CROSS REFERENCE

FILE: TRUSTEE - Committees

Faculty-Trustees Study Committee

RE:

Institute Policies and Future

LETTER DATED:

SEE:

Institute General

Faculty-Trustee Study Committee

CORRECTIONS AND ADDITIONS TO DRAFT INTERIM REPORT OF THE JOINT FACULTY-TRUSTEE STUDY COMMITTEE:

1). page 7, line 2, delete comma

2). page 7, line 5, "relation<u>s</u>"

3). page 21, line 14, "Faculty <u>merely</u> because"

4). page 21, line 15, delete comma after "needed"

5). page 22, line 6 from bottom, semicolon for comma after "solution"

6). page 27, new item 6:

6. On what grounds, and by what means, should we seek new endowment? What is the relevance of the provisions of the new tax legislation?

THE INSTITUTE FOR ADVANCED STUDY

PRINCETON, NEW JERSEY

OFFICE OF THE DIRECTOR

9 November 1955

To the Trustees of the Institute for Advanced Study:

At a meeting of the Faculty of the Institute for Advanced Study on November 8, 1955, the Faculty expressed approval of the joint committee to study and report on the problems of the Institute's resources and program; and named Professors Harold Cherniss, Freeman Dyson, Marston Morse and Erwin Panofsky to represent them on the committee.

The first meeting of the committee is planned for November 28th in Princeton.

Robert Oppenheimer

Robert Oppenheimer

THE INSTITUTE FOR ADVANCED STUDY

PRINCETON, NEW JERSEY

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THE INSTITUTE FOR ADVANCED STUDY
PRINCETON, NEW JERSEY

OFFICE OF THE DIRECTOR

14 December 1955

Dear Mr. Maass:

It seems to me that I had best report directly to you the status of Dr. Jule Charney. This will importantly affect, though in ways that are not now completely clear to me, some of the future programs of the Institute.

Dr. Charney is 38 years old. He is a leader in applying new methods of numerical calculation to meteorological problems. He and von Neumann developed methods for the prediction of large storms; he has developed a theory of the Gulf Stream; and he and Phillips have made the promising beginnings of a theory of the general circulation of the atmosphere and dynamic climatology.

Dr. Charney first came to the Institute on von Neumann's invitation in 1948. He has been here ever since as a member--and since von Neumann's departure as Acting Director--of our project in meteorology. This project is supported almost wholly by a Navy contract (Nonr 1358(02)); the annual sum is approximately $75,000. The work of this group uses approximately one third of the effort of the electronic computer. Fast computation is an essential part of the methods developed by Charney. A few years ago von Neumann suggested that we consider giving Charney a permanent, and perhaps a professorial appointment in the School of Mathematics. He pointed to the fact that Charney's work was not only successful, but increasingly well known, and that unless Charney were taken into the Institute family and given assurances of future support, he would probably choose to go elsewhere. Strong opposition to this proposal developed on the part of several of the pure mathematicians, on the ground that they did not think that Charney's field warranted Institute support. At that time, all we could agree on was that embarking on the permanent support of meteorology and associated subjects called for additional endowment, since it was a new departure academically. I so reported to the Trustees. The matter was raised again last year, and I attempted to resolve the differences between those in the School of Mathematics--especially the physicists--who approved the professorial appointment for Charney, and those who were opposed to this course. We were not able to reach agreement, and in the absence of new endowment, or of a clear decision of policy on the part of Faculty and Trustees, the matter was let lapse.

This autumn Charney told me that he had been kept informed of his situation by von Neumann, and that he could not indefinitely postpone making suitable and stable arrangements for his future. The contract on which he is now operating, and the appointment as a member of the Institute, will both lapse with the end of 1956. Last Friday Dr. Charney told me that he expected shortly to have suitable invitations from three universities at which work in theoretical meteorology is being supported: New York University, the Massachusetts Institute of Technology, and the University of California at Los Angeles. He told me further that his colleague Dr. Phillips would go with him to the place of his choice; that he would like to make his decision shortly--not later than January of 1956; and he would feel obligated to tell his associates in the project of his decision in order that they might make their plans for the future. He explained to me that if he had an offer from the Institute, he would still be very much tempted to stay here because of the many advantages, but that he felt he could no longer postpone his decision on the basis of uncertain suggestions about our possible future course. I told him that I would explore the possibility of resolving the matter quickly and affirmatively, and that unless I saw prospects that this could be done I would understand that he would accept one of the forthcoming invitations.

I have now talked separately with all members of the Faculty of the School of Mathematics, including von Neumann, with whom I talked on the telephone. The Faculty of the School is about evenly divided between those who would wish to see us offer Charney a professorship, and attempt subsequently to solve the problem of the permanent support of his project, independent of year to year government subsidies; and those who were firmly opposed to taking this step at this time. It seemed to me clear that positions were so firmly held that nothing resembling unanimity could emerge from collective discussion, and I have therefore concluded that the School will not recommend to the Faculty, and cannot recommend to the Trustees, that we offer Charney a professorial appointment.

Opinions clearly may differ as to the wisdom or folly of this course. Charney and Phillips are the leaders, and the only quasi-permanent scholars of distinction in the meteorology project. They also provide an important, if not the most important field of application of the electronic computer project. Thus, what is happening will have a profound bearing on the Institute's project researches, on our views of the future relations between the computer and the Institute, and to some extent on the decisions which we will be free to take about the scope of our work in the mathematical sciences. It is for this reason that I have written to you at such very great length. I should of course, as always, be glad to discuss this matter with you if you should wish to.

Faithfully yours,

Robert Oppenheimer

Mr. Herbert H. Maass
100 Park Avenue
New York, New York

Draft of a letter to Mr. Hochschild, with copies to members of the Trustee-Faculty Study Committee:

Dear Harold:

Thank you for the copy of your note of February 20th to Harold Cherniss; I am very glad that you wrote it. It brings to a sharp ~~point~~ focus one of the matters on which I most wished to solicit Committee discussion and deliberation, and to which we must turn our attention with great seriousness if we are to do our duty.

It is true that in the work at the Institute today sociology and education, and the other set of subjects, would appear primarily only in their historical meaning, and ~~in a~~ historical context; but the reason that we do not have a professor of education or sociology is not because our work is organized in two schools, a School of Historical Studies and a School of Mathematics. That we could change, and should change, in the interests of doing the best possible job, if we could carry out ~~the~~ our mandate to conduct advanced study, in education or sociology.

I do not want to labor the words "advanced study" too often. It is in our charter, and it is our name. It does stand for something: it means study which not only requires intelligence and imagination and devotion, but a long period of learning, ~~and~~ of the acquisition either of technique or of background. In this sense I have come to the reluctant conclusion that the subjects mentioned in the third complete paragraph of page 2 of the Cherniss aide memoire are not now subjects suitable for, or susceptible of advanced study. I have read widely in many of these subjects; they interest me as a layman and a student. I may nevertheless be wrong.

It is precisely to this point the Committee needs to direct its attention, and on which we should seek a community of understanding and an ability to explain our differences, if they do in fact exist.

I am glad that Cherniss recorded the conversation as it did occur, and I hope there will be other conversations which do come to grips with this question.

Faithfully yours,

Do not do or not do for commendation or money.

Don't save the world

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deductibility 20-30%

50% increase in HS Faculty in 5-10 yrs.

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THE INSTITUTE FOR ADVANCED STUDY
Princeton, New Jersey

C O P Y

February 22, 1956

Dr. Robert Oppenheimer,
Director,
Institute for Advanced Study,
Princeton, N.J.

Dear Robert,

Among the points that have been discussed in the meetings of the Joint Committee of Faculty and Trustees the following are some that I should think it proper and important to develop in the final report of the Committee to the Faculty and the Trustees. As I express them here they are in some respects no doubt too general, and I think that the Committee itself should in further discussion come to a clear understanding of their more specific implications before they are incorporated, if the Committee should wish to incorporate them at all, in the final report.

1. The essential character of the Institute as it now exists should be maintained intact.
   a) Its activities should be confined to genuinely advanced study in fields which in the present circumstances are susceptible of such study without the use of elaborate physical equipment and the maintenance of permanent staffs of technicians.
   b) It should be alert to the possibility of expanding its activities into any field of study that answers to the description above büt only if there is available for appointment a genuinely eminent and productive scholar in that field of study.
   c) It should neither engage in any activity nor refrain from any, simply because it might thereby attract additional financial support; and it should not feel obligated to include in its programme any area of investigation, simply because of a popular belief that by such investigation the cure of social ills or the solution of troublesome problems ought to be discoverable.
   d) It should preserve itself as an intimate and informal community of independent scholars working with the greatest possible freedom from regulations and restraints; and it should not increase in size to a point at which such intimacy, informality, and freedom might be impaired or jeopardized.
2. The faculty of the Institute should be strengthened within the framework of the present programme of the two Schools.
   a) Pure mathematics should be represented in the numerical strength and versatility that it had when Professors Weyl and von Neumann were active. This will require one professorial appointment in addition to the appointment of Borel as professor and Serre as a long-term member; and the strength thus achieved should be maintained.
   b) The activities in theoretical physics should be extended to such aspects of the science as astrophysics, chemical physics, and fluid dynamics; and two professorial appointments in fields such as these should be made.
   c) The School of Historical Studies should continue to concentrate its activities upon the various aspects of European culture in its development from

classical antiquity to the present time. It should be reinforced by the appointment of about five new professors specializing in periods and topics within this framework that are not now represented, e.g. European history from the 15th to the 18th centuries, history of science, history of philosophy and religion, history of language and literature, and legal, economic, and diplomatic history.

3. The Institute in its uniqueness fulfills an important function in the educational and intellectual life of the United States and of the western world. It is important that it should continue to do so; and this it can do only if it develops on the highest scholarly level as something other than merely another graduate school, however eminent, among the graduate schools of this country. Patronage of it should be solicited frankly and firmly on these grounds, in order that it may fulfill still more adequately and effectively this function with which it has come to be identified ever more widely throughout scholarly circles in the western world.

As I believe it desirable for the committee to discuss the details of the above points, so I think that there are other matters either touched upon only casually or not discussed at all hitherto which we should consider before we proceed to formulate a final report. Among these are

1) The point raised in Harold Hochschild's letter of February 20th and your reply to it of February 21st. It is essential that all members of the committee should reach a very clear understanding concerning the subjects considered inappropriate to the regular and permanent programme of the Institute, the difference between these and what may be called "peripheral subjects", and the reasons for these judgments.
2) The problem of the library - space, service, and staff - both with regard to imminent and to more long-range requirements. Some members of the committee, I feel, desire to discuss this subject in detail; and many faculty-members not on the committee have raised questions about it with me time and time again.
3) Stipend funds for temporary memberships and the questions of the number and character of appointments as well as the criteria and procedure of election.
4) Support of publication. I am sure that the Faculty wishes neither an Institute Press nor a formal series of Institute Publications; but, since these questions have frequently arisen, I think that we might well formulate our opinions concerning them and also consider the advisability of seeking funds for the greater support of publication by members of the Institute both permanent and temporary.
5) The matter of the salary-scales for assistants, secretaries, and service-staff. I am not sure that this subject comes within the terms of reference of our committee; but it is a question that will need review by someone. It has a direct bearing upon the reasons advanced for seeking increased endowment, and we might at least call it to the attention of the trustees.
6) I think that the importance of the Director's Fund as a means of supporting "peripheral studies" should be stressed in our final report. In connection with this we might consider our resources for bringing to the Institute for special purposes and for brief periods of time visitors or groups of visitors to take part in conferences or seminars on specific scholarly problems.

I believe that we still have too much to discuss to permit us to make a final report to the Trustees in April. Moreover, I think that it would be wise for you to present to the Faculty an outline of our conclusions long enough before the final report is made to enable all faculty-members to make suggestions and express opinions which our committee could then consider before formulating the final report. For all this there is not time before the end of the current term. I should, therefore, prefer that only an interim report be presented to the Trustees in April with the promise of a final report to come perhaps about at the end of this calendar year.

Yours ever,

/s/ Harold

Harold Cherniss

Copies sent to: Messrs: Dyson
Greenbaum
Hochschild
Lewis
Morse
Panofsky

INSTITUTE FOR ADVANCED STUDY

Draft

Interim Report of the Joint Faculty - Trustee Study Committee

At a meeting of the Trustees of the Institute on the 27th of October, 1955, it was voted to create a special committee, under the Chairmanship of the Director, to be composed of three members of the Board of Trustees and two Professors each from the two schools of the Institute. The Chair appointed Messrs. Greenbaum, Hochschild and Lewis as the Trustee members. At a meeting of the Faculty of the Institute on the 8th of November, 1955, the Faculty welcomed the establishment of the Committee and elected Professors Cherniss, Dyson, Morse and Panofsky to serve on it.

The purpose of the Committee was to make a comprehensive and critical survey of what the Institute is now doing in its academic policy and program; to consider whether this program needs amendment or alteration or extension; to ask whether the program is now being adequately carried out; to ask what new undertakings it would be reasonable for the Institute to embark on that are consistent with its overall policy and Charter; and in all of these matters to consider whether the resources now available to the Institute are adequate, and, if not, to assess as well as may be possible the additional resources required, either for doing better what we are now doing, or for new undertakings.

It has been clear to us from the beginning that we could not make a useful contribution to answering these questions without a common and reasonably complete understanding of what is now going on at the Institute and an examination of whatever rationale exists for this activity. It was also clear that we would need to reflect and discuss at some length what the Institute's Charter and tradition mean in the context of today's intellectual and academic life.

Thus we have not expected to report quickly and decisively to the Faculty and the Trustees. This report is an interim report, intended in the first instance as an internal committee document for discussion among us and for eventual discussion with the Faculty and other members of the Board of Trustees. Indeed, it is clear at this writing that there are a number of questions in the minds of members of the Committee which we have not touched on at all, and without a consideration of which we shall not have answered the questions that are put to us. We shall return to some of these at the end of this report.

The Committee has met four times, each time for about six hours. It seems likely that several more such meetings will be required, and that we can hardly expect to submit a final report before the autumn of 1956. One consequence of this time scale is that on many questions, which were before the Institute at the time the Committee was created, it has been forced to seek a solution and to take action without explicit reference to the Committee's work, even though the Committee's recommendations, if available, might well have been relevant. We list four such examples:

1. The inadequacy of our housing, and the means whereby to meet the expense of improving it were before the Trustees when the Committee was created. We have now virtually completed our application for a loan from the Federal Housing Authority for approximately 70% of the anticipated construction costs, and are submitting requests for bids for the construction of the housing. In all probability, any comments the Committee may make on this matter would be too late to be useful.

2. A second acute problem facing the Institute at the time of its October meeting had to do with the status of our contract research: the program in numerical meteorology and theoretical geophysics on the one hand, and the development and operation of the Electronic Computer on the other. It is now clear that the former will be abandoned. The principal scientists involved will go elsewhere. It is also clear that the Institute will cease its work on the design of larger and faster computers, that it will attempt to operate the present computer for a limited period as an important facility for the researches conducted here in mathematics and physics, and an even more important facility for researches in physics and astrophysics at Princeton University. The future of this operation, which is radically different in scope, purpose and nature from that undertaken by the Institute a decade ago, is a suitable subject for Committee recommendation, as will appear below.

3. A third example is the nomination by the Faculty for a Professorship in the School of Mathematics of Armand Borel, and the nomination for a long-term membership in the same school of Jean-Pierre Serre. These nominations have come to the Trustees in the traditional way, and must be acted on before the Committee's final report, though it might bear directly upon them.

4. Finally, the budget now in preparation contains provision for increases in the stipend funds in the schools for the coming academic year to meet a very serious need that has arisen, especially in the School of Historical Studies. Our report will certainly consider the adequacy of our stipend funds and suggest policies for determining their size, and discuss the methods by which we now allocate them, and the criteria we use. But here again these considerations cannot bear on the immediate problem of running the Institute at the present time.

This Committee has been kept informed on all these matters. It has not construed it as its function to provide emergency advice to Faculty or Trustees; on the contrary, as a Committee, we have been concerned to give counsel on Institute policy for the next five or ten years, and to estimate as faithfully as we can the implications of this policy for the budgetary needs and resources of the Institute.

* * * * * *

We began our work with the study of the present work at the Institute. That of the School of Mathematics falls in three fairly clearly distinguished fields: in pure mathematics; in theoretical natural science, almost wholly theoretical physics; and in the contract research program.

The work in pure mathematics is unique in a double sense. There is no other place in the world where work of such scope, depth, and vigor is carried out in this field; and there is no other field of advanced study at the Institute in which there is, in magnitude and catholicity, a comparable effort. At present there are six pure mathematicians on the active resident Faculty of the Institute, and the number of members in pure mathematics is about fifty, or half of the total membership of the Institute.

This work is catholic, both as to the fields of study and as to the schools of mathematics represented which interact with and cross-fertilize each other. It is a great international center in which French, English, German and Japanese mathematicians participate, along with Americans, and in which the only important missing school is that of the Soviet countries. The work is independent of experiment and of the stimulus of natural science; it is far more abstract and immensely more powerful than the mathematics of the turn of the century. It is recognized throughout the world, and attracts a large fraction of fellows, of professors on sabbatical leave, and of foreign scholars, who elect to come to the Institute in preference to any other institution. Most of the members are supported by funds from the departments of the Government or from other outside sources.

We are clear that there are both historical and intrinsic reasons why what is true of our work in mathematics is not true in other fields. The historical reason is, of course, the generous, continuing and enlightened support, given to this work from the very beginning of the Institute, and the great eminence of its early Faculty. The intrinsic reasons are no less important, though they are harder to explain. Modern mathematics combines great difficulty, abstractness and power both with specialization and harmonious elements of unity. It is self-contained, self-sustaining, and almost self-generative. It is therefore extraordinarily fruitful for men with different interests or different styles to consult one another, and to learn by their conversations and by their lectures what they would only later, and with far greater difficulty, learn from the literature. A mathematician may come to the Institute and be quite confident that he can find out about anything really important in current work in the field.

One of the questions which this Committee has considered is whether the Institute could realize in other subjects, for instance in theoretical physics, or in some branch of history, a comparable massive pre-eminence. It would appear that the answer to this question is in the negative. In physics, for instance, the close connection of theory with experiment, the dependence of theoretical progress on experimental discovery, and the corresponding fluctuating character of really deep and important developments, all distinguish it sharply from mathematics. So too does the immense dispersion and wealth of the institutes and laboratories devoted to physics, and the great variety of derivative branches of physics. In this field it would manifestly not be feasible, and it is most doubtful, that it would be desirable to establish at a single institution the dominant position which we have in mathematics.

It is commonly thought, as it was at one time true, that the relation between current mathematics and current theoretical physics are close and intimate. The developments of mathematics referred to above, its increasingly abstract nature, and its concern with extending and uniting those classical branches of mathematics, such as algebra, geometry and analysis, that arose together with physics, all help to explain the change. The absence of von Neumann and the death of Weyl have removed from the Institute the last two great mathematicians likely to make direct and important contributions to physics. As Dyson said in his obituary notice of Hermann Weyl, "Now he is dead, the contact is broken, and our hopes of comprehending the physical universe by a direct use of creative mathematical imagination are for the time being ended."

The work in physics at the Institute shares with that in mathematics its international character and its intensity of communication between different workers. It has been highly concentrated in a fundamental and special field: The attempt to understand the properties, behavior and existence of the particles of nature, and specifically the attempts to apply methods of field theory and of group theory to this understanding. Brilliant work has also been done at the Institute on problems less near the frontier, primarily on the so-called problems of order, which are important for an understanding of solids. At the present time theoretical physics as a whole is, despite this, not adequately represented, partly because of Placzek's death and Van Hove's departure. The undertaking is smaller than in mathematics. Physics is represented by three members of the Faculty, and a total membership of about twenty-five.

Very occasionally members in other branches of theoretical science, in biology, chemistry and astrophysics come to the Institute. No one on the Faculty is expert in these subjects. More sustained is the work in theoretical psychology, which is guided by an advisory committee, and where two or three members tend to come each year, usually to study and analyze and report on experimental work to which they have devoted many prior years. For these subjects there is little direct technical relation, either with theoretical physics or with mathematics, although the presence of experts in the latter disciplines is occasionally helpful to the former.

The Committee has heard both Dr. Charney and Dr. Goldstine report on the contract researches for which they are respectively responsible. Dr. Charney is acting head of the work in meteorology; and he described to us the successes that he and his associates have had in identifying, by numerical calculation of the properties of appropriately simplified models, some of the salient features of large storms, of the general circulation of the atmosphere, of the Gulf Stream, and of hurricanes. The Committee learned, with mixed feelings, of Dr. Charney's impending departure. On the one hand, his work is lively, full of interest, and quite promising; on the other, it requires staff and supporting equipment, going far beyond what the Institute makes available to its

Professors; and there are differences of judgment and taste as to the depth and difficulty of the contributions that Charney has made.

Dr. Goldstine reported on the general program of the computer. It devotes one-third of its time to the problems of the meteorologists, a small amount of its time to internally generated mathematical problems, and the rest to problems in physics, astrophysics and mathematics which come from members of the Institute and the University and associates elsewhere. In addition, the Institute has been developing, under its computer contract, components and designs which might be helpful in faster and larger computers. Dr. Goldstine reported that with the departure of the meteorologists, and on the occasion of his discussing the matter with us, he had come to the conclusion that the Institute should alter and, if possible, discontinue its efforts in this field. The engineers at the Institute are few, and are, in comparison to those available to industry, inadequate to the task of developing the next generation of computers. We are, in his opinion, neither appropriate nor qualified for such an undertaking. He, himself, believes that the sacrifice of his mathematical interests, entailed in the direction of this project, is no longer justified. He explained to the Committee that the Institute's legitimate scientific needs in this field could be fully met by quite limited access to a computer located in Princeton or even, though less conveniently, by access to a computer in New York or Philadelphia.

The Committee expressed approval of the projected plan of eliminating the engineering program and some of the studies of the mathematics of computation, and of seeking at the earliest practical time to transfer the operation of the computer to another management. This transfer, of course, should protect the Institute's legitimate interests in having access to a computer, and should honor, as far as possible, the obligation to the University, not to deprive it suddenly of a resource on which some University scientists have come to depend. The Committee suggests that only very special circumstances, not now foreseen, would warrant the reinstitution of the Institute's contract research program. Some reasons for this view will appear below.

* * * * * *

At present, studies at the Institute, which do not fall within the School of Mathematics, are in the School of Historical Studies. One of the Committee's purposes has been to enquire whether this framework was too narrow. In the School of Historical Studies today, there is a good deal of unity, both in the emphasis on historical method and in the elements of unity in field and scope. On the other hand, there is breadth and variety of technical approach; as the Director stated at the October meeting of the Board of Trustees, this effort "should concern itself broadly with the history of man on this Earth wherever it is recorded and in whatever form."

Today the fields of study represented by members of the Faculty, and those primarily pursued by visiting members in the School of Historical Studies, all have to do with one aspect or another of the history of the West, from pre-Hellenic times to the present. In this respect, the situation is rather different than it was ten years ago, where Middle-Eastern studies and Chinese studies were also represented, as were special, non-historical, undertakings in finance and economics.

There are now eight members of the Faculty in this school, and the total membership is about thirty. The small number of members, compared to those in mathematics and physics, reflects, in part, the relative scarcity of scholars in many of the fields represented; in part it is also to be attributed to the fact that funds in support of scholarship in these fields are very much more limited and harder to find than in the mathematical sciences; and that therefore the size of the stipend budget of the school effectively restricts the size of the membership, as is not the case in mathematics.

It is true that in Greek archaeology, the Institute, in a smaller field and in a smaller sense, occupies something of the central position that it does in pure mathematics; a substantial fraction of Greek archaeologists have been members of the Institute. In the history of European art something comparable, but perhaps still smaller may be true. But certainly a minute fraction of historians, working in the general span of fields covered by the school, have been members.

We may list here for explicitness the specific fields in which the eight Professors are specialists: Two are Greek archaeologists, one of them primarily an epigrapher; one is a Greek philologist and historian of philosophy and science; one is an historian of Imperial Rome and Byzantium and a numismatist; one is a mediaeval historian; one an historian of European painting; one of modern British history; and one of modern diplomatic history. It is clear that there are striking and, at first sight, bizarre lacunae in this roster. We shall return below to a discussion of this point.

In addition to the stipend funds of the two schools, the Institute has a so-called Director's Fund. Its purpose is to provide grants for temporary members in cases where their work is distinguished and interesting, and their membership receives the approval of one or another Faculty; but where their work lies in a field in which no member of either Faculty is reasonably competent. Examples are: theoretical biology and psychology, law, philosophy, and literature. The role of this fund will be clearer after we have reviewed the criteria by which we must select our Faculty, and the consequence of the exclusion from the Faculty of scholars in many fields of inherent intellectual interest.

* * * * * *

In reviewing the Institute's work, we have had two questions in mind: (1) Are our present fields of work wisely chosen? And (2) Why are there so many other fields richly represented in universities and graduate schools in which we are not active at all? There is a third question, which is whether we are going about our work properly in those fields in which we are engaged. This last question the Committee has so far hardly considered, since it involves an evaluation of the methods by which we select our members and our Faculty, the adequacy of our library and other facilities, and the general good sense with which the place is operated.

As for question (1), the Committee is of the opinion that what we do is good, with the single probable exception of the contract research program, which is to be rapidly reduced, and probably shortly abandoned.

It is to question (2) that we have given almost all our attention: What, if anything, ought we to be doing that we are not doing today? This has involved for us the formulation of a set of criteria in terms of which it would, for instance, be possible to recommend for or against, instituting a program for sinology or biochemistry or political science. Any such criteria will necessarily appear strict and forbidding; and before advocating them, we should insist that they sometimes be honored in the breach. But as general rules, which should not be violated light-heartedly, or without grave reason, we set them forth in items A to F herewith:

A. We are limited to some extent by the Charter, the Certificate of Incorporation of the Institute. The relevant declaration with regard to fields of study is contained in Item 2, which follows:

> "The purpose for which this corporation is formed is the establishment, at or in the vicinity of Newark, New Jersey, of an institute for advanced study, and for the promotion of knowledge in all fields, and for the training of advanced students and workers for and beyond the degree of Doctor of Philosophy and other professional degrees of equal standing."

In the letter addressed by the Founders to the first Board of Trustees, this mandate is emphasized in the following words:

> "...The primary purpose is the pursuit of advanced learning and exploration in fields of pure science and high scholarship to the utmost degree that the facilities of the institution and the ability of the faculty and students will permit..."

We have considered how to apply these instructions, and we have also considered the question of whether they too severely narrowed the field of our work. In doing so, we have the sanction of the Founders, who concluded their letter with the following paragraph:

> "This letter is written in order to convey to the Trustees the conception which we hope the Institute may realize, but we do not wish it or any part of it to hamper or restrict our Trustees in their complete freedom of action in years to come if their experience with changing social needs and conditions shall appear to require a departure from the details to which we have herein drawn attention."

B. In trying to give more specific meaning to the Founders' instructions, we note, as a first step, what "the changing social needs and conditions appear to require." Today, in the Western world generally, and in this country in particular, there is plenty of money for science. By the standards that prevailed when the Institute was founded, jobs and patronage are plentiful. They are the most plentiful, the most overwhelming precisely in those areas of practical application and relatively easy success. But, even in pure mathematics, even in the most recondite parts of theoretical physics, money in support of research is not what is missing.

What we can provide, rather permanently for our Faculty and for a limited time for our members, is freedom, freedom from the press of intricate, organized scientific activity, freedom from unremitting requirements of classes, freedom from the administrative paraphernalia that have become so threatening to the tranquillity of scholarship with the growth of institutions, programs and student bodies. We can provide more than freedom: Something that is implicit in the word "patronage". We can provide the appreciation of a man's colleagues for the difficult, the deep, the unusual, and the beautiful in his own work; and we can provide him with an opportunity to see this intimately, and often at the time of creation, in the work of his colleagues.

In the fields of science, it is these functions that are called for by the situation of the day; and it is only in these that the Institute can make a contribution not wholly overwhelmed by the billions spent upon science elsewhere. We can also provide for direct consultation and communion between men of different countries; and we can provide it precisely at the level of abstractness and novelty where practical grounds would hardly afford the basis for bringing men together.

In historical studies, the situation is not similar; but it has elements of similarity. Here the foundations and the governments make vast funds available for studies deemed relevant to practice, studies aimed primarily at the present and the future in economics and political science, in sociology and social psychology, to cite examples. Support for the scholar whose interest lies in a deep understanding of the past is hard to find; and such scholars are in no less need than are the mathematicians of relief from the burdens of contemporary college and university life. They are also in no less need of opportunities to learn at first hand what colleagues in other lands have been up to.

The lavish support of applied science, of natural science generally, and of the practical aspects of the social sciences clearly give a contemporary argument for interpreting with some strictness the phrase "pure science and high scholarship" in the Founders' letter.

C. The situation just outlined seems to us to reënforce all other arguments that the Institute should not attempt experimental researches; and should be cautious in those studies which require a large supporting staff of technical, but not scholarly workers. There is no need for another great nuclear laboratory, and yet its cost would swallow the whole of the Institute's endowment; there is no need for another Bureau of Economic Research even if we could afford one, which we cannot.

In addition to these arguments, which derive from the present landscape of intellectual activity, there are others based on the way the Institute conceives its function. Members outnumber the Faculty by four or five to one. They are men who come here for intensive work for a year or two at the most. If we are to have them and thus fulfill perhaps our major function, we must restrict ourselves to fields in which such temporary work is possible. Projects, area studies, experimental programs require a continuity of organization, of staff, and often of equipment which cannot be achieved by people coming for a winter or a year. The introduction of equipment, or the institution of major project researches would add to this community great numbers of workers who are not scholars; it would introduce problems of regulation and organization; it would make the Institute very different than it has been or is. The Committee believes that it would be a mistake to lose our ability to perform the functions that we now do perform.

These are some of the reasons why present developments in the Institute's project research seem to us healthy, and why we would advise against instituting such research at all lightly.

These views have a clear bearing on the fields that should be cultivated at the Institute. A field in which massive equipment, staff, or organization is required is not a field for us.

D. There are other ways in which the intimacy and freedom of the Institute could be jeopardized and its community impaired. One of these would be a too great and too rapid growth, which would call for compartmentalization and organization which would interpose a barrier to free and informal and intimate communication; and which would bring to the Institute those elements of organization which it is part of its purpose to enable its members to escape. We conclude that the Institute should not rapidly increase in size, and that such increases should be watched with care, to see that they have not had seriously harmful effects, and that they should be reversed if they have begun to show such effects.

Some growth is natural to any enterprise; and we shall come below to make specific recommendations about the character of that growth over the next decade. But the arguments which favor it will be meaningless, if in the process of growing we have lost the virtue which is our reason for existence.

It follows from this that there is a limit on the number of fields in which we can be active that derives from our limits of size. Were we to add to our historical studies the history of China, the archaeology of India and the pre-history of Africa, we should no doubt have added interesting fields. Nevertheless we should probably not try to do it.

E. Perhaps our hardest theme has been to get an adequate and appropriate understanding of what we ought to mean by "advanced study". It could be taken to mean any singularly intelligent, creative or great achievement in cultural or intellectual life. But we believe this interpretation too broad for the determination of Institute policy. We believe that the word "study" should be taken to mean what it says; and that works of art and literature, normative, hortatory or prophetic writing, and rhetoric, clearly have no place as "studies", though they do have a place as objects of study.

We further believe that what makes study advanced is not so much the native talent and orginality of the investigator as the fact that he must have learned a great deal in order to conduct it. He may, for instance, have learned the disciplines and arts of modern mathematics, or the specialized concepts, methods and lore of atomic physics; or he may be steeped in what was written and said, acted and recorded in early Imperial Rome. This knowledge, this learning, will have taken a long time to acquire; in the case of an historian, perhaps much of a life-time. It will be a treasure. It is to the fostering and application of such treasure that the Institute is devoted. This is what we mean by advanced study; and this is why we must not conclude that because a subject is of practical interest, or has attracted large numbers of experts to it, it necessarily follows that it is a suitable subject for us.

We shall return to the application of these views in specific instances. But they will lead us to the conclusion that although creativity and high intelligence are necessary conditions for a professorial appointment to the Institute, they are by no means sufficient.

F. There are two kinds of field of study at the Institute. In one there will be on the Faculty one, or preferably a group of people, who are themselves outstanding experts, and who will have an interest in bringing to the Institute, for consultation and encouragement, other members who work in the same field. To enter a new field the Institute needs to make such Faculty appointments. Where we do not have a Professor, we can and still do invite members; and this is one of the essential functions of the Director's Fund. Such invitations will tend to have an exploratory, and sometimes a casual quality. We may conclude, from getting to know a member's work and the member himself, that he is someone whom we ought to have here permanently, and that we would like more like him. We may conclude, as is manifestly the case in psychology, that we are playing a very helpful part in providing temporary memberships, but that it is unlikely that any of the men who have come here would in the long run flourish away from laboratories and experience. Or we may conclude that the Institute has, in this instance, invited someone who does not fully conform to what we ought to mean by an advanced student, but who has nevertheless had a year to get some interesting work done which he would otherwise not have had.

Your Committee believes that an occasional appointment, which turns out not to correspond to advanced study, though not desirable, is a reasonable price to pay for the flexibility which it provides; and that a too rigid and absolute application of categorical standards to temporary appointments would be likely to do us harm. But in the making of Faculty appointments, we are, for many reasons, constrained to apply high standards. The commitment is financially grave; even more, it is academically grave, in establishing that there is a field of study that we wish to cultivate.

We have, in the making of Faculty appointments two requirements that must intersect. On the one hand, we must find a scholar in a field of which we are confident that it does involve advanced study. We must also find a man of achievement, intelligence and greatness. And no blueprint for developing the Institute will have meaning unless the men exist to give life and reality to the proposals.

It is sometimes asked whether there are any fields of which it can be said that we would not appoint a man to our Faculty if he were sufficiently intelligent and creative. Probably, apart from the requirements of relevance, and of our inability to provide equipment and staff, there are few fields in which such pronouncements can be made absolutely. But even an intelligent man cannot become an advanced student in a subject which does not have the relevant material, or become educated with great learning, or with difficultly acquired techniques, if the learning does not exist and the techniques have not been invented. The founders of great disciplines were not always practitioners of advanced study. Sometimes they were. But such men will not appear with great frequency in the next decade. Thus it is possible for us to say of some fields that it is most unlikely that we shall enter them by a Faculty appointment, on the ground that the material for advanced study does not exist in them and probably will not exist in the decade for which we are writing.

* * * * * *

I. When we now look at what the Institute is and is not doing, in the light of the arguments and the criteria set forth above, we come to a number of findings, some affirmative and some restrictive. These we should now review.

We have concluded that in its essential character, the Institute should be preserved. It should not attempt the vast expansion which any more complete coverage of all fields of research and study would imply; it should keep away from the laboratory and the experiment; it should not change its size radically; it should not admit as members, and certainly not to its Faculty, men whose work is interesting, but elementary; it should continue to be an international center, open to scholars from all parts of the world, with a primary, but not exclusive emphasis on their welfare, their achievements and their intellectual and professional growth; it should never appoint to its Faculty, because an appointment is needed, in a particular field; it has no need to be complete, and cannot be; it should never appoint a man no matter how brilliant, unless his work rests on and creates a corpus of learning; it should in all these matters follow the traditions which have in general characterized it in the past; and it should not grow too fast or too much.

II. We have examined in a good deal of detail what elements of desirable growth are likely for the decade ahead; and, as a part of this, have asked ourselves with some seriousness whether we were right in anticipating that we would not enter many important fields prominent in higher education in this country or throughout the world. We can now summarize our conclusions on these matters:

A. The mathematicians of the School of Mathematics believe that if their present recommendation for the appointment of Borel is approved and accepted, they will be close to having a Faculty broad enough and strong enough to carry on. They estimate that one further appointment, or a total strength of about eight would be right in the present and foreseeable state of mathematics, and with the present strength and interests of the Faculty. This Committee is only poorly qualified to judge the substantive elements of this judgment. We would expect that the Faculty in mathematics would need to be, would wish to be, and should be maintained at about this level. It is not anticipated that the temporary memberships in mathematics will rise or should rise above their present level.

B. As indicated above, the physicists at the Institute have concentrated to a very great extent on the deep and difficult problems of particle physics. In the last decade there has been much progress, and much brilliant and beautiful work on the theoretical side of this problem. But your Committee has been told that the problems that lie ahead appear very deep indeed, that it is not clear by what means they will be solved, that it is not clear to what extent experimental clues may be required for their solution, and that a continuing effort, concentrated entirely on these questions, does not appear adequate as an Institute program. For one thing it gives too narrow a range of problems for the younger members, by setting a style which discourages them from studies in other parts of physical theory where progress may not be so difficult. For another, it deprives the Institute of the interest and vitality that branches of science now flourishing could bring. And for another, it does not do justice to the possibility that the methods now applied in other parts of physical theory may be relevant and even necessary if progress is to be made toward finding the basic principles, now unsuspected, which describe the order of the atomic world. Thus the physicists desire to add to their Faculty, in such fields as astrophysics, fluid dynamics and chemical physics. They have informed us that they are now considering possible candidates.

It is also clear, particularly in view of the fact that three Professors of physics are very much of the same age, that the Institute should be prepared to make new appointments also in the basic field of particle physics, should during the coming decade a new man or a new method or a new discovery make that appropriate.

Your Committee believes that in other fields of theoretical science, Faculty appointments are probably not now justified by the state of the science. It is doubtful whether in biology, in chemistry or in psychology, purely theoretical work, divorced from the laboratory, is of sufficient robustness and difficulty. On the other hand, in these subjects temporary memberships should certainly be encouraged. Probably the system of an advisory committee, already adopted in psychology, might with profit be applied to the biological sciences.

C. Within the School of Historical Studies, it is the view of your Committee that historical work should be concentrated, as it now is, on the history of the European tradition and on areas or subjects closely contiguous with this. It is clear that there are many periods and many aspects of this tradition whose history is not represented on the present Faculty; and whenever eminent scholars are available, we should certainly wish to represent the history of Europe from the 15th through the 18th Centuries, the history of science, of music, of philosophy and religion, of law and literature; we would be interested in legal, economic and diplomatic history.

Your Committee believes that not all of these virtual great scholars will in fact be found, or will be available to us. But we think that perhaps ten years from now the Faculty of the School of Historical Studies will be increased by about five over its present strength of eight. This cannot be an exact assessment; but your Committee affirmatively does recommend that we attempt to find and appoint scholars to bridge and to enrich the study of the Western tradition.

It is clear that this development should and must also be accompanied by an increase in the number of members in the school. No exact equality in Faculties or memberships seems to us necessary or obtainable as between the two schools. They are now more out of balance than seems to us healthy.

D. This Committee has considered quite earnestly some subjects, the history of which would naturally fall within the scope of the School of Historical Studies, but which can be approached by non-historical, substantive methods. We are not concerned as to whether scholars so engaged are or are not counted in the School of Historical Studies. We are concerned as to whether we are likely to find them, and whether we ought to seek them. We are clear that if a philosopher, as opposed to a man primarily an historian of philosophy, who was a great scholar and a great philosopher, were to become known to us, we should take a great interest in having him here on our Faculty. This interest would be increased if his philosophy had a synoptic bearing on human affairs as manifested in man's history, on the one hand, and on the sciences and logic and mathematics on the other. We regard it as very much less likely that we should appoint a Professor of theology, jurisprudence or political economy, as opposed to an historian dealing with the development of these subjects.

There are a group of disciplines of which we are persuaded, that as of today they are unlikely to qualify as subjects of advanced study; these include, but are not limited to, most of the social sciences. This view we hold of economics, political science, social psychology, literary and artistic criticism, anthropology and sociology. We do, however, recommend that the Director's Fund be used to bring the best scholars in such fields to the Institute for temporary memberships, for exploratory purposes, and for their inherent interest; and we further suggest that the Director's Fund be used for conferences which might serve to widen our views and correct our prejudices.

* * * * * *

We must turn now to the implications of what we think ought to happen at the Institute in the next ten years, that we think probably will happen if there are adequate resources. Whenever a Professor is added to the Institute Faculty, the Institute is committed explicitly for his salary, travel, retirement and assistants to about $25,000 a year; and implicitly for a good deal more. He will need the services of a secretary; he will have bibliographical needs which the library probably will not initially be in a position to meet; he will want, and he ought to want, to have some temporary members interested in his field, on the average perhaps of four members for each member of the Faculty, of which at least two typically will require stipends. It is thus reasonable to say that each new Professorship requires in explicit funds $25,000 a year and ought to be based on additional income of at least $50,000 a year. Thus, since we are talking of increasing our Faculty by about ten members in the coming decade, we are talking about an increase in budget of about $500,000. If we now assume that our present budget of about 1 to 1.2 million is justified on the basis of a portfolio of market value of approximately 30 million, we should need an extra 12 million in endowment. We would need this not to change, not to alter, but only on the basis of what we now are and what we intend to be.

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There are matters which the Committee has not so far discussed, or which it has discussed only in a most inconclusive and random way. They clearly also have a bearing on our financial needs, which should thus not be formulated in more than a most provisional way at this time:

1. Do we do the right thing with our stipend funds? Are our stipends large enough and is our method of determining their magnitude wise? Are there any reforms in the procedures by which we select members that we ought to consider or institute?

2. Are we following the right course in the growth of the library and in the physical arrangements for housing it? Are our other facilities adequate, such as those that we support and borrow in McCormack Hall for historians of art? Are we making adequate provision for high-speed computing for the future, primarily for the physicists and mathematicians who may need it?

3. If our Faculty is to grow by about 50%, and if the membership in the School of Historical Studies is also to increase, will we have room? Is the cafeteria large enough? What problems will confront us because of the fixed nature of some elements of our physical plant? What will the building program for new offices cost?

4. Should we change our policies with regard to publication by seeking a more uniform format or more formal imprimatur? Is the Publications Fund available where it is needed? Is it adequate?

5. What can we predict of the number, what should be done for the salary, of the Institute's staff?

Only when we have looked at these points and drawn conclusions from them shall we be able to define our financial needs.

P. S. --Since we propose a 50% increase in Faculty, I would conclude that we needed a 50% increase in endowment. I doubt whether we shall find methods of calculation more precise than this. RO

T & R
Events: housing, Chames; Computer.
Decade Policy: what in grad Schools, Fdn program
1st four is
Math
Phys — Sci Psy
Hist — sweep; Nature unity.

Remain
Stipends etc
Staff + Salaries
Library - McCormack - Computer.
Space - Cafeteria
Publication

History: unity in emphasis of history; some unity in field & scope, breadth (differ from 10 years ago 8/30 Quote Ro
Enumerate chairs; center in Gr. archaeology, > hist of Europ art; small fraction of historians in our general field; growing in #, quality, interest.

How the Directors Fund works -
Literature ~~Rely~~, Phil, Law, Biology, ~~etc~~ - Psych -

Nature of Review: 1) are present things right;
2) why not others, represented in U
; Criteria a) quote Charter, letter.
b) the functions: Faculty, members;
Background: Money for Science, practical affairs, even Math & Phys; less for scholars; Bustle & teaching in U; International communication;
c). equipment, permanent staff
d) intimacy, ~~comm~~ minimum of compact mentalization & organization, communication
e) what is advanced study? corpus of difficult to master material; not in all fields, typically not in fields of apparent importance to practice distinction from intelligent, creative, great achievement, possible in lit & arts, Politics & Pol Theory,
f) Faculty & members; in latter experiment & some flexibility desirable; in former need great scholar. Cannot seriously enter without; existence of great men (e.g. Schweitzer, Hobbes, Lenin, Baudelaire) not adequate -

Conclude

(1) essential character right

(2) No current activity inappropriate (except contract research to Bylaws); Fac, & criteria for selection of members.

(3) No radical changes

(4) Growth & balance

a) 1+1 pure Math; accept estimate

b) State of particle physics; effect of concen. on younger members, uncertainty of road to progress, & possibility that methods used in other part of phys. relevant; desire to broaden: astrophysics, chemical physics, fluid dynamics; hard to consider; Dont prefer to add in P. phys. when man or method ordiscovery; perhaps 3 in 10 yrs to 6. Doubts on th. biology, chemistry, ops for permanent.

c) Conc on European tradition & contiguous areas ^not only & also Ch. hist + music. ~ 50 60

d) Non historical: Philosophy - synoptic. Less Likely Theology, Jurisprudence, Political Economy. Most Soc. Science: sociology, anthropology. Ch. AM p 2 — Members sure.

→ Director & time - visit - meetings

What does this imply: additional cost / prof explicit; implicit - members, library; staff, plant.

explicit 700,000; + implicit, 1.2 , × 10.

IF assume 30 ok for 1-1.2, then need extra.

Only on basis of what we are & intend.

Establishment of C - T, F
T of R; purposes
Need of time; meetings; interim

Immediate problems; housing; contract research; Meteor; computer; opt in math, strengthening; ~~to~~ be informed; not advise, rather 5-10 year policy & implications

Study of present situation
School of Math: Pure Math, ~~Physical~~ Theoretical Natural Science, Contract Research.
Unique (double) Role in Pure Math; 6/50
Catholic as to ~~sub~~ fields + schools (Soviet)
Recognized; Large fraction of Fellows, sabbatical, foreign scholars elect to come
Reasons: historical; intrinsic difficulty; unity self-confined nature of Math.
Relations of Math & Phys; Weyl, v Neumann, Quote Dyson.
Physics 3-20; particle-field; order; ~~Narrowness~~
With Pleczek's death, v Hove departure limited.
Very occasional biology, chemistry, astronomy; more sustained in psy.; little direct technical rel with math or phys.
> Chang + Goldstine report.
Contract: mixed on Meteor
Clear on not develop computer; hope disengage from anomaly of operation, support log & math studies steps under way — 1 Only very special circumstances warrant reconstitution of such programs

THE APPLIED PHYSICAL SCIENCES

Jule Charney and Walter Munk

I. INTRODUCTION

Our charge is to examine the influence of the computer in applied physical sciences. We have taken a narrow interpretation, limiting ourselves to the geophysical sciences; even there, the emphasis is almost entirely on the fluid envelope of the planet Earth. This is in part because of our own limited competence, in part because we believe that the underlying principles of the computer revolution are better brought out by case histories than broad generalities. Even so, we end up with more than we can handle.

II. EARLY HISTORY OF COMPUTING IN METEOROLOGY

In 1946, at the end of the Second World War and on the eve of the arithmetical revolution, a conference on mathematics was held at Princeton University in celebration of its bicentennial anniversary.[1] Two famous mathematicians spoke of the future importance of the computer. Hermann Weyl expressed concern that the store of mathematical substance which formed the basis for current generalizations was in danger of becoming exhausted without outside help, "be it even by such devilish devices as high-speed computing machines". And John von Neumann remarked that the success of mathematics with the linear differential equations of electrodynamics and quantum mechanics had concealed its failure with the nonlinear differential equations of hydrodynamics, elasticity and general relativity. He expressed the hope that the computer-aided solution of a large store of problems in nonlinear continuum mechanics would indeed supply a basis for mathematical generalization.

To him meteorology was _par excellence_ the applied branch of mathematics and physics that stood the most to gain from high-speed computation. Earlier that year he had called a conference of meteorologists to tell them about the general-purpose electronic computer he was building at the Institute for Advanced Study and to seek their advice and assistance in designing meteorological programs for its use. Charney had the good fortune to attend and recalls that the response from the established figures was interested but less than enthusiastic. C.-G. Rossby perhaps best voiced their feelings by stating that the mathematical problem was not yet defined: there were more unknowns than equations, for we had not yet been able to express the components of the Reynolds stress tensor in terms of mean flow variables. Citing L. F. Richardson's[2] gallant but unsuccessful attempt to solve the hydrodynamical equations of the atmosphere by hand calculation, Rossby said that computation could not be successful before observation, experiment and analysis had led to a better understanding of fundamental atmospheric processes, in particular of atmospheric turbulence.

His caution had the positive effect of convincing von Neumann of the need for physical as well as mathematical analysis, but he failed to appreciate the great psychological stimulus that the very possibility of high-speed computation brought to meteorology. All of its branches were given new urgency and new importance by the promises that the contributions to the atmospheric circulation from a variety of physical processes could be synthesized mathematically within the computer. This was especially true of dynamical meteorology, where the requirements of mathematical tractability had forced such oversimplifications that theories of the large-scale circulation were of little use in prediction and incapable of meaningful comparison with reality.

Charney joined von Neumann's group two years later and took up the task of formulating a hierarchy of mathematical models embodying successively more and more of the physical and numerical aspects of the general prediction problem, hoping in this way to avoid the dangers of introducing a great many poorly understood factors all at once.[3] He had previously devised a method for overcoming the mathematical difficulties responsible for Richardson's failure. His point of departure was the realization that a compressible, stratified fluid held gravitationally to a rotating sphere can support a variety of wave motions, including acoustic and inertial-gravity oscillations, which are of little meteorological importance but which impose highly restrictive conditions on numerical algorithms for solving the gas dynamical equations. He proposed to filter out these "noise" motions by imposing certain equilibrium constraints on the primitive equations of motion.[4] It can be established by scale analysis that the slow, large-scale motions containing the bulk of the atmosphere's energy are close to equilibrium in the sense that the pressure, gravitational, centrifugal and Coriolis forces are nearly in balance. Because of this balance, quantities like acceleration and velocity divergence are obscured by the noise motions, and any scheme, such as Richardson's, which is based upon their explicit calculation will give nonsense in the first few time steps. The balance equations deal only with observables and are therefore consonant with meteorological experience and theory. They were immediately successful in application to prediction.

Not surprisingly, the simplest model in the hierarchy was Rossby's own. He had previously suggested that the atmosphere at a level between 3 and 6 km behaves as if it were a two-dimensional, incompressible flow and had deduced his famous dispersion formula for long, small-amplitude waves in a uniform zonal current.[5] Charney and Eliassen[6] verified that there were indeed important aspects of the observed motions, involving horizontal energy dispersion rather than vertical dispersion or overturning, which were explicable as two-dimensional phenomena; whereupon Charney, Fjørtoft and von Neumann[7] devised a numerical method for solving the two-dimensional vorticity equation

$$\frac{\partial Z}{\partial t} = \frac{1}{a^2\cos^2\phi}\frac{\partial(Z,\psi)}{\partial(\phi,\lambda)}\ , \qquad Z \equiv \nabla^2\psi + 2\Omega\sin\phi\ ,$$

for the stream function $\psi(\phi,\lambda,t)$ on a rotating sphere of radius a, latitude ϕ, longitude λ and angular speed Ω. The solutions, carried out in 1950 on the ENIAC*, bore a sufficient resemblance to reality to inspire further effort.

At that time one of the principal problems of meteorology was extratropical cyclogenesis, the formation of the large cyclonically rotating vortices responsible for weather in middle and high latitudes. Charney[8] and Eady[9] had explained cyclogenesis theoretically as an instability of the mean zonal flow according to which the incipient cyclone grows at the expense of the potential energy associated with the mean equator to pole temperature gradient; and Phillips[10] had shown that a model consisting of two superimposed homogeneous, incompressible layers of different density was capable of simulating the main features of the more general theoretical models. Accordingly, the first models programmed for the new computer at the Institute for Advanced Study consisted first of two and then of three layers. An exceptionally strong and sudden development of a cyclone over the Eastern United States was

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* Electronic Numerical Integrator And Computer, at the Ballistics Research Laboratory of the U.S. Army Ordnance Department, Aberdeen, Maryland. The ENIAC was built as a special-purpose computer by J.P. Eckert and J.W. Mauchly at the University of Pennsylvania and later provided with a general-purpose control after the design of von Neumann.

successfully predicted in 1953 with a three-layer model, of course after the event. The results tended to substantiate the theoretical ideas concerning its mechanism of generation, and they interested the U.S. Government in the possibilities of numerical weather prediction. A national numerical forecasting unit was set up in 1954 and began experimental operation in 1955. Similar units were established in other countries.

Jeffries[11], Bjerknes[12] and Starr[13] had shown empirically that the cyclone is a major element in transporting angular momentum poleward from the tropics to maintain the mid-latitude westerly zonal flow against frictional dissipation; and Kuo[14] and Charney[15] had proposed the explanation that the cyclone wave, while deriving energy from the potential energy associated with the meridional temperature gradient in the zonal flow, returns kinetic energy to the flow through the action of the Reynolds stresses in the presence of a stabilizing meridional vorticity gradient. A rudimentary, thermally-active model permitting this type of interaction was constructed by Phillips - with sealing wax and string so to speak - and in 1955 the first dynamically consistent simulation of the atmosphere's general circulation was obtained; the conjectured mechanisms were found to operate, and many important features of the observed circulation were simulated.[16]

In 1955, a second conference of meteorologists was held at the Institute for Advanced Study to consider the implications of these extremely encouraging results for long-range prediction and for the simulation of climate. Von Neumann[17] divided the motions of the atmosphere into three categories: (1) those that are determined primarily by the initial conditions; (2) those that are practically independent of the initial conditions;

and (3) those that are not so far from the initial state that they are unaffected by the initial conditions but sufficiently far that the initial conditions do not express themselves clearly. The corresponding prediction problems are short-range prediction, sinulation of climate, and long-range prediction respectively. From general experience he expected the first problem, for which the extrapolation parameter is comparatively small, to be the simplest, the next most difficult to be the asymptotic problem for which the extrapolation parameter is very large, and the most difficult of all to be the intermediate problem for which the extrapolation parameter was neither large nor small.

His expectations have been amply borne out. The greatest successes have been achieved in short-range weather prediction and in the simulation of climate; a beginning attack has even been begun on the simulation of a coupled ocean-atmosphere system; but long-range prediction has remained essentially an unsolved problem. It is not even known what, if anything, can be predicted for more than two weeks. Nevertheless, the potential social and economic benefits to be derived from even a modest extension of forecast range or accuracy have justified a considerable investment in observing and data processing equipment. The computer has brought a new maturity to meteorology whereby theory and observation at last exist on an equal footing, and the requirements for numerical prediction and physical understanding have become a major influence in the selection of observational systems. The Global Atmospheric Research Program, sponsored jointly by the World Meteorological Organization and the International Council of Scientific Unions, is the international expression of this reality. A description of these efforts is given in a number of publications.[18,19]

The considerable progress that has been achieved in the past twenty-five years has been based largely on the adaptation of old ideas to the new computational framework; gradually the growth of sophistication in numerical techniques and of speed and capacity of computers has made it possible to incorporate existing knowledge of physical processes in something like the order of their importance for the atmospheric circulation. There have been difficulties. Parkinson's law of computing in meteorology is that problems expand to fill all computers. As a result, spatial resolution and truncation error have diminished only slowly, and it has not always been possible to distinguish between mathematical and physical error. Short-range numerical predictions for the middle and upper atmosphere have demonstrated their usefulness, but it has been only recently that the more complicated surface boundary-layer structures, involving friction and topography, have been sufficiently resolved to yield useful numerical predictions of wind and temperature. The numerical prediction of cloud and precipitation remains only marginally useful because it involves poorly understood condensation phenomena on still smaller scales.[20]

The question of predictability arises. In the early 1950's there was an apparent divergence of view concerning the nature of the meteorological prediction problem between what may be called the Princeton school under von Neumann and the Cambridge (Massachusetts) school under Norbert Wiener. Von Neumann and Charney regarded prediction as a determinate initial value problem, whereas Wiener[21] and others at M.I.T. emphasized the stochastic character of the atmospheric motions and proposed instead to employ linear "black box" prediction methods based upon long time-series of past data. Wiener went so far as to state privately that von Neumann and Charney were misleading the public by pretending that the atmosphere was predictable as a determinate system. Without taking sides in the controversy, let us attempt to look at the problem of numerical prediction from a general point of view.

At a height of 100 km the density and pressure in the atmosphere are less than a millionth part of their surface values. Below 100 km the atmosphere is very nearly a perfect gas obeying known laws of physics: the Navier-Stokes equations of motion are not seriously in doubt; absorption, emission and scattering of radiation by the principal atmospheric molecules is not a mystery; the calculus of radiative transfer is well advanced; and while there remains more to be learned about such phenomena as the microphysics of cloud formation, scattering and absorption by suspended particulates, and the influence of trace gases on ozone photochemistry, the laws of motion and energy exchange may be considered quite well understood. If Laplace's mathematical intelligence were replaced by a computing machine of unlimited speed and capacity, and if the atmosphere below 100 km were spanned by a computational lattice whose mesh size were less than the scale of the smallest turbulent eddy, say one millimeter, there can be little doubt that numerical integration of the partial differential equations embodying the known dynamical and thermodynamical laws of motion would simulate and predict atmospheric behavior with considerable accuracy. Would the problems of meteorology then have been solved? Or, contrariwise, would more be known than can presently be learned by careful observation of the atmosphere itself, which, after all, is its own analog computer?

The answer would seem to lie somewhere between these extremes. With respect to prediction, even if one disregards the indeterminacies arising from lack of knowledge of fluctuations at the boundaries - these could be overcome by extending the calculations higher into the atmosphere and lower into the oceans - it is very likely that all accuracy would have vanished in less than one month. This is not because of quantum indeterminacy, or even because of macroscopic errors of observation, but because the errors introduced into the smallest turbulent eddies by random fluctuations on the scale of the mean free path (ca 10^{-5} mm at sea level), although very small initially, would grow exponentially until in a very short time these eddies would have become indeterminate and would have begun to affect the next larger scales by nonlinear interaction. The escalation to larger scales would continue until eventually the main energy-bearing scales would have been rendered indeterminate.[22,23,24] This escalation of the error occurs because the turbulence exhibits a similarity behavior such that the time scales of the interactions increase in approximate geometric ratio with scale (at least for the smaller scales) in such a manner that the error progresses from 1 mm to 10 km in less than one day, and from 100 km to the planetary scales in a week or two.*

Unfortunately, or fortunately, the limitations imposed by the finiteness of the velocity of light and the least size of a stable memory storage element make computation for the 5×10^{28} points in the one-millimeter lattice impossible in times short of the astronomical. Even the 5×10^{9}

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* The upward propagation of uncertainty toward larger scales must face a downward propagation of energy toward smaller scales in the high-wavenumber "Kolmogoroff" range. Leith and Kraichnan[25] have shown that uncertainty wins.

points in a lattice with a horizontal mesh size of 10 km and a vertical mesh size of 100 m is beyond the capacity of any present or contemplated computer. Thus one is forced to reduce the effective number of degrees of freedom by dealing with volumes containing turbulent elements ranging over several decades of scale, and it becomes necessary to determine the turbulent transports of mass, momentum, and energy from one volume to the next, i.e., to develop a theory of small-scale turbulence. More generally, one must develop statistical theories for all processes smaller than the least computational mesh size. These include not only mechanically and thermally driven turbulence, but also cumulus convection, internal gravity-waves produced by flow over obstacles, wave interactions at the air-sea interface, etc. Ideally the mesh sizes should be smaller than the scale over which the small-scale processes are statistically homogeneous. Given realistic limitations in computer speed and capacity, the computational lattices must ultimately become variable in space and time, or the base functions for Galerkin methods of approximation must change in time.

The problems of mechanically or convectively driven turbulence, or of cumulus convection, are no closer to solution than those of the general circulation of the atmosphere. They, too, can be aided by computer simulation; but the reduction in scale is not an essential reduction in complexity, and it has been estimated that each will require approximately the same amount of computation as the general circulation.

It is perhaps fortunate for human activity that motions whose scales are so large that they can be resolved by possible computational lattices constitute a fairly well-defined set. This is because the energy of the turbulent motions decreases very rapidly with scale below a space scale of about 1000 km and a time scale of two or three days. It is this circumstance that makes the atmospheric flow predictable in principle for as long as a week and makes the large-scale motions the controlling entities.

It would thus appear that there is some justification for both the Princeton and the Cambridge schools of thought. It is possible to look at the large-scale motions of the atmosphere as a determinate system for short periods of time, providing one has a statistical theory permitting the incorporation of the turbulent fluxes of matter, energy and momentum. But for longer periods of time the atmospheric variables, even on a large scale, must be regarded as random.

There remains the question as to the best method of statistical prediction. The use of linear prediction methods is based on the assumption that one is dealing with a conservative system, or at least one which is in statistical equilibrium. In this case the ergodic theorem asserts that a present state of the atmosphere may be found to any degree of approximation in a catalog of past states, if the time series is sufficiently long. It is very doubtful that such methods will prove feasible, not only because the time series would have to be impossibly long but because the atmosphere-ocean-Earth system is nonconservative, and the very notion of statistical equilibrium on a climatic time scale is in doubt. It is more likely that long-range prediction will become a matter of calculating probability distributions and ensemble averages by Monte Carlo methods and climatic theory a matter of calculating statistical moments from long computer simulated time series.

III. SOME CASE HISTORIES

The past twenty-five years has seen much progress in meteorology and oceanography. To what extent has this progress been aided by the computer? And how has the computer affected the manner in which problems are selected and solved? We believe that answers to such questions are best given by presenting a few representative case histories.

The uses of the computer in fluid geophysics may be classed as synthetic, experimental, heuristic and data-analytic. The first category includes the prediction or simulation of the large-scale circulation of atmospheres and oceans, numerical studies of predictability, simulation of climate and climatic variation, of mechanically and convectively driven turbulence on various scales, of atmospheric and oceanic tides, and of cloud microphysics and dynamics. The second category deals with the simulation of individual phenomena as a means of inferring their physical causes, insofar as these phenomena can be isolated in the highly interactive, nonlinear atmosphere-ocean system. Among the atmospheric problems in this category are extratropical cyclogenesis, tropical cyclogenesis and frontogenesis. The oceanographic problems include the formation of such major currents as the Gulf Stream, the Kuroshio, the Somali Current, the Antarctic Circumpolar Current, and the Equatorial undercurrents. The third category includes the construction of highly simplified numerical models, sometimes in conjunction with laboratory experiments, sometimes alone, as a means of discovering new relationships having a bearing on the nonlinear behavior of the atmosphere and oceans. For example, games played with highly truncated Fourier series or with a finite number of vortex elements have been valuable in illuminating the nonlinear and stochastic properties of complex systems whose detailed numerical prediction, especially over long periods of time, is either prohibitive or so time-consuming as to furnish little understanding for the effort expended. The fourth category deals with problems of data reduction and analysis as a means of inferring physical behavior from raw observations. The following are some case histories taken from the various categories.

1. Tropical cyclogenesis. One of the major stumbling blocks to accurate simulation of the atmospheric circulation is lack of knowledge of the statistical dynamics of cumulus convection. Solar insolation is made available for driving the atmospheric circulation primarily by evaporation of moisture from the sea surface and liberation of the latent heat by condensation in rising currents of air. In the tropical atmosphere the release of latent heat adds sufficient buoyancy to the rising air parcels to produce gravitational instability. This instability is manifested as cumulus convection. It has been conjectured that tropical disturbances arise from a kind of cooperative interaction between an ensemble of cumulus cells and the large-scale field of motion.[26] According to this hypothesis, frictionally-induced convergence of moisture in the surface boundary layer of a large-scale cyclonic disturbance supplies moisture for cumulus convection, and the convection in turn supplies latent heat energy for driving the large-scale disturbance against frictional dissipation. Theoretical analysis is made difficult by the fact that condensation processes on both small and large scales are intrinsically nonlinear, even for small amplitudes (the release of heat of condensation depends on the

sign as well as the magnitude of the vertical velocity). For this reason adequate theories relating the convection to the large-scale motion are lacking. Nevertheless, several semi-empirical schemes for incorporating the large-scale effects of the release of latent heat have been used for the numerical simulation of tropical disturbances. The growth of an axi-symmetric disturbance into a hurricane has been simulated with some success by Ooyama[27] and others. Numerical models have even been used to simulate hurricane modification by artificial and natural means.[28]

But there have been difficulties. All small-amplitude symmetric disturbances of an atmosphere at relative rest grow into hurricanes. No small-amplitude asymmetric disturbances grow into hurricanes.[29] Since only a small fraction of all real tropical disturbances do grow into hurricanes, the conclusion of the syllogism is that the cyclogenesis requires an initial asymmetric, finite-amplitude disturbance. We conjecture that the hurricane arises as a kind of finite-amplitude instability, and that only when cumulus statistics are properly understood, and the interaction of the perturbation with the asymmetric shearing flow in its environment is properly taken into account, will the prediction of tropical cyclogenesis become possible.

2. <u>The general circulation of the atmosphere</u>. The invention of the balloon-borne radiosonde and the military requirements of World War II brought about a rapid expansion of the global observational network in the thirties and forties. For the first time the three-dimensional structure of the atmospheric circulation became sufficiently well known to supply a foundation for theory. To a first approximation the circulation

may be described as an axisymmetric, circumpolar vortex on which wave and vortex perturbations are superimposed. Analysis has suggested that travelling disturbances are due to instability of the mean zonal flow[8, 9] and stationary disturbances to forcing of the zonal flow by mechanical[6] and thermal[30] action. The deformation fields in the cyclone wave disturbances give rise to velocity and temperature discontinuities. These are the so-called frontal surfaces whose three-dimensional structure was described by J. Bjerknes and H. Solberg[31] just after World War I and whose formation was described by T. Bergeron[32] a few years later. A dynamical theory of frontogenesis has only recently been given, by Stone[33], Williams[34] and Hoskins[35].

Linear analysis of wave and vortex modes and their first order interactions with the mean flow have been useful. One seeks causal laws, but a numerical prediction or a numerical experiment is not in itself a causal law; it can only verify a causal law. When one is dealing with nonlinear, nonstationary phenomena, causal laws require for their statement or interpretation a basic vocabulary of characteristic wave and vortex modes of which the more complex motions are composed. Undoubtedly a fuller explanation will require that one consider self-interactions and higher-order interactions, as well as secondary and tertiary instabilities, but it is likely to remain true for some time that linear thinking based on simple models will serve as a first-order guide in the labyrinth of nonlinearity.

Phillips' numerical experiment[16] synthesized several of the processes that had been investigated analytically. It was followed by more elaborate experiments with more realistic atmospheric models. Numerical integrations for the entire global circulation with realistic surface boundary conditions and energy sources and sinks have simulated the characteristic features of the large-scale circulation with some fidelity[36]. Others have simulated the simpler rotating-tank analogs.[37] These integrations, when taken in combination with the theoretical studies and the simple laboratory and numerical model experiments, are a kind of definition-in-use of what one means by an "understanding" of the atmospheric circulation.

One may now ask: how has the computer contributed to our understanding of the general circulation? Let us enumerate its accomplishments: the theoretical and laboratory models are too greatly simplified to be directly applicable to the atmosphere; numerical computations have verified that the hypothetical mechanisms do indeed operate. The theories of cyclogenesis were developed before the computer, but the theories of frontogenesis were influenced by laboratory and numerical models of the frontogenetic process. The most successful models of tropical cyclogenesis are numerical. Genuinely nonlinear phenomena, such as the fluctuating interaction of the perturbations and the mean flow were first simulated numerically and only later, and to a very limited extent, analytically. Although we have no general theories of nonlinear atmospheric and oceanic processes, the accumulating store of special results is beginning to reveal characteristic properties, such as transition from steady, to regularly fluctuating, to random regimes, which are shared with quite different nonlinear systems.* These add support to von Neumann's anticipation that the computer will eventually supply a basis for meaningful generalization in nonlinear continuum mechanics.

* See, for example, Pasta[38].

When a computer simulation successfully synthesizes a number of theoretically-predicted phenomena and is in accord with reality, it validates both itself and the theories - just as the birth of a child who resembles a paternal grandfather legitimizes both itself and its father. The theoretical ideas of extratropical cyclogenesis and of the interaction of the cyclone with the zonal flow were legitimized in this way. But this synthetic use of the computer was effective only in combination with observation and theory. When one element in the trichotomy is absent the synthesis fails. If it is theory, the computer may function as an experimental-heuristic device. It has played this role in the theory of tropical cyclogenesis.

3. The general circulation of the oceans. The computer has not played an equally important role in the theory of ocean circulations. Stommel[39] remarked in 1954 that the theories of the ocean circulation had a peculiarly dreamlike quality. They retain some of this quality to the present day. The cause is the difficulty of observing the deep ocean and the lack of a sufficient economic incentive for doing so. The turbulent transfers of momentum and heat (and salt) are not understood, especially in the deeper layers of the ocean, and yet they play a more decisive role in the oceans than in the atmosphere. It is not known, for example, whether turbulent diffusion is vertically downward from the surface or along isentropic surfaces inward from coastal boundaries.

Nevertheless, theory has flourished. In the late forties physical oceanographers, having little knowledge of turbulent transport mechanisms, concentrated their attention on predicting the behavior of the vertically integrated mass transport. Sverdrup[40] employed the principle of force equilibrium already mentioned in Chapter II to estimate the mass transport of the principal ocean currents from the observed surface wind stress. His explanation failed at the western boundaries where the reduced order of the equilibrium equations made them incapable of satisfying the condition of zero normal mass transport. Stommel[41] and Munk[42] explained the western intensification of the ocean currents, i.e., the Gulf Stream and the Kuroshio, as nature's way of satisfying the boundary conditions by introducing higher order frictional forces; Charney[43] showed that this could be accomplished by inertial accelerations without invoking ad hoc assumptions concerning turbulent Reynolds stresses; and Kamenkovich[44] combined the frictional and inertial processes into a single theory. Later theories explaining vertical structure were developed by Robinson and Stommel[45] and Robinson and Welander[46], but again with ad hoc coefficients of momentum, heat and salt diffusion.

Classical analysis sufficed, or at most computers were used as auxiliary devices for numerical quadrature and for solving two-point boundary value problems. Such nonlinearities as existed were mitigated by hodograph or similarity transformations. Recently, computers have been used more intrinsically by Charney and Spiegel[47], McKee[48] and Philander[49] to construct theories of the equatorial undercurrents. These are also critically dependent on the mechanism of turbulent diffusion and consequently incapable of giving truly satisfactory explanations.

Computation suffers not only from a lack of knowledge of turbulent transport mechanisms but from the existence of widely varying time scales in different layers of the oceans. These range from days or weeks in the upper layers and near the equator where the action of wind stirring is large, to years at midlatitudes and somewhat greater depths where velocity and density gradients remain appreciable, to centuries in the abyssal regions where velocity and density gradients are very small. This is characteristic of a circulation which is driven by heating and friction at the upper boundary of a fluid in a gravitational field.* Similar problems are encountered in the simulation of the deep Venus circulation if it is assumed that the continuous cloud cover prevents the penetration of solar radiation to great depths. Despite these limitations, numerical models of the global oceans have been constructed by Sarkisyan[50], Kamenkovich et al.[51], Bryan and Cox[52], and Bryan[53]. The first two avoid the multiple time problems by dealing with a homogeneous ocean; in the second two it is not clear that the deep circulation reaches a steady or statistically steady state. Nevertheless, certain features of the observed ocean circulation are simulated by all models, as for example the Gulf Stream, the Kuroshio, and, in the case of the latter two, some aspects of the vertical thermohaline structure of the oceans. Recently Cox[54] has been able to reproduce features of the Somali Current, showing that, unlike the other western boundary currents, it is much more influenced by local wind action.

The Antarctic Circumpolar Current, as its name implies, is not as obviously confined to a closed oceanic basin as the other major currents.

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* The oceans may also be driven to some extent from the bottom upwards by interaction of the tides with bottom topography.

Munk and Palmén[55] and Stommel[56] pointed out that its dynamics must also be different. It is, however, constrained by the narrow and shallow "Drake Passage", and Gill and Bryan[57] have shown by numerical simulation that the entire character of the current, including its transport, is strongly affected by the shape of this passage. If, for example, the Drake Passage were deepened, the circumpolar transport would decrease by a factor of three. The calculated current is not realistic; again, the model cries out for a better understanding of the turbulent eddy viscosities and heat conduction.

Measurements of deep ocean currents by means of neutrally-buoyant floats tracked acoustically or by current meters attached to moored buoy cables have revealed a degree of unrest in the deep oceans which threatens to overthrow some 'dream' theories and suggests entirely new mechanisms of turbulent diffusion. The Mid-Ocean Dynamics Experiment (MODE) planned for 1973 is a concerted attempt to measure these motions in a limited region of the western north Atlantic. The study is unusual because it is accompanied by a theoretical and numerical effort to examine a series of idealized models for guiding observation. Bottom topography is thought to play an important role. Theoretical analysis and numerical experiments carried out by Rhines[58] have shown that vortex shrinking and stretching due to up- and down-slope flow acts as a mechanism for trapping the energy of oceanic Rossby waves.

4. Climate and climatic change. Lorenz[59] has raised the question of the statistical transitivity of the solutions of the atmospheric or the coupled atmosphere-ocean equations. Is there a unique climate? Or is it possible that two different initial states might lead to two different, but stable, statistics? Examples of both possibilities occur in rotating,

differentially-heated, cylindrical annuli. For certain values of the boundary parameters, the flows are degenerate: different initial conditions lead to different steady or regularly fluctuating regimes. Such flows have been studied by Lorenz[60] with simple truncated Fourier and Fourier-Bessel expansions and by Charney[61] with finite point vortex elements.

No feasible method has been proposed for simulating the actual climate except by carrying out numerical calculations of the global circulation for long periods of time. When one considers that there are no accepted theories of anisotropic, nonhomogeneous turbulence of any kind, it is not to be expected that the vastly more complicated general circulation of the atmosphere-ocean system will reveal statistical regularities that would permit the inference of an _a priori_ statistical theory of climate. Seasonal averages computed from global integrations carried out for a period of one year by Manabe, et al.[62] at the National Oceanographic and Atmospheric Administration Geophysical Fluid Dynamics Laboratory at Princeton University compare reasonably well with normal climatic averages. But there are discrepancies. To what extent these are due to physical and mathematical inadequacies in the model and to what extent to the natural variability of the atmosphere from one year to the next is not yet known. Numerical calculations carried out with a simpler model, and for longer periods of time by Katayama, Mintz and Arakawa[63], with a fixed ocean temperature field but with seasonally varying solar insolation, reveal surprisingly large variations in seasonal averages from one year to the next. If the ocean temperatures had been permitted to vary, these variations would presumably have been larger.

The variations would presumably have been even larger, and perhaps of longer persistency, if the oceans had been coupled into the system. We know that certain baroclinic oscillations in the oceans have time constants of the order of centuries, and that the "overturn" time of the oceans is a thousand years (as determined from C^{14} ages). Long time series of oceanographic variables are almost entirely lacking, but where they exist, they show striking anomalies occurring for years, or even a decade. This type of observation is now being greatly accelerated under the auspices of the International Decade for Ocean Exploration.

Once realistic averages of the global circulation have been obtained, one is then in a position to carry out numerical experiments concerning the role played by variable solar input, by variable boundary conditions, etc. We know that planetary perturbations of the Earth-Moon-Sun system are associated with significant changes in the radiation pattern, such changes occurring with typical time scales of 10^4 - 10^5 years. These planetary perturbations correlate reasonably well with profiles of paleotemperatures in deep-sea cores, as determined by isotopic methods. Going back even further to time scales of 10^7 - 10^8 years, one can consider the effects of the disappearance of mountain chains, the closing of Drake Passage, and the variable distribution of oceans and continents. This promises to be an active field of computer application in the decades to come.

A beginning has been made in the development of a numerical model of the combined atmosphere-ocean circulation[64]. It has been possible to simulate the effect of ocean currents on the distribution of temperature, humidity and precipitation in the atmosphere. At the moment the most profound difficulties concern lack of knowledge of turbulent exchange processes in the oceans and the very long oceanic time scales in the deep ocean.

5. Predictability and turbulence. Predictions of the large-scale motions of the atmosphere with the most elaborate of the numerical models are found to be quite accurate for periods of one or two days and to remain superior to the climatic norm up to five days. Some positive skill persists up to nine or ten days.[65] What is the ultimate limit of accuracy? The growth of random observational error was first studied by Thompson[66] and Novikoff[67], but Lorenz[22] was the first to clearly define the problem of predictability as one of instability. He pointed out that the atmosphere is an unstable system in which small perturbations grow until ultimately the predicted flow pattern differs from the observed by as much as two states selected at random. The basic uncertainty is independent of measurement. It begins at the smallest turbulent scales and propagates toward larger scales at a rate which is roughly the characteristic time scale in the statistical equilibrium resulting from interactions among all scales. When uncertainty is introduced at scales which can be resolved by the computational grids, it is possible to investigate its further propagation by numerical simulation. At sub-grid scales its propagation has been investigated by Lorenz[23] and Leith and Kraichnan[25] using statistical turbulence models based on closure hypotheses relating higher to lower order statistical moments.

The rate of progression of uncertainty depends strongly on the nature of the turbulence spectrum. At very small scales, atmospheric and oceanic turbulence appear to satisfy the Kolmogoroff hypotheses of isotropy, homogeneity, and localness in wave-number space, which lead to the $k^{-5/3}$ dependence of the kinetic energy spectral function on the scalar wavenumber k in the so-called inertial subrange. At larger scales, velocity shear and density stratification become important, and at still larger scales the horizontal kinetic energy spectral function exhibits an approximate k_H^{-3} dependence on the horizontal scalar wavenumber k_H. In this case the characteristic time is independent of wavenumber and predictability is greatly extended; it is limited by the non-localness of the interactions in the wave-number space (Leith and Kraichnan[25]). The k_H^{-3} spectral behavior has been ascribed to the two-dimensional characteriof the flow. Onsager[68], Lee[69], Batchelor[70], and Fjørtoft[71] have pointed out that vorticity conservation in two-dimensional flow prevents the kind of energy cascade toward high wavenumbers which is produced in three-dimensional flow by the stretching of vortex tubes. This circumstance led Kraichnan[72] to postulate inertial subranges for two-dimensional turbulence in which energy injected in a given wavenumber band is transferred uniformly to lower wavenumbers whereas mean-square vorticity is transferred to higher wavenumbers. A similarity argument then gives the Kolmogoroff $k_H^{-5/3}$ law in the former range and the k_H^{-3} law in the latter. Kraichnan's hypotheses were apparently confirmed in numerical experiments carried out by Lilly[73], although similar experiments performed by Deems and Zabusky[74] yielded a k_H^{-4} dependence, corresponding to a statistical theory of Saffman's[75] based on the hypothesis that the vorticity field in statistical equilibrium may be characterized by a collection of random step-function discontinuities. Orszag[76,77] has questioned the accuracy of the numerical experiments and has proposed more accurate ones which are now underway.

In any case, Charney[78] has denied that the atmospheric flow can be regarded as two-dimensional and has shown that the k_H^{-3} behavior is due rather to rotational constraints on the large-scale flow. His theory also predicts equipartition among the horizontal components of kinetic energy and the available potential energy and therefore a k_H^{-3} dependence of the temperature variance spectrum as well. Direct measurement and numerical simulation suffer equally from an inability to determine spectra and predictability at small scales. As far as can be determined, they appear to be in accord with Charney's predictions.

In numerical determinations of predictability the practice is to calculate the evolution of a model circulation for a long period of time, store the results, then insert a small random perturbation at an intermediate time and repeat the numerical calculation. The growth of the standard deviation between the perturbed and unperturbed temperature field for the GFDL model[79] remains well below the deviation between two random states for more than two weeks. This result agrees qualitatively with the model calculations of Leith and Kraichnan[25], who find that an initial state determined with the horizontal resolution that may be expected from a satellite-based observing system results in significant predictability of the large-scale motions for more than a week.

Numerical simulations will eventually indicate what statistical quantities remain predictable for periods longer than the deterministic predictability time, i.e., the degree to which the signal due to anomalies in the surface boundary conditions, such as ocean surface temperature, etc., can be detected in the noise due to the growth of uncertainty in the initial conditions. The theory of predictability grew up with computers and there is little doubt that it will remain tied to them.

6. Tides. The theoretical tide problem is defined as follows: given the motion of the Earth, Moon, and Sun, and given the (bottom and coastal) boundaries of the world's oceans, compute the tides. Because of the complexity in the configuration of the world's oceans, this boundary value problem was beyond the scope of solution until the development of modern computers. Munk vividly remembers discussions with von Neumann on this problem in 1946. The principal difficulty, then and now, has to do with the turbulent energy dissipation in the shallow waters of marginal seas. Von Neumann suggested absorbing boundaries as a means of parameterizing the dissipation.

It may come as a surprise to this audience that the problem of tides was not disposed of by Sir Isaac Newton. The trouble with the Newtonian solution, according to which the sea surface is distorted into an equipotential surface, is that it bears no resemblance to reality. The "potential tide" is a static solution; in fact, there are all sorts of resonances of oceanic basins, whose periods are of the same order as that of the tide producing forces, thus requiring a dynamic treatment. Quasi-static solutions correspond to small values of a parameter $\varepsilon = \Omega a/c$, the ratio of the velocity of the sublunar point on a spinning Earth with angular velocity Ω and radius a, compared to the velocity of free waves.

For long gravitational waves, $c = \sqrt{gh}$ where h is ocean depth, and ε happens to be near 1, the most complex of circumstances. The problem is further complicated by the existence of other classes of waves which owe their existence to the Earth's rotation (related to Rossby waves in the atmosphere). These form equatorially and continentally trapped waves called Kelvin waves. Similar solutions exist for waves travelling around large islands, and along the continental slope, undersea escarpments and other depth "discontinuities". The normal modes are then a complex combination of different classes of solutions, and for the general case of neither large nor small ε, these were not sorted out even for basins of simple geometry until computers became available.

At the 1961 General Assembly of the IUGG in Helsinki, Pekeris flashed across the screen the first solution to the theoretical tide problem for the world's oceans, with a comment that it was in good agreement with observations. But not until eight years later was there an opportunity to examine the results.[80] The original work had been for a 12°×12° global grid; as it turned out, a tightening of the mesh system to a 1° grid did not lead to the expected improvement. Pekeris's work was performed on the Weizmann Institute's computer Golem, whose capacity grew in parallel with the tidal grid. Even today, the numerical solution remains unacceptably sensitive to seemingly minor details in the discretization of the boundary. The computational boundaries contain many re-entrant corners, and evidently these affect the eigenvalues in the finite difference approximation. Precise approximation of the free periods is crucial because the frequencies of one or more of the normal modes of the world's oceans lie close to the driving frequency (not surprising in view of the diversity of modes as mentioned above).

However, the key difficulty remains in the choice of coastal boundary conditions. To accommodate dissipation, three conditions have been used:

(i) vanishing normal velocity at coastlines[81, 82]

(ii) a specified albedo at coastlines or continental shelves[83]

(iii) specified (observed) values at coastal stations[84] and/or at selected islands.[85, 86]

There is a further problem concerning the appropriate bottom boundary conditions. The fact that the ocean bottom is not rigid, but yields under the combined effects of tidal forces and ocean loading, can alter results by a factor of two; yet is not taken into account in most of the numerical work.[87] Hendershott has now formulated this mutual interaction between the ocean and solid-Earth tides. The problem is crucial also to measurements on land of gravity, strain, or tilt; even in the very center of continents these are significantly contaminated by the effect of ocean tides, and unless properly taken into account, geophysical conclusions that one might draw will be in error.

Finally, the recent global tide calculations by Hendershott permit an estimate of the oceanic tidal dissipation, about 3×10^{12} watts. This is pleasingly close to an estimate made many years ago entirely on the basis of astronomic observations; namely, the departures of the Moon's longitude from those computed by Keplerian mechanics. The astronomic observations give only the total energy dissipated; they cannot tell whether this takes place in the ocean, the solid Earth, or for that matter on the Moon. We now know that it takes place largely in the oceans. This has an important bearing on reconstructions of the Earth-Moon history.

Here is a case in point where availability of computers has led, within a decade, to something resembling the solution of a problem. This is because the physical laws, embodied in the Laplace tidal equation (1775), were fairly well understood (apart from boundary dissipation) and could now be applied to a complex geometry. Quite the opposite holds for the atmospheric tidal problem. Here a spherically-symmetric geometry is probably adequate, but there has been a gross misjudgment as to the pertinent physical processes. The outstanding observational fact is the predominance of the solar 12^h tidal oscillation over the lunar $12^h.42$ in the ratio 15:1; gravitational theory favors the lunar tide 2:1. To account for this discrepancy a sharp resonance peak precisely at 12^h has been postulated, and a lot of theoretical effort has been spent to prove its existence. But now Siebert[88] and Chapman and Lindzen[89] have demonstrated (following a suggestion by Kelvin) that the solar tide is thermally driven, and the arguments for resonance amplification have vanished. The advance in understanding is comparable to that for ocean tides, but the role played by computers (though substantial) has been in an auxiliary capacity.

7. Spectroscopy. Computers have played a vital role in the analysis of geophysical observations. We consider only one aspect: numerical spectroscopy. This is most valuable for processes of such low frequency that analog filters are not readily available. For example, in the early days of spectral analysis of ocean waves, the recording was made on a film that could subsequently be played back at high speeds to bring the interesting frequencies into the resonance range of R.C.-filters. At the

present time, discrete sampling and numerical spectroscopy can be done with such efficiency that the numerical method competes with analog even at high frequencies. The numerical effort is necessarily extensive: a 1% resolution with 100 degrees of freedom required 10^4 data points; for multivariate processes the number is correspondingly higher. The increasing capacity of computers plus the introduction of more efficient algorithms has made it possible to keep up with most of the geophysical requirements.

Geophysics has profited in some vital ways. In some instances the numerical spectroscopy has led to the discovery of processes by the detection of unsuspected weak lines. In fields blessed with high signal/noise ratios (earthquakes, tides) the numerical spectroscopy has revealed the underlying natural background and thus the ultimate limits to detection and prediction. In other instances, the emphasis on high resolution has revealed the fine-structure of spectral peaks and with it some of the most interesting physics. This is particularly true for the normal modes of vibration of the Earth.

For certain broad-band stationary processes, turbulent motion in the atmosphere and ocean, internal and surface waves, the numerical spectroscopy has yielded some simple forms, $k^{-5/3}$, k^{-3}, ω^{-5}, ... Usually these had already been suggested from dimensional consideration, and the numerical analysis yielded the ranges, if any, over which the idealizations were applicable. Such analyses may yield simple displays of very complex time series.

One of the most interesting developments is the generalization of spectral analysis to bi-linear (and tri-linear) interactions, pioneered by John Tukey. The extent to which a frequency, $\omega = \omega_1 \pm \omega_2$, is excited by a quadratic sum- and difference-frequency interaction can be investigated in this manner. It is surprising that this bi-spectral analysis has not been more widely used; in the instances it has been applied it showed itself to be a tool of great power.

But perhaps the most important contribution of computers to the analysis of geophysical observations has been to discipline the observer. It has forced him to face up to the realities of the sampling theorem, to the unreliability of spectral estimates for finite time series. In a field where investigators remained notoriously ignorant of concepts long after thay had become familiar to their colleagues in optics, acoustics and electrical engineering, this was a welcome and overdue development.

8. Decision-making and early compaction. An increasingly important contribution by computers to geophysics is by an active, on-line participation in field experiments. One of the characteristic properties of the atmosphere and the ocean, as distinct from laboratory experiments, is the high degree of intermittency. Many of the most interesting events are episodic.

The known kurtosis in the distribution function of so many geophysical variables is an indication of intermittency. Mandelbrot attributes the "infrared catastrophy" of so many geophysical spectra to intermittency. Mollo-Christensen (the most enthusiastic proponent of intermittency) will go so far as to say that any theory that explains a geophysical process in terms of the average situation is likely to be wrong.

The implication of intermittency on geophysical data-taking is severe. Routine sampling at fixed data rates would seem to be just the wrong way to go about it: it will be highly redundant nearly all of the time, and inadequate at the rare interesting time. A fixed program of intermittent rapid sampling superposed on a standard low-sampling rate is an improvement, but does not really meet the issue. What is needed is a high rate of sampling conditioned by the rare important events. This is common sense, and is precisely the program followed when there is strong human involvement: special recording flights into severe storms, a tighter data grid over seamounts, etc., etc. This needs to be carried out automatically.

The following proposed experiment can serve as an example. Physical conditions near the deep-sea floor (temperature, current, pressure) are monitored by instrumented capsules. These are freely dropped, left on the bottom for a month to a year, and then recovered by acoustic command from a surface vessel. A miniature computer monitors the signals, and increases the sampling frequency during the occurrence of a high-frequency event. All this would be quite simple; most of the logical circuitry is required to have the computer make sensible decisions when the instrument malfunctions.

In a typical experiment of this kind, we record 10^5 data words of 12-18 bits each. Eventually the published results typically involve 10^2 numbers, whether in tabular or graphical form. Accordingly, there is a data compaction by a factor 10^3. At present all this compaction takes place after the return of the observations to the laboratory. Some of the compaction could be done by computers during the experiment.

There is then a question as to whether one uses on-line computers for early decision-making, or whether one relies on large memory banks and telemetering channels. We have come to the conclusion that the present technology favors decision-making over data storage and transmission. There are those who will argue that anything short of "complete" recovery of observations may lead to the overlooking of the unexpected, and therefore most crucial, results; that a preset program of data reduction will give you little more than what you already know. But there is an element of procrastination here, an unwillingness to think seriously about the reduction of data until the observations have been terminated. The many files of "original data" that overflow into the halls of the Scripps Institution, and that have never been looked at, would support this point of view.

The problem may be put another way: the information that is rejected in a preset decision-making program is still there, not in the file cabinets, but in the oceans and atmosphere (where it belongs). We will go after this information at some future time, again with the use of on-line computers capable of early data compaction.

What has been said here is particularly true in the study of earthquakes. Unattended instruments on the Moon and the sea floor can take advantage of on-line computers for selective sampling at critical times.

IV. DISCUSSION

Even within the limited scope of our review, it is apparent that the subject is very broad and that we have done it limited justice. A review of the first 25 years of achievements by modern computers has been a worthwhile exercise; we doubt whether this will still be the case at the end of the next 25 years. As time goes on, the subject becomes more and more like asking about the impact of telescopes on astronomy. Initially there were some effects that could be sensibly traced to these technological innovations; eventually they become so much ingrained in their fields that a discussion of the influence is nothing short of a monograph of the entire subject.

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