

Why Are Research Grant Applications Disapproved?

Characteristic shortcomings of rejected applications to the National Institutes of Health are described.

Ernest M. Allen

In the 12 months that ended 30 June 1959, the National Institutes of Health received and acted upon nearly 6000 competitive applications (1) for grant of funds to initiate or continue projects in medical and related biological research in the many research institutions of the country.

Of this number, approximately 2000 met with disapproval at the hands of the 30-odd advisory boards of scientists that give the applications technical review. These boards, known as study sections, are made up of distinguished scientists, active in research, who are connected with universities and other research institutions throughout the country.

That these "juries" of their peers are forced to render a verdict recommending disapproval of the grant applications of so large a number of scientists in a year's time is a matter of concern to those who contemplate it. Year after year the phenomenon continues to present itself, however, and even the most experienced scientific investigators may occasionally suffer.

We may liken the verdict of disapproval by a study section following its study of a research proposal to the decision by a group of medical consultants that a case before them is one of a certain acute illness. Illnesses have symptomatology and epidemiology. What is the epidemiology of the disorder we are considering, and what is its symptomatology? An epidemiological study is often a long-range and time-consuming undertaking, and is mentioned here only to emphasize a need. Symptomatology can, however, be described from a few collected cases, and it is the

purpose of this article to indicate the symptomatology as displayed in a sample group of "disapprovals."

The sample consisted of 605 applications for grant of funds to initiate or continue research which were disapproved (2) in the round of study-section meetings in the spring of 1959. Thirty-three study sections were involved, representing the many different areas of research support in the over-all field of medical and related biological research covered in the National Institutes of Health's research grants program. The adverse comments made on these 605 research proposals prior to the vote on each are summarized in the minutes of the various study sections. These minutes have furnished the material for the analysis reported here.

As would be expected, a round-table discussion of a research proposal by from 15 to 20 scientists in the same general field, before vote on the question of approval, is not a mere voicing of stereotyped phrases. When a shortcoming of a research proposal is commented on, however, it is easy to decide whether it concerns the *problem*—the question the proposed research would seek to answer—or the *approach* by which the answer is to be sought, or the *competence*—the total of scientific judgment and technical skills—that it is proposed to mobilize toward the pursuit of the research. These three categories, with a fourth that has been called simply "other," have been made the basis for a primary classification of all shortcomings that were commented upon in the study-section minutes dealing with the 605 disapproved (2) research proposals. The results of this initial classification may be seen in the four italicized subcaptions in Table 1. It is seen that in 58 percent of the

cases of the "disorder" we are considering, there are symptoms in the area of the problem: its importance or timeliness, in the general scientific area covered by the NIH program of research support, is not sufficient to warrant expenditure of NIH research funds on it. In 73 percent the approach is involved: the method of attack, as proposed, will not yield sufficiently useful data. In 55 percent the disorder manifests itself in inadequacy in the scientific competencies to be brought to bear upon the research, and in 16 percent there are other, miscellaneous manifestations.

In setting up these four classes it is recognized that failure to choose a meritorious or timely problem (class I) and failure to plan an adequate approach (class II) are reflections upon the investigator's judgment or the adequacy of his information regarding recent advances in the chosen area of research or in collateral areas. To combine class I and class II criticisms and put them in class III would, however, defeat the purpose of spreading out the spectrum of criticism to permit examination of the intensity of each portion.

In view of the fact that before a decision is made to undertake or continue a federal project of any kind, not excluding research projects, the proposal must be measured against the criterion, "Is this project necessary?," the absence of the specific criticism of lack of necessity may be surprising. If the specific criticism appeared at all, it would fall in class I. Clearly, no research can be regarded as necessary if it is "not likely to yield any new or useful information" (Table 1, criticism No. 1). The same may be said for the remaining adverse criticisms that make up class I. One or another of these was made in connection with 58 percent of the 605 applications. It can be said, therefore, that a verdict equivalent to "this research is not necessary" has a high rate of occurrence among the reasons for disapproval given in the study-section recommendations. If, on the other hand, the proposed research has *scientific merit*, if the approach is good, and if the investigator and his associates in the project can supply the necessary competence, then, in an era when scientific advance is clearly necessary, the project, promising as it does to contribute to such advance, must itself be deemed necessary. This is particularly true of basic research: needed "break-

Dr. Allen is chief of the Division of Research Grants, National Institutes of Health, Bethesda, Md.

throughs" may come from it in almost any direction.

It should be emphasized that the criticism of lack of "competence," as the term is used in class III, does not necessarily mean that the investigator proposing the research is not a good scientist. The lack of adequate competencies could be simply a lack of sufficient acquaintance with the recent literature bearing upon the proposed research, or, say, failure to include provision for the collaboration of a well-trained biochemist in a proposed research study in which some advanced biochemical technique is called for.

The shortcomings pointed out by the study sections are given in Table 1, arranged under the four category headings. There are 26 entries in the list. Each of the 26 is a blend of closely related but variously phrased criticisms. The phraseology adopted in each instance can be regarded as a sort of verbal mode or mean for the group of criticisms it stands for. It is believed that the "averaging" has in no instance distorted significantly the meaning of any of the criticisms as actually phrased in the study-section records.

Just as the percentages corresponding to the four main classes add up to more than 100 percent, so also those within any one of the four groups add up to more than the percentage for that group. In both cases the excess is due to the fact that a given research proposal may have more than one adverse characteristic. For the 605 applications, one or another of the 26 criticisms occurred to an over-all total of 1558 times.

It may be noted that the criticisms are not mutually exclusive and, also, are not of the same order of either comprehensiveness or importance. Criticism No. 11, for example, is general enough to embrace Nos. 13 and 15. The criticisms far down the list in each of the first three classes are clearly of less importance than the leading items and in some instances would obviously not in themselves warrant disapproval of an application. Such differences are to be expected when the basic material for the analysis is, as here, taken from a summary record of free and unconstrained discussion of a research proposal.

The leading item in each of the first three categories is equivalent to "unqualified" or "not suitable." The problem is not qualified to be included among meritorious research problems

(No. 1); or the approach is not suitable for this specific problem (No. 8); or the man is not qualified to conduct this specific research (No. 17). The second item (No. 9) under *approach* is comparable, except that it indicates that the applicant has failed to give enough information to permit the study section to arrive at a decision on whether or not the approach is suitable. Nebulous description of the problem (No. 7), on the other hand, is rare. Apparently the investigator finds it easier to define sharply the goal of his proposed research than to bring into sharp focus the route he proposes to follow. Most of the other items in each of the three large categories are equivalent to "partially unqualified."

Item No. 3, "the problem is more

complex than the investigator appears to realize," might with good reason have been placed in class III instead of class I.

The criticisms that occurred in the discussions summarized in the study-section minutes were, of course, voiced primarily in an effort to arrive at a balanced judgment of the merits of each research proposal, not to aid investigators in perfecting their conceptions or description of research proposals. Some criticisms—for example, "the approach lacks scientific imagination" (No. 12)—would be of little help to the investigator if they were relayed to him. It is believed, nevertheless, that the total list of 26 adverse characteristics—the "symptomatology" for disapproved research projects—could well be

Table 1. Shortcomings found in study-section review of 605 disapproved research grant applications, April-May 1959. All percentages are to the base number 605.

No.	Shortcoming	%
<i>Class I: Problem (58 percent)</i>		
1	The problem is of insufficient importance or is unlikely to produce any new or useful information.	33.1
2	The proposed research is based on a hypothesis that rests on insufficient evidence, is doubtful, or is unsound.	8.9
3	The problem is more complex than the investigator appears to realize.	8.1
4	The problem has only local significance, or is one of production or control, or otherwise fails to fall sufficiently clearly within the general field of health-related research.	4.8
5	The problem is scientifically premature and warrants, at most, only a pilot study.	3.1
6	The research as proposed is overly involved, with too many elements under simultaneous investigation.	3.0
7	The description of the nature of the research and of its significance leaves the proposal nebulous and diffuse and without clear research aim.	2.6
<i>Class II: Approach (73 percent)</i>		
8	The proposed tests, or methods, or scientific procedures are unsuited to the stated objective.	34.7
9	The description of the approach is too nebulous, diffuse, and lacking in clarity to permit adequate evaluation.	28.8
10	The over-all design of the study has not been carefully thought out.	14.7
11	The statistical aspects of the approach have not been given sufficient consideration.	8.1
12	The approach lacks scientific imagination.	7.4
13	Controls are either inadequately conceived or inadequately described.	6.8
14	The material the investigator proposes to use is unsuited to the objectives of the study or is difficult to obtain.	3.8
15	The number of observations is unsuitable.	2.5
16	The equipment contemplated is outmoded or otherwise unsuitable.	1.0
<i>Class III: Man (55 percent)</i>		
17	The investigator does not have adequate experience or training, or both, for <i>this</i> research.	32.6
18	The investigator appears to be unfamiliar with recent pertinent literature or methods, or both.	13.7
19	The investigator's previously published work in <i>this</i> field does not inspire confidence.	12.6
20	The investigator proposes to rely too heavily on insufficiently experienced associates.	5.0
21	The investigator is spreading himself too thin; he will be more productive if he concentrates on fewer projects.	3.8
22	The investigator needs more liaison with colleagues in this field or in collateral fields.	1.7
<i>Class IV: Other (16 percent)</i>		
23	The requirements for equipment or personnel, or both, are unrealistic.	10.1
24	It appears that other responsibilities would prevent devotion of sufficient time and attention to this research.	3.0
25	The institutional setting is unfavorable.	2.3
26	Research grants to the investigator, now in force, are adequate in scope and amount to cover the proposed research.	1.5

used as a check list for criticism of grant applications by the investigator himself prior to their submission, no matter to what granting agency.

Summary

A list is given of 26 shortcomings mentioned repeatedly in study-section discussion of 605 research grant applications that were subsequently disapproved (2) (by vote, following the dis-

cussion). The shortcomings have to do with either (i) the conception of the research problems, or (ii) the proposed route of approach toward their solution, or (iii) the competencies to be mobilized toward prosecution of the research, or (iv) miscellaneous other matters. The percentage frequency of applications (in the total of 605) in which a given shortcoming was found is reported for each of the 26. Certain of the items in the list are discussed briefly.

Notes

1. Competitive applications are (i) applications requesting support for a new research project not previously supported by the NIH, and (ii) applications requesting *continuation* of support beyond the existing term of commitment. The "nearly 6000" applications referred to in the text consisted of 4600 proposals to initiate and 1200 to continue research; also, but not included in this analysis, there were 4500 awards of an additional year's support granted on study-section recommendations *previously* made and 650 (competitive) applications for additional funds to supplement awards already made.
2. Disapproval of an application by a study section has the force only of a recommendation to one of the National Advisory Councils. Council acceptance of the recommendation makes the disapproval official.

Charles Oberling, Research Worker on the Nature of Cancer

Charles Oberling, director of the Institute for Cancer Research Gustav Roussy of the University of Paris, was fortunate in his upbringing as scientist and man. It shaped him, as if purposely, to fight against the most challenging of diseases and to enjoy his life.

Oberling was born in 1895 in Metz, but his father—a postal clerk whom he ever remembered with admiring love—soon moved the family to Strasbourg. Here Charles was educated, gained entrance to the university, and studied medicine. But not without interruption. He joined the army in 1914, was seriously wounded twice, and only in 1920 became a doctor. Then fortune favored him again. The university had as professor of pathological anatomy the renowned cytologist Pierre Masson, inventor of revealing histological methods, who took Oberling on his staff. During eight years with Masson, Oberling received an intensive training in cytology and through this was enabled to discover new facts about the nephroses and the reticuloendothelial system, and to obtain a firsthand knowledge of tumor cells. He was assistant professor at Strasbourg when, in 1928, he was asked to become an associate professor of the Faculty of Medicine of Paris in a division dealing with histology, embryology, and pathological anatomy.

It happened that professor Gustav Roussy, who headed the division, was an experimentalist, ardently concerned with founding an institute for cancer research. Soon he had Oberling experimenting with him on the tumors of laboratory animals. In another two years he had achieved his institute, and Oberling was made its *chef de service*. Together they published a definitive atlas on the growth of the human central nervous system, but Oberling's work was now mostly experimental. He found out much about the transplantable growths of small mammals and the virus-induced tumors of fowls, and in addition he showed such ability as teacher and organizer that he was called to Strasbourg in 1937 to succeed Borrel in the chair of hygiene and microbiology.

Amédée Borrel is now a well-nigh legendary personage to French scientists concerned with the actuation of tumors. He it was who first ventured the view, in 1903, when little was sure about viruses, that they are the cause of tumors, and for this view he sought and fought throughout his later life. The times were hostile to it, for the hopes aroused in the Pasteurian epoch that tumors would prove to be due to microbes had been destroyed by tests of the newly transplanted growths of

the rat and mouse: these growths yielded no other cause, on transfer, than their own living cells. Realizing through his own experimentation that this was so, Ehrlich had quit the tumor problem for others that he could solve.

Oberling's new academic duties required that he learn far more than he taught, and in strange fields. Hence, he did little on cancer before World War II began. By that time he had such a reputation as hygienist that late in 1939 he was asked by the Shah of Iran to reorganize medicine in Teheran and to try to make the city a more healthy place. Toward these ends he would be appointed dean of the Faculty of Medicine. He was working on war gases, but he was advised by the French Government to accept the offer. During two years in Iran he reformed the medical curriculum in the university, converted the city hospitals into university centers staffed with the best specialists, founded a school for nurses, and greatly improved sanitation throughout the immediate region. Then, with his task carried out, he journeyed in 1942 to the United States to undertake cancer research anew, but upon landing was again asked to come to Iran, this time to improve health throughout the country.

On the way back he had the first of the cardiac "strokes" which were to harass him throughout his remaining 17 years. Feeling unequal now to the task in Iran and unwilling to return to an occupied France, he brought his wife and boys to the United States and tried to enter its army. He was rejected because of his medical history, and so he became pathologist-in-chief at the Mary Imogene Bassett Hospital in Coopers-town, New York. The directors of that enlightened institution could not have known that in appointing him they acted to further cancer research even