Hospital Care	1,900.8	1,234.8	65.0	666.0	35.0
Nursing Home Care	322.9	299.8	92.9	23.1	7.2
Physician's Services	873.9	701.4	80.3	172.4	19.7
Drugs	310.1	279.4	90.1	30.8	9.9
Nursing Services	92.7	64.3	69.4	28.4	30.6
Nonpersonal Services	819.3	492.5	60.1	326.8	39.9
Research	244.5	117.0	47.9	127.5	52.1
Training	37.1	19.8	53.4	17.3	46.6
Other Health Services	73.3	34.4	46.9	38.9	53.1
Construction	275.4	190.0	69.0	85.4	31.0
Net Cost of Insurance	189.0	131.3	69.5	57.7	30.5
Indirect Costs	38,766.2	28,795.2	74.2	9,971.0	25.7
Mortality	34,781.4	25,824.6	74.3	8,956.8	25.8
1962	1,705.8	1,286.8	75.4	419.1	24.6
Previous years[a]	33,075.5	24,537.8	74.2	8,537.7	25.8
Morbidity	3,984.8	2,970.7	74.6	1,014.2	25.5
Institutionalized	507.9	455.9	89.8	51.9	10.2
Non-institutionalized	3,476.9	2,514.6	72.3	962.3	27.7

[a] Established losses in 1962 from deaths in previous years based on survival probabilities resulting from eliminating cardiovascular diseases and cancer, assuming the death and disability rates for each cause were zero while the rates for all other causes remained unchanged.

Source: The President's Commission on Heart Disease, Cancer, and Stroke: A National Program to Conquer Heart Disease, Cancer, and Stroke, Government Printing Office, Washington, D.C., Feb. 1965, p. 452, Table 5.

research success have to be realized, and at what rate of discount, in order for the present value of anticipated benefits to exceed current expenditures on research? As the present value calculations show in Table IV-2, if the interest rate is 6 percent or less, full success could occur 50 years from now, and benefits would exceed costs by at least twice the national expenditures on research into these diseases, and at least $2\frac{1}{2}$ times the federal expenditures as estimated by the President's commission. At 8 percent, the full research success would have to occur before the fiftieth year. At a 3 percent interest rate, benefits would exceed costs if the diseases were eliminated in 100 years.

One could look at these figures another way and say that, under the stated assumptions, research expenditures could have been expanded nationally from $244.5 million on these diseases to $460 million for zero "profits" at a 6 percent discounting, and up to $828 million at 5 percent. In terms of the Hirshleifer criterion---"every project with a benefit/cost ratio greater than 1 should be adopted" (p.138)---then the aggregate research expenditures in 1962 are easily justified, under the assumed conditions. But the assumptions leave much to be desired, so that the results of this analysis have extremely limited utility. We have ignored many benefits of medical research, simply because we cannot measure them; human life has been evaluated solely in terms of productivity; and we have imposed upon research the almost unbelievably difficult task of finding costless preventives for all cardiovascular and neoplastic diseases within a period of 50-100 years.

Table IV-2. Present Value $[1/(1+i)^n]$ of Medical Research When Costs from Cardiovascular Diseases and Cancer are Eliminated in Year X_t (t= 1, 5, 10...50). Total Value of These Diseases in Year t = $9,200 Million.

	Interest Rate						
Year X_t	1%	2%	3%	4%	5%	6%	8%
t=	Present value in millions of dollars						
1	9108	9016	8924	8832	8740	8648	8556
5	8740	8372	7912	7728	7176	6900	6256
10	8372	7544	6808	6532	5612	5152	4232
15	7912	6808	5888	5152	4416	3864	2944
20	7544	6164	5060	4232	3496	2852	1932
25	7176	5612	4416	3496	2760	2116	1380
30	6808	5060	3772	2852	2116	1564	920
35	6532	4600	3312	2300	1656	1196	644
40	6164	4140	2852	1932	1288	920	460
45	5888	3772	2392	1564	1012	644	276
50	5612	3404	2116	1288	828	460	184

This procedure is not intellectually consistent. We allocate resources to medical research and application not to expand our total GNP, but rather because we like good health and length of days. Assume for purposes of argument that health was of concern only so far as it related to wealth and was in no way a consumption good itself. Our interest then should only be in per capita income. When an individual dies, society gains to the extent that he does not use up resources in the future. Averting the death of an individual is only worthwhile to the extent to which he contributes more than he takes away. We see no reason, in general, to assume that this will be the case. If we eliminate mortality gains and losses from consideration, the outcome of the analysis changes radically. In the example cited above, net economic costs are reduced from $43.1 to $8.3 (morbidity costs are still relevant--assuming the morbid continue to use non-medical resources at the average rate).

Expenditures justified in the example would have to be reduced by 80% to be justified on this corrected basis. On most reasonable assumptions, this program cannot be justified on purely economic grounds.

However, as we mentioned above, the primary gain is the increased health of the nation. We live in a wealthy, developed nation, and we can afford to treat health as a consumption good. The value of eliminating heart disease, cancer, or any other illness is the amount that individuals in our nation would be willing to pay. One would expect that most individuals would pay more than their <u>individual</u> expected loss to eliminate a particular illness.* This would mean that the numbers given in mortality costs above would be an underestimate of the personal values for eliminating the disease.

We might mention two related errors frequently made by cost/benefit analyses in this area.

1. Analysts are sometimes presented with complete programs for analysis. If program A has a higher benefit/cost ratio than B, it should be undertaken before B, but it is possible that a mixture of A and B is better than A alone. At the optimum, marginal not average benefit/cost ratios are equalized.

2. It is not unusual to see figures relating $ of research expenditure on different diseases to their incidence. These comparisons are meaningless. There is no reason to expect that the figures should be even roughly proportional. If all research dollars were equally likely to cure a disease, we should spend all our funds on the disease with the greatest incidence. In the jargon of economics, we should allocate ~~the~~ our funds so that the

{increased probability of cure from a marginal dollar on research} x (incidence of disease)

is equalized for all diseases.

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*Here we mean total loss, not income per year. If an individual If an individual earning $5,000 per year has a 20% chance each year of contracting a fatal disease, we are positing he would be willing to pay $1,000 per year to ward off the disease. Clearly, this prevention program reduces his yearly income.

NIAID SPECIAL EMPHASIS RESEARCH PROGRAMS

June 5, 1967

In previous years the strictures of limited research grant funds encouraged this Institute to develop, with the help of Council, the concept of "areas of high program relevance." This concept permitted the intrinsic scientific merit to be supplemented by a fiscal priority of payment when the grant application was judged to have high program relevance. The experience gained in developing program priorities and the current estimate regarding FY 68 funding now substantially lessens the importance of fiscal priority and stimulates the Institute to take an even more active programming position in certain areas.

Active programming requires a considerable commitment of staff time and it is probable that only the major research problem areas can be so treated. It is planned therefore to focus special attention only upon certain of the former areas of high program relevance. These will be called "Special Emphasis Research Programs" and they are listed below. In the text which follows is a brief expository paragraph that attempts to indicate the major problems in each program together with specific problems that stand in the way of development of instruments of control.

Drug Resistance and Microbial Diseases
Streptococcal Infections and Sequelae
Congenital Defects Caused by Microbial Agents
Antiviral Substances
Chronic and Degenerative Diseases of Microbial Origin
Infectious Hepatitis
Emphysema and Chronic Lung Disease
Transplantation Immunology
Clinical Allergy and Immunology
Malaria

Some of these program areas have already been the subject of expert committee discussion and analysis (Chronic and Degenerative Diseases, Emphysema, Transplantation Immunology); a follow-up committee meeting is scheduled for emphysema; and a first meeting of a committee on drug resistance (with specific attention to gram negative organisms) is being planned.

What follows is another description of the same program. It is from Advancement of Knowledge for the Nation's Health.[23]

Special-Emphasis Research Programs

There are eight special-emphasis research programs that the Institute and its advisors have selected for increased attention from the multitude of possible research projects in the Institute's total mission. A number of interlocking considerations enter into this selection. The area should be of current acute public health importance and one in which recent scientific developments indicate the possibility of rapid progress (e.g., chemotherapy of leprosy) or where a significant disease problem is being overlooked (e.g., emphysema) or is not attracting sufficient talent because essential techniques are lacking (e.g., hepatitis) or because professional rewards are apt to be lacking (e.g., chronic and degenerative diseases of microbial origin). The Institute also strives to be alert to developments (e.g., tubercle bacillus cell-wall vaccine) which promise improved public health but which if left unattended, like the discovery of penicillin, might remain for many years unexploited for public benefit.

The goals of these programs are general: to focus the attention of scientists on specific areas; to encourage and support research by grants and by intramural projects; and to assist in the exchange of information so that specific deficiencies will be highlighted and opportunities for research recognized. Each program is under the continuing surveillance of at least one professional staff member, and each receives periodic general attention from advisory groups, particularly the Institute's National Advisory Council. Any of these programs could be expected to become a nationally organized research program when the general scientific base is adequate to permit the formulation of specific goals, to have developed essential techniques and to delineate developmental pathways.

APPENDIX

Project Hindsight and Biomedical Science

Project Hindsight is a DOD sponsored study of "recent science and technology which has been utilized by the Department of Defense in weapon systems."[1] The method used in Hindsight involves using teams of scientists and engineers to examine recent weapon systems. They try to identify "each contribution from recent science and technology which, in their judgment, is clearly important either to increased system performance or to reduced cost, compared to a predecessor system when such can be identified."[2] Once such a scientific "event" has been identified, someone on the team investigates to discover "the principal contributors, the organizations with which they were working at the time when the work was done, the date when the feasibility or practicability of the idea was first demonstrated, the nature of the work (science or technology), theobjective of the work, the approximate cost, the funding sources, etc."[3]

Hindsight found that 96% of the eevents examined were funded, directly or indirectly, by DOD. It also found that 95% of all events were motivated by an "understood" DOD need. Hindsight therefore concluded that undirected research since 1945 (as far back as the study went) has made relatively little contribution to defense needs. This conclusion, however, stimulated considerable criticism and protest from scientists who felt that basic research was under attack.

A hard look at Project Hindsight produced a number of significant criticisms. It was argued that basic science may, and often does, contribute to the "events" observed by Hindsight; but since Hindsight doesn't look that deeply, the contribution is missed. In fact, it would be very difficult to evaluate such subtle contributions; nevertheless, it can be convincingly argued that without the contributions of basic science, many of the events observed would not have taken place, or would have been delayed.

Another argument is founded on the premise that the best training for scientists, pure or applied, is basic research. Many of the investigators who contributed to events studied by Hind sight received their training in basic research, and without basic science, especially in the universities, there would be no applied scientists. Another aspect of this argument is the value of cross-fertilization between basic and applied science, which keeps both areas vigorous.

Project Hindsight does not deny the validity of such criticism--indeed, it implicitly accepts the value of basis research, without attempting to measure it. Hindsight has limited objectives: First, "to identify and firmly establish management factors for research and technology . . .;" and second, "to measure the overall increase in cost-effectiveness in the current generation of weapon systems. . ." In these terms, Hindsight is an excellent piece of research. And it is not unnatural to ask whether we can apply the type of analysis used in Project Hindsight to the biomedical area.

A biomedical Project Hindsight might look at any of a number of questions. It might parallel the DOD study by looking at improvements in health care, tracing the research and development events which contributed to the improvement. This kind of a study might give NIH enough information to make some kind of a cost-benefit comparison between various kinds of research, at least as far as health-delivery objectives are concerned.

On the other hand, such a study would probably be fraught with difficulty. One problem would be determining whether particular research was directed or undirected, applied or basic. It would also be difficult, perhaps very difficult, to pin down "events" in the DOD meaning of the word. A project as ambitious and expensive as Hindsight is probably not desirable for NIH. However, some effort of this sort--perhaps a small pilot project--might well be valuable. NIH has not, to our knowledge, carried out any substantial or formal investigation of the profitability of different areas of research. A small-scale study which would help NIH manage and allocate its research funds would seem to be worth the money.

"We would be hesitant to predict the outcome of such a study. The following comments from an analysis of Project Hindsight may be of interest.

An extremely informal survey among doctors suggests that a medical Hindsight, conducted by NIH or HEW, might yield results similar to those of Hindsight on the applied-nonapplied question. More lives would be saved or lengthened by applied research (e.g., drug-evaluation) than by nonapplied work (e.g., research on the genetic code), though some doctors were not