Director's Office: Faculty Files: Box 36: Weil, André- Member, Professor and Emeritus 1957-1959
From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

THE INSTITUTE FOR ADVANCED STUDY
OFFICE OF THE DIRECTOR
PRINCETON, NEW JERSEY

2 April 1959

Dear André:

I have the copy that you sent to me of your letter of March 31st to Woodward. As Director of the Institute, who presides over the Faculty, I have the clear duty of reprimanding you for the manner in which you have addressed a member of our Faculty, and its elected Secretary. I enjoin and entreat you not to repeat the offense.

Robert

Robert Oppenheimer

As to the matter of your letter, I am enclosing a copy of excerpts of a letter I have written to Woodward.

Professor André Weil
The Institute for Advanced Study

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cc Mr. Morgan

20 April 1959

Dear Professor Weil:

The Trustees of the Institute, meeting on April 18th, have fixed your salary, and that of your colleagues, at $22,500 a year, starting July 1, 1959.

I am glad to tell you the good news.

Very sincerely,

Robert Oppenheimer

Professor A. Weil
The Institute for Advanced Study

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For Weil

23 December 1958

To Whom It May Concern:

This is to certify that André Weil is a Permanent Member and Professor in the School of Mathematics of the Institute for Advanced Study. This is further to certify that the Institute is in session from the end of September to before Christmas, and from mid-January to early April, the exact dates this year being September 29 to December 19, 1958 and January 12 to April 10, 1959.

For the Director

Verna Hobson
Secretary to the Director

Seal

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Fac Weil

COPY

cc: Beurling
Borel
Gödel
Morse
Oppenheimer
Selberg
Whitney

March 1, 1958.

Dear Montgomery,

I am, of course, in favor of Fraisse, Grünbaum, Nakaoka. Concerning the latter, I feel just as you do. Why should we bother to wait an indefinite amount of time? Just to "play the game of contracts" with Sammy? And possibly leave Nakaoka inadequate time to take care of visas, travel funds, etc.?

It seems rather definite that Hano will go to Chicago next year. Therefore I assume that the offer to Nakaoka would be on the basis of Borel's assistant fund. Is that so? Or, if not, does it all mean that more money will be available than we had expected? If so, one might still think of Neron, perhaps. Morikawa has written to me that he will be glad to come, for one term or two terms as we may wish. I have no answer yet from Heegner; I will write him once more, and then write him off unless he answers soon.

I have had no comments from any of you to my proposed letter to W. Weaver. Presumably you all wanted to wait for the outcome of the Chicago conference. From what appears on the surface (from the copy of their report, which you sent me), that appears to be wholly negative. On the surface, it appears as a defeat for the Chicago proposal, but also shows complete inability to offer anything constructive to replace it; all it says is that everything should be just as before, except that mathematics should get more money, and that some of it should be awarded for five years; this would of course be an improvement, but does not go far enough. Also, the idea of "strengthening as many departments of mathematics as feasible" seems quite silly.

At the same time, it would be quite surprising that Chicago should give up their big project, just because the conference was not in its favor. Quite possibly the conference, in their strategy, was to be no more than a smoke screen, behind which they could go on with their manoeuvering at top level. If one thinks (as I emphatically do) that their project would have a very bad effect on the future development of mathematics in the country, one cannot depend on the results of the conference.

For these reasons, I should still be in favor: (a) of sending a letter to Weaver, more or less according to my draft, but modified so as to take into account the report on the conference and any observations which you and the rest of our colleagues may have to make; (b) giving a fairly wide circulation to the constructive portions of that letter (i.e. all those which do not deal specifically with Chicago).

With best regards

Cordially yours

/s/ A. Weil

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Weil

C
O
P
Y

3 rue Auguste-Comte
Paris 6e, France
March 24, 1958

Dr. Warren Weaver
Rockefeller Foundation
49 West 49th Street
New York 20, N. Y.

Dear Dr. Weaver:

I have had no occasion of communicatin with you since the war. But I cannot forget how Louis Rapkine introduced me to you in 1941, and how you, and, through you, the Rockefeller Foundation extended to me a helping hand which made it possible for me to subsist during those difficult times. Again in 1944, when I asked to be helped out of an intellectually intolerable situation, you were the only one to show a full understanding of my problems and to give me the assurance that at least I could depend on your active sympathy.

Fortunately it is not about my personal affairs that I have to approach you now. My appointment to the Institute for Advanced Study has settled these to my entire satisfaction. The question which I wish to discuss is far more important. It is one that will vitally affect the whole future of mathematics in this country.

I have before me two documents. One is entitled a "Proposal for the Establishment of a Center for Advanced Study in Mathematics", purportedly issued by the Department of Mathematics of the University of Chicago and dated January 15, 1958. The other is the "Report of a Conference on Training and Research Potential in Mathematics", sponsored by the National Science Foundation and held in Chicago on February 20 and 21, 1958.

The latter does not require elaborate comments. Mathematicians are supposed to be particularly sensitive to contradictions. If this is true of them when taken individually, clearly it is not so when they are assembled in a group. Otherwise such a group would hardly have stated (in "Resolution 1") that "the growth of mathematics in our society requires new programs and new ideas", and then proceeded with resolutions, the gist of which is that, in all essentials, nothing need be changed in existing methods and practices, and that everything should go on just as before, only more so. The one novelty advocated by the conference (Resolution 2) is that government agencies should award "block grants" for periods of five years; these, however, should still be "on the basis of a specific proposal from a department". On the other hand, the conference favored "the strengthening of as many departments of mathematics as feasible" (Resolution 4). As the report says that all resolutions were approved with "only a single dissenting vote", this (as well as some other resolutions, also flatly contradicting the original Chicago proposal) must have been approved by at least two of the three Chicago representatives at the conference, although it seems completely destructive of their purpose. Perhaps it took some naïveté on their part to imagine that about thirty mathematicians, representing 27

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different institutions (some of them of rather questionable importance) could be told about a possibly forthcoming cake and then willingly forego their right to a share of it. Even the most enlightened self-interest seldom takes the form of such self-denial. But this does not mean that spreading the butter as evenly as possible would necessarily help the cause of mathematical progress.

Perhaps the fact that the Chicago representatives (or at any rate two of them) signed the report of the conference means that they have abandoned their project; but I am inclined to doubt it. As the work of mathematicians with great reputations and considerable administrative experience, it has to be treated seriously, in spite or because of its sensational character, and even though the three men who launched into space this mathematical sputnik did not even bother, before doing so, to get it approved by their own department.

One need not stoop to notice the unusual amount of self-praise included in that proposal (remarkable, I think, even in our days of publicity blurbs disguised as projects). But, as it also contains some rather severe strictures upon the Institute for Advanced Study, and as I have belonged to the University of Chicago for ten years and left it for the Institute only recently, it may not be amiss for me, before entering into a discussion of the broad issues involved, to touch briefly upon my experiences at Chicago. I shall try to do this with all possible restraint, limiting myself to what seems relevant to my present purpose.

When I joined Chicago in 1947, Marshall Stone had just been overhauling the Department rather thoroughly. This had been done without my advice being given or asked for. Some of the weaknesses of his work, which later became apparent, were even then perceptible to my eyes. Nevertheless, the impetus given by this wholesale remodeling created a sensation in the mathematical world and gave the impression that Chicago would now be restored to its traditional position as a major mathematical center. The appointment of Chern, by far the soundest ever made there (and the only one, incidentally, for which I can claim some share of the credit) strengthened that impression.

Since then, the Department has witnessed nothing but stagnation; and, in such a matter, stagnation means downfall. Those who could, with some optimism, be considered as promising young men in 1946 are, to say the least, not young any more, and not promising. But all of them are now full professors, while truly promising men who, on several occasions, came to Chicago as instructors or research associates were allowed or rather made to go. Suggestions for the strengthening of the Department by the infusion of a modicum of fresh blood were consistently ignored. All power was firmly held by those three men whose signature is affixed to the Chicago "Proposal"; all three became increasingly detached from mathematics, increasingly absorbed by administrative matters, so-called University politics, the pulling of wires in Washington, and the game of contracts. By the time I left Chicago, I had reluctantly become convinced that, short of a miracle, it had very little future as a center for advanced study in mathematics.

Now Chicago is asking for the National Science Foundation to perform this miracle. They motivate their request, strangely enough, by noting that they are "weak in the important areas of algebraic geometry, algebraic function theory, etc.".

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Does an institution which has shown such lack of wisdom in handling its own affairs deserve Federal support, particularly on the huge scale of their proposal? Is it likely to show greater wisdom once money starts pouring in?

There are, it seems to me, two simple objective tests by which to judge whether an academic institution deserves support. It must have shown consistent wisdom in assembling, keeping together and gradually strengthening a really active team of scientists, including young men whose merit has not yet become widely publicized. It must show its ability to attract (and not by money alone) the scientists whom it wishes to acquire. The facts quoted above, to which I could add many more, show how Chicago meets the first test. As to the latter, it is a matter of public knowledge that, since I came away about a year ago, they have made major offers to no less than five mathematicians of worldwide reputation; all five, one after the other, have turned the offers down.

I have said enough about Chicago. At any rate, their proposal suggests that, in the eyes of some experienced people, the present situation has created an opportunity for obtaining Federal support on a fairly large scale for the most advanced type of mathematical work in this country. Perhaps the real need is for improvement in the teaching of mathematics (and not only of mathematics) at the secondary level. Perhaps, too, support of mathematics at the highest level, in order to reach maximum efficiency, ought to be offered on an international scale rather than within the boundaries of one country; in this, the work of the old General Education Board might serve as a truly admirable model. It may not be practical to raise the latter issue; I am hardly competent to discuss the former. I must therefore confine myself to considering the possibilities for supporting and improving advanced mathematics in the U.S.A.

To create a mathematical center ex nihilo is, it seems to me, quite beyond the scope and possibilities of any agency or foundation. It would require, on the part of its founders, a most unusual combination of mathematical insight, human understanding and organizing abilities. Moreover, since the supply of first-rate mathematicians in this or any country is quite limited, it would imply the destruction or weakening of some existing centers, which is hardly a desirable goal in itself.

Federal support must therefore necessarily be restricted to the helping and strengthening of existing centers, although not at each other's expense. Before doing this, one must recognize them. This is not to be done by locating on the map the points of highest concentration of members of the National Academy or of recipients of honorary degrees, but by applying the two simple tests mentioned above. The questions to be asked are these: which institutions have shown the greatest wisdom in assembling and keeping together an active department and particularly in selecting the most talented young mathematicians? Which institutions have acquired such a reputation for a favorable scientific atmosphere and for the encouragement given to research that some of the best mathematicians will gladly seize an opportunity of joining them?

Judging by these standards, I see two, and no more than two, mathematical centers in this country which cover a really wide area of modern mathematics. One is in Princeton, and consists of the University and the Institute for Advanced Study.

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The other is in Cambridge, and comprises Harvard and the Massachusetts Institute of Technology. On a smaller scale, and within the limitations of much smaller budgets, several departments of mathematics have shown high merit in recent years. It would be invidious to attempt making a limitative list, but the names which occur at once are those of Johns Hopkins, Michigan, Northwestern, Stanford, and, quite lately, Berkeley. I am purposely omitting from my list those institutions (such as the Institute of Mathematical Sciences at New York University, the Kansas group, etc.), whatever their merits, whose very existence, unless I am much mistaken, depends upon contracts from the armed forces, and which, for obvious reasons, specialize largely in so-called applied mathematics. It seems to me that the whole relation between pure and applied mathematics, and between the latter and the applications of mathematics, ought to be considered afresh, but that this could hardly be done within the framework of the present set-up. I shall therefore, for the time being, disregard this extremely delicate problem altogether.

Speaking only of pure mathematics, I suggest that there should be a certain number (and a relatively small one, perhaps no more than five or six) of Federally recognized "centers of advanced study in mathematics", such recognition being extended on the basis of the above tests. The recognition should not be permanent, but subject to review at stated intervals (say, every fifth year). A center could consist of one institution, or of several institutions so close to each other as to afford convenient daily contacts between their faculties, research-workers and students.

In order to win recognition, an institution would have to keep its faculty (by which I mean that portion of its faculty to be considered as part of the proposed center) reasonably free to devote themselves to research, i.e. reasonably free from administration and routine teaching. It is my experience that the teaching of one undergraduate and one graduate course (with no administrative work added to this) is quite compatible with research, except for those few mathematicians who are unusually allergic to teaching or unusually conscientious teachers. This limitation should be kept most strictly in the case of the younger men, as teaching is more of an imposition on them than on those with greater experience. It goes without saying that temporary members of the centers, brought in by means of outside funds, should have no such duties whatsoever, while being free to give advanced lectures or take part in seminars if they wish.

After recognition has been granted to such centers, what should be done to support them? The answer seems simple enough. Nothing spectacular or sensational need be attempted. One should help those centers to do better and more conveniently what they are already trying to do, and what they are on the whole, in their own bungling fashion, doing fairly well. Here again, the policy and practice of the Rockefeller Foundation (whose motto, as I remember, used to be: "Make the peaks higher, don't try to fill up the valleys") might well be taken as a model. These simple principles have a number of important corollaries.

First and most important, the N.S.F. should substitute itself for all the agencies of the armed forces (O.O.R., O.N.R., A.F.O.S.R.) so far as pure mathematics is concerned. It is right and proper that these agencies should subsidize the types of applied mathematical research (such as ballistics, operational research, etc.) which are of direct and immediate interest to them.

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In addition to this, they have, in recent years, taken the remarkably farsighted view that whatever is good for mathematics is, in the long run, good for the armed forces; and they have given truly invaluable help to research in pure mathematics. However grateful we mathematicians should be to them for such help, generously granted during a delicate transitional period, it is now time to say, firstly, that this is hardly their proper function, and, secondly, that it has enmeshed us in a web of red tape and confusion from which it is becoming ever harder to extricate ourselves. In order merely to keep track of the various contracts on which most major institutions have become increasingly dependent for their normal operation, a special staff has become necessary. Some contracts are free from any conditions; some are "with strings attached" (as to subject-matter, e.g. analysis vs. number-theory; as to citizenship of the men under contract; etc.). An elaborate reviewing machinery has been set up, most of which, as everybody knows, is wholly fictitious, since proposals from mathematical top brass are considered quite apart from their scientific merit (by which I do not intend to suggest that they have none, but merely that it is not a relevant consideration, since, quite properly in many cases, it is taken for granted on the basis of the names on the project). Complicated deals are being made all the time, in order, broadly speaking, to accommodate all deserving men while ensuring some superficially satisfactory distribution between the various agencies and between the various institutions. Because of the red tape involved and because of the uncertainties of the Federal budget until the last moment, long-range planning is out of the question, and it is frequently impossible to make definite plans even a few months in advance. Within the mathematical world, the adepts at this peculiar game (the "game of contracts") have become ever more indispensable and have come to devote more and more of their time to it, at the expense of honest mathematical work. This is clearly exemplified by the situation at the University of Chicago, as described above. It is true that "block grants", awarded for five-year periods, would be a partial remedy to some of these evils; but they would not cure the disease.

All this could easily be brushed away at one stroke, at no expense and even with some saving of the taxpayer's money, if it were agreed that all Federal support of pure mathematics should be the responsibility of the N.S.F., and that a stated proportion of this should be in the form of grants, with no strings attached, to those institutions which have deserved such support, i.e. to those which have won recognition as centers for advanced study in mathematics. This would still leave room for contracts in favor of deserving individuals or groups, not belonging to such centers (or who for some reason would prefer to dissociate themselves from the latter), as well as for contracts (from the N.S.F., or, within the spheres properly belonging to their range of interest, from the agencies of the armed forces) to special groups working on specific problems. As to the grants to the recognized centers, their use should be subject only to one condition, viz., that it should conform to the broad purposes specified in the general regulations governing such grants.

These grants should be administered by the departments of mathematics of the institutions making up those centers, or by suitably designated sub-committees. If a center consists of more than one institution, a joint committee would take care of the matter, with, I hope, a minimum of red tape. For instance, if one assumes, for the sake of argument, that Princeton were to become such a center, there would have to be a joint committee, from the Department of Mathematics

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of Princeton University and the School of Mathematics at the Institute. Here I should emphasize what must have been obvious from the beginning of this letter, viz., that I am here expressing merely my personal views, and not those of my colleagues at the Institute, still less the collective views of the Institute itself.

In such grants, overhead, traveling and secretarial expenses should be abolished. The former, so far as pure mathematics is concerned, has no justification at all, except as a hidden subsidy to the institutions benefiting from contracts. Secretarial expenses, in pure mathematics, are almost non-existent, as soon as red tape is cut down to a minimum. A certain amount of traveling is definitely useful; but, also in this respect, existing practices ought to be reconsidered. Firstly, the vicious usage of holding, chiefly for prestige purposes, innumerable conferences, colloquia, symposia (my Greek dictionary says: "Symposium, a drinking party") ought to be stopped. The organizers of those conferences are frequently heard to say that they are useful, not because of their formal aspects, but by providing opportunities for informal contacts. Why, then, not dispense with the formal aspects altogether? This could easily be achieved by allotting, to each recognized center and perhaps also to some distinguished scientists in their individual capacity, a fund, to be used at their discretion for informal contacts and consultation with colleagues located elsewhere. Not only would this be far more useful than the system now in vogue, but it would be less expensive. Scientists are seldom extravagant; and public opinion within the profession would deal quickly with the few who might use such funds for unworthy purposes.

According to this plan, the grants to recognized centers would be used essentially as contracts are used at present, viz., in order to recruit and attract research-workers for comparatively short or medium periods of time (from one semester, say, to two years); the support of predoctoral students ought rather to be on a separate basis, as it is now. It should be clearly specified that, to the individual scientists benefiting from such a grant, the grant is a free gift, involving no obligation, and therefore tax-exempt; the abolition of overhead would amply compensate for the very small loss to the Treasury implied in this. Even now, it is always understood that, in the realm of pure mathematics, the so-called "contracts" are purely fictitious, the beneficiaries being left free to devote themselves to their own problems and to spend their time as they see fit; there is no reason why this should not be openly acknowledged by granting them the same status enjoyed by Guggenheim fellows, members of the Institute, etc.

Among those who would be eligible for grants from the recognized centers, one could include professors on sabbatical leave, who at present have to resort to various devices in order to make up for the usual loss of salary while on half-pay. On the other hand, it might be wiser to provide a special fund for them.

In addition to all this, it seems quite desirable (although not necessary) that the N.S.F. should set up and endow a small number of research professorships with tenure. The number should be kept small, because the number of mathematicians qualifying for such positions is small, unless existing institutions are to be depleted of their best personnel. As to the machinery involved (which again would have to be kept down to a bare minimum), I have not sufficient administrative experience to make definite suggestions. A professorship of that type could either be attached to an existing institution, or to a recognized center consisting of

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more than one institution, or directly to the N.S.F.; in the latter case, the holder might be left free to reside at any recognized center of his own choice. If, for the sake of argument, a professorship were to be attached to a center to be recognized in Princeton, the holder would be free to do his work (including, if he so wishes, seminars or advanced lectures, but no routine teaching) at the University, at the Institute, or at both; in view of the very small number of cases involved, I cannot believe that such petty matters as the allocation of office-space, etc., could ever be a serious cause of friction; on the contrary, an arrangement of this kind would be invaluable for promoting scientific cooperation. The very small amount of administrative work to be done (which would hardly include more than the writing of salary checks) could be handled, I imagine, either by the N.S.F. or by the institution most directly concerned.

The problem of housing is of considerable importance in any program involving moderately large numbers of research-workers spending periods of an average duration of one year away from their normal residence. The Institute for Advanced Study has solved the problem for its members by means of its housing project. Since comparable housing does not at present exist elsewhere, a professor on sabbatical leave, a postdoctoral student on an N.S.F. fellowship, a Guggenheim fellow, have almost no option but to go to the Institute or go abroad; they will often go to Princeton, even though their own scientific interests might have directed them elsewhere. Certainly such concentration is neither necessary nor desirable. The Institute Project could serve as model in some cases. Anyway, each center should have housing units, adequate to its expected needs, and allowing, if possible, some margin for future expansion. In most cases, this can hardly be achieved without a capital grant from Federal funds. On the other hand, it would seem only fair that those institutions which already have housing should receive adequate compensation if they are to put some of it at the disposal of a Federally recognized center of which they might be no more than a component part.

The Chicago "Proposal" lays great stress on offices, these being, it says, "the laboratory of the research mathematician". This seems an exaggeration. European mathematicians have not been doing so badly, even though they seldom enjoy such facilities. In this country, too, there are mathematicians who do most of their work at home. Let me not, however, contest this issue. Private offices in departmental buildings can do no harm; for those who have to live in congested quarters, or who find their family more disturbing than students and visitors, they can do much good. Providing office-space requires money. Let there be money, by all means. Money cannot create good mathematics. Used wisely in moderate amounts, it can help mathematics considerably. My earnest hope is that it will be so used.

Yours sincerely,

/s/ A. Weil

AWcdu

André Weil

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cc: Professors Beurling
Gödel
Borel
Morse
Oppenheimer
Selberg
Whitney

April 18, 1958

Dear Deane:

I just found your letter of April 1 on coming back from a trip to Vienna. Maybe, in view of the date, it ought not to be taken seriously. On the other hand, if it is, I am sorry that you should want to leave the Annals, especially if this is going to involve me. Perhaps you could postpone the final decision until we can talk it over together? In any case I could not possibly start with such duties until I have had the opportunity of discussing things with other members of the editorial board; in fact, if I accept the job at all, I should prefer to begin only in the Fall. As you know, I shall be in Princeton from June 10 to June 30. I hope you won't mind postponing the whole matter until I get there.

Are there precedents to the arrangement proposed by you concerning Ambrose and Zippin? If it just means using up Air Force money which otherwise would be wasted, then of course I am all for it. Otherwise there are matters of general policy involved in this, about which I am not at all clear. As to Ambrose's request concerning the year 1959-60, I hope it can wait until the Fall; I think well of Ambrose, but I am not at all in favor of dealing with such matters piecemeal, and I think his request has to be considered along with other possibilities for the same year.

With best regards

Cordially yours,

/s/ A. Weil

A. Weil

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Weil

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February 13, 1958

Professor R. Brauer
President
American Mathematical Society
Providence, R. I.

Dear Professor Brauer:

This is my third year as representative of the Society on the Editorial Board of the American Journal. The circumstances which led to my accepting that assignment have been so completely altered by Wintner's premature and sudden death that I must now ask to be released from it. It should be understood that my resignation is final and not subject to any conditions. But I owe the Society a statement of the reasons which have motivated this decision.

It is not necessary, but it seems clearly desirable and convenient, that the greater part of the best mathematical work done in each country should be concentrated in one or more first-class journals rather than diluted with inferior material. Individuals and institutions of limited means, particularly in the poorer countries, can then make an intelligent selection of those periodicals to which they wish to subscribe. Also, the chances for each mathematician to learn something worthwhile by casual search are much enhanced if he knows where to look for work of high quality. A careful analysis of American mathematics has convinced me that enough work of that kind is being produced here to keep up two first-class journals, and no more than two. This takes into account the very welcome fact that foreign mathematicians will occasionally offer some of their best work for publication in this country.

Of the existing journals in the U.S.A., the American Journal of Mathematics has the longest and most impressive tradition. For many years, largely because of Lefschetz's superb editorship, the Annals of Mathematics have held the first place, while the American Journal was gradually sinking into mediocrity. As to the journals published by the Society and by a number of academic institutions on this continent, it has obviously never been intended or attempted to maintain for them any high standard of excellence, with the possible exception of the Transactions during Tamarkin's editorship. These statements are not meant as a criticism. For a number of reasons, scientific and sociological, it is unavoidable that a good deal of comparatively inferior material should get into print. I am merely trying to describe the main facts as I see them. Of course I am aware that a less summary description would reveal a number of deviations from the general trend.

Very properly, when the Society made me an editor of the American Journal, they refrained from giving me any directives; nor did I ask for any. I was left completely free to exercise my own judgment concerning editorial policy. I found that Wintner was extremely desirous of restoring the Journal to its original function as a first-class periodical. For the reasons stated above, I, too, felt strongly that this was something worth working for. We set about doing it; and I believe that we reached a considerable measure of success. This implied

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rejecting many papers which, by the usual standards, were perfectly publishable. We took the view that, by doing so, we did little harm to the authors, provided it was done quickly; for such papers would get into print anyway. This applied particularly to the case of well-established authors; we thought that promising young beginners should receive a more considerate treatment and should at times be measured by less exacting standards. We could not hope to be exempt from errors; whether or not quick decisions imply a higher percentage of errors than a slower procedure is a debatable point. Had we been dictators over the whole field of mathematical publications, the weight of our responsibility would have been crushing; since, however, opportunities for publication of even moderately deserving papers remained plentiful even after rejection from the Journal, we found that we could carry it lightly.

Unrealistically high standards would have stifled the Journal altogether; this had to be avoided. On the other hand, our policy made it possible to reduce to a bare minimum the backlog which is the curse of most journals. We found that we had no difficulty in publishing quickly whatever we thought worth publishing at all.

These were our principles; and I should not in the least be interested in editorial work in a journal where such principles could not be maintained. When several editors have to work together, it is almost inevitable that the weakest one will drag his colleagues down to the level of his own standards. But no one can maintain high standards unless he has had considerable experience in more than one branch of mathematics, and, above all, unless he enjoys a secure and unassailable position in the mathematical world. Since Wintner's death, there is, except for Chow, no such person at Johns Hopkins.

I greatly admire Chow's success in assembling there a really fine group of active young mathematicians, and in doing so in spite of the limitations of a budget barely consonant with the distinguished traditions of that institution. It is nevertheless a fact that no one there, with the exception of Chow himself, would be in a position to assume the duties of an editor with the degree of competence and independence which I consider necessary in the present situation. To this should be added the fact that an editor, in order to discharge his duties efficiently, must have a modicum of punctuality and of businesslike qualities which not every good mathematician can be expected to possess. As to Chow, he was already most reluctant to act as editor in Wintner's lifetime; it was only by pointing out to him that there was no workable alternative that Wintner and I persuaded him to accept such duties.

Should the Society be as convinced as I am that another first-class journal, besides the Annals, is desirable, it could perhaps insist with Johns Hopkins that they do the needful to maintain the standards which Wintner and I tried to achieve. But, should it be so convinced, it could far more easily make its own publications conform with such standards. Indeed, while the Society never expressed disapproval of the principles I followed as editor of the Journal, it never indicated that it approved of them.

This being so, I feel that whatever abilities I may possess as editor would be wasted if I spent more time on the Journal, and that many of my

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colleagues can do, better than I, the kind of editorial work which it will require from now on.

To these considerations should be added the inconvenience of performing my editorial duties from Paris, where I am spending the current year. As long as I felt useful, I did not mind this. Since such is no longer the case, I must naturally wish to be relieved of these duties as early as practicable.

It has been traditional for the Society always to appoint their representatives on the editorial board of the American Journal in consultation with the Johns Hopkins editors; in the present situation, this means consulting with Chow. In order to give you time to do this without undue hurry, I am making my resignation effective as from April 1, 1958.

As the present letter raises issues of policy, I should be much obliged if you would kindly have it circulated among all members of the Council of the Society.

Yours sincerely,

/s/ A. Weil

A. Weil

cc: W. L. Chow

Mimeographed copies to:
L. Ahlfors
A. Borel
H. Cartan
C. Chevalley
S. S. Chern
J. Dieudonné
K. Iwasawa
K. Kodaira
J. W. Milnor
F. Mautner
R. Oppenheimer

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From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

Dr. Oppenheimer

Paris, Feb. 14, 1958

Dear Colleagues:

As agreed before I left Princeton, I am sending the draft of a letter to W. Weaver. I should be most grateful if you would send me as general or as detailed comments on this as you care to make. Of course the letter has only to express my personal views, not yours or those of the Institute. On the other hand, I have drafted this on the spur of the moment; my opinions are very far from being solidified, and confronting them with yours may still lead me to modify them considerably. Also, you may be in a position to point out to me that some of the statements I make in that letter, even though I mean them, may be unwise under the circumstances; in this, again, I should very much wish to have the benefit of your reactions and of your experience. For the final draft, which I shall make on the basis of the present one and of your comments, I must of course claim exclusive responsibility.

As the matter seems rather urgent, please write to me quite soon. You will get a copy of the final draft as soon as it is ready.

Sincerely yours,

A. Weil

A. Weil
3 rue Auguste-Comte
Paris 6e, France

cc: School of Math. Faculty

- -

D R A F T

Dear Dr. Weaver:

I have had no occasion of communicating with you since the war. But I cannot forget how Louis Rapkine introduced me to you in 1941, and how you, and, through you, the Rockefeller Foundation extended to me a helping hand which made it possible for me to subsist during those difficult times. Again in 1944, when I asked to be helped out of an intellectually intolerable situation, you were the only one to show a full understanding of my problems and to give me the assurance that at least I could depend on your active sympathy.

Fortunately it is not about my personal affairs that I have to approach you now. My appointment to the Institute for Advanced Study has settled these to my entire satisfaction. The question which I wish to discuss is far more important; it is one, in fact, that will vitally affect the whole future of mathematics in this country.

I have before me a "Proposal for the Establishment of a Center for Advanced Study in Mathematics", purportedly issued by "the Department of Mathematics of the University of Chicago". This mathematical sputnik, launched into space by three Chicago professors (who, incidentally, did not even bother, before doing so, to get it approved by their department) contains a certain amount of self-praise, unusual, perhaps, even in our days of publicity-blurbs disguised as projects. It also contains a number of severe strictures upon the Institute for Advanced Study.

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I have belonged to the University of Chicago for ten years. When the opportunity arose for me recently to leave it for the Institute, I found that I was no less desirous, on scientific grounds alone, of leaving Chicago than of joining the Institute. I had, in fact, become convinced that Chicago (barring a miracle) had very little future as a center for advanced study in mathematics, while the Institute had one fully worthy of its tradition. Perhaps, before entering into a discussion of the broad issues involved, I may make a few comments concerning my experiences in Chicago; I shall try to do this with all possible restraint.

When I joined Chicago in 1947, Marshall Stone had just overhauled the department. This had been done without my advice being given or asked for; some of the weaknesses of his creation, which later became apparent, were even then perceptible to my eyes. Nevertheless, the impetus given by this wholesale renewal created a sensation in the mathematical world and gave the impression that Chicago would now be restored to its traditional position as a major mathematical center. The appointment of Chern, by far the soundest ever made there (and the only one, incidentally, in which I had some share) strengthened that impression.

Since then, the department witnessed complete stagnation; and, in such a matter, stagnation means downfall. Those who could, with some optimism, be considered as promising young men in 1946 are, to say the least, not young any more, and not promising. But all of them are now full professors, while truly promising men who, on several occasions, came to Chicago as instructors or research associates were allowed or rather made to go. Suggestions for the strengthening of the department by the infusion of a modicum of fresh blood were consistently ignored. All power was firmly held by those three men whose signature is now affixed to the Chicago "Proposal"; all three became increasingly detached from mathematics, increasingly absorbed by administrative matters, so-called university politics, the pulling of wires in Washington, and the game of contracts.

Now Chicago is asking for Federal support; it seeks to motivate this request by noting that it is "weak in the important areas of algebraic geometry, etc." Had a proper climate been created for me to be able to do really useful work there, I might not have left Chicago. Even if I had left it, Chicago could very easily have retained the services, say, of Lang (an instructor there a few years ago, now an associate professor at Columbia) and of Matsusaka (a research associate on an N.S.F. contract at Chicago until last year, now an associate professor at Northwestern), both of whom I wished to keep there, and both of whom were quite eager to stay; it would then not find itself "weak in algebraic geometry". Does an institution which has shown such lack of wisdom in handling its own affairs deserve Federal support, particularly on the huge scale of the present proposal? Is it likely to show greater wisdom once money starts pouring in?

There are, it seems to me, two simple objective tests by which to judge whether an academic institution deserves support. It must have shown consistent wisdom in assembling, keeping together and gradually strengthening a really active group of scientists, including young ones whose merit has not yet become widely publicized. It must show its ability to attract (and not by money alone) the scientists whom it wishes to acquire. The facts quoted above, to which many more could be added, indicate that Chicago hardly meets the first test. As to the second one, it is a matter of public knowledge that, since I came away not quite a year ago, they have made major offers to no less than five mathematicians of worldwide reputation; all five, one after the other, have turned the offers down.

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I have said enough about Chicago. Their proposal indicates, at any rate, that the present situation has created an opportunity for obtaining Federal support, on a fairly large scale, for the most advanced type of mathematical work in this country. Perhaps the real need is for improvement in the teaching of mathematics at the secondary level; perhaps, too, support of mathematics at the highest level, in order to reach maximum efficiency, ought to be offered on an international scale rather than within the boundaries of one country; in this, the work of the old General Education Board might serve as a truly admirable model. It may not be practical to raise the latter issue; I am hardly competent to discuss the former. I must therefore limit myself to considering the possibilities for supporting and improving advanced mathematics in the U.S.A.

To create a mathematical center ex nihilo is, it seems to me, quite beyond the scope and possibilities of any agency or foundation. It would require, on the part of its founders, a most unusual combination of mathematical insight, human understanding and organizing abilities. Moreover, since the supply of first-rate mathematicians in this or any country is quite limited, it would imply the destruction or weakening of some existing centers, which is hardly a desirable goal in itself.

Federal support must therefore necessarily limit itself to helping and strengthening existing centers, although not at each other's expense. Before doing this, one must recognize them. This is not to be done by locating on the map the points of highest concentration of members of the National Academy or of recipients of honorary degrees, but by applying the two simple tests mentioned above. The questions to be asked are these: which institutions have shown the greatest wisdom in assembling an active department and particularly in selecting the most talented young mathematicians? Which institutions have acquired such reputation for a favorable scientific atmosphere and for the encouragement of scientific work that some of the best mathematicians will gladly take an opportunity of joining them?

Judging by these standards, I see two, and no more than two, mathematical centers in this country which cover a really wide area of modern mathematics. One is in Princeton, and consists of the University and the Institute; the other is in Cambridge, and consists of Harvard and the Massachusetts Institute of Technology. On a smaller scale, and within the limitations of much smaller budgets, several institutions have shown high merit in recent years. It would be invidious to attempt making a limitative list, but the names which occur at once are Johns Hopkins, Michigan, Northwestern, Stanford, and perhaps, lately, Berkeley. I am purposely omitting from my list those institutions (such as the Institute of Mathematical Sciences at N.Y.U., the Kansas group, etc.), whatever their merits, whose very existence, unless I am much mistaken, depends upon contracts from the armed forces; for obvious reasons, they specialize largely in so-called applied mathematics. It seems to me that the whole relation between pure and applied mathematics, and between the latter and the applications of mathematics, ought to be considered afresh, but that this could hardly be done within the framework of the present set-up. I shall, therefore, for the time being, disregard this extremely delicate problem altogether.

Speaking only of pure mathematics, I suggest that there should be a certain number of Federally recognized "centers of advanced study in mathematics", such recognition being extended on the basis of the above tests. The recognition should

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not be permanent, but subject to review at stated invervals (say, every fifth year). A center could consist of one institution, or of several institutions so close to each other as to afford convenient daily contacts between their Faculties, research-workers and students.

In order to win recognition, an institution would have to keep its Faculty (or rather that portion of its Faculty to be considered as part of the proposed center) reasonably free to devote themselves to research, i.e. reasonably free from administration and routine teaching. It is my experience that the teaching of one undergraduate and one graduate course (with no administrative work added to this) is quite compatible with research, except for those few mathematicians who are unusually allergic to teaching or unusually conscientious teachers. This limitation should be most strictly adhered to in the case of the younger men, as teaching is more of an imposition on them than on those with greater experience. It goes without saying that temporary members of the centers, brought in by means of outside funds, should have no such duties whatsoever, while being free to give advanced lectures or take part in seminars if they wish to do so.

After recognition has been granted to such centers, what should be done to support them? I may be miserably lacking in constructive imagination; but, to me, the answer seems simple enough. Nothing spectacular or sensational should be attempted. One should help those centers to do better and more conveniently what they are already trying to do, and what they are on the whole, in their own bungling fashion, doing fairly well. Here again, the policy and practice of the Rockefeller Foundation (whose motto, as I remember, used to be: "Make the peaks higher, don't try to fill up the valleys") might well be taken as a model. These simple principles have a number of important consequences.

First and most important, the N.S.F. should substitute itself for all the agencies of the armed forces (O.O.R., O.N.R., A.F.O.S.R.), so far at least as pure mathematics is concerned. These agencies, in recent years, have taken the remarkably farsighted view that whatever is good for mathematics is, in the long run, good for the armed forces; and they have given invaluable help to mathematical research. However grateful we mathematicians should be to them, it is now time to say, firstly that this is hardly their proper function, and, secondly, that this has enmeshed us in a web of red tape and confusion from which it is becoming ever harder to extricate ourselves. In order merely to keep track of the various contracts on which most major institutions have become increasingly dependent for their normal operation, a special staff has become necessary. Some contracts are free from any conditions; some are "with strings attached" (as to subject-matter, e.g. analysis or partial differential equations vs. number-theory; as to the citizenship of the mathematicians involved; etc.). An elaborate reviewing machinery has been set up, much of which, as everybody know, is wholly fictitious, since proposals from mathematical top brass are considered quite apart from their scientific merit (by which I am not trying to suggest that they have none, but merely that it is not a relevant consideration, since, quite properly in many cases, it is taken for granted on the basis of the names on the proposal). Complicated deals are being made all the time, in order, broadly speaking, to accommodate all deserving men while ensuring some superficially satisfactory distribution between the various agencies and between the various institutions. Because of the red tape involved and because of the uncertainties of the Federal budget until the last moment, long-range planning is out of the question, and it is frequently impossible to make definite plans even a few months in advance. Within the mathematical world,

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the adepts at this peculiar game (the "game of contracts") have become ever more indispensable and have come to devote more and more of their time to it, at the expense of honest mathematical work. This is clearly exemplified by the situation at the University of Chicago, as described above.

All this could easily be brushed away at one stroke, at no expense and even with some saving of the taxpayer's money, if it were agreed that all Federal support of pure mathematics should be the responsibility of the National Science Foundation, and that a stated proportion of this should be in the form of annual grants, with no strings attached, to those institutions which have deserved such support, i.e. essentially to those which have won recognition as centers for advanced study in mathematics. This would still leave room for individual contracts in favor of deserving individuals not belonging to these centers. Naturally this suggestion will hardly be welcomed by many specialists in the game of contracts and members of various committees, who will thereby be deprived of the mainspring of their present power and of the opportunity for frequent trips to Washington. Perhaps for that reason it is wholly unpractical.

The grants to the centers should be administered by the departments of mathematics of the institutions making up those centers, or by suitably designated subcommittees. If a center consists of more than one institution, a joint committee would take care of the matter, with, I hope, a minimum of red tape. For instance, assuming, for argument's sake, that Princeton would become such a center, there would have to be a joint committee, from the department of mathematics of Princeton University and the School of Mathematics at the Institute. Here I should emphasize what must have been obvious from the beginning of this letter, viz., that I am here expressing merely my personal views, and not those of my future colleagues at the Institute, still less the collective views of the Institute itself.

In such grants, overhead, traveling and secretarial expenses should be abolished. The former, so far as pure mathematics is concerned, has no justification at all, except as a hidden subsidy to the institutions benefiting from contracts. Secretarial expenses, in pure mathematics, are almost non-existent, as soon as red tape is cut down to a minimum. A certain amount of traveling is definitely useful; but in this respect, too, existing practices ought urgently to be reconsidered. Firstly, the vicious practice of holding, for prestige purposes only, innumerable conferences, colloquia, symposia (my Greek dictionary says: "Symposium, a drinking party") ought to be stopped. The organizers of such conferences are frequently heard to say that these are useful, not because of their formal aspects, but by providing opportunities for informal contacts. Why, then, not dispense with the formal aspects altogether? This could easily be achieved by allowing, to each recognized center and to some distinguished scientists, a fund, to be used at their discretion for purposes of informal contacts and consultation with colleagues located elsewhere. Not only would this be far more useful than the system now in vogue, but it would be less expensive. Scientists are seldom extravagant; and public opinion within the profession would deal quickly with the few who might use such funds for unworthy purposes.

According to this plan, the grants to recognized centers would be used essentially as contracts are used at present, viz., in order to recruit and attract research-workers for comparatively short or medium periods of time (from one semester, say, to two or three years). This could include professors on sabbatical leave, who at present have to depend on various kinds of outside help (whether from

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a contract, a Guggenheim fellowship, a grant from the Institute for Advanced Study, etc.) to make up for the usual loss of salary while on half-pay. On the other hand, a special fund might well be provided for the latter purpose; in fact, the N.S.F. has already virtually recognized, in practice if not in theory, the usefulness, from a general point of view, of an occasional change of scientific atmosphere for a mathematician in the teaching profession.

Furthermore, it seems rather desirable (although not necessary) that a small number of research professorships with tenure should be set up by the N.S.F. The number should be small, because the number of mathematicians qualifying for such positions is small, unless existing institutions are to be depleted of their best personnel. As to the machinery involved (which again would have to be reduced to a bare minimum), I have not sufficient experience to make definite suggestions. Such professorships could either be attached to an existing institution, or to a recognized center consisting of more than one institution, or directly to the N.S.F.; in the latter case, the holder might be left free to reside at any recognized center of his own choice. If, for argument's sake, such a professorship should be attached to a center to be recognized in Princeton, the holder would be free to do his work, and, if he so wishes, to give advanced lectures, at the University, at the Institute, or at both, at his option; in view of the very small number of cases involved, I cannot believe that such petty matters as the allocation of office-space, etc., could ever be a serious cause of friction. The very small amount of administrative work to be done (which would hardly amount to more than the writing of salary-checks) could be handled, I imagine, either by the N.S.F., or by the institution most directly interested.

The problem of housing is of considerable importance in any program involving moderately large numbers of research-workers spending periods of an average duration of one year away from their normal residence. The Institute for Advanced Study has solved the problem for its members by means of its housing project. Since comparable housing does not at present exist elsewhere, a professor on sabbatical leave, a postdoctoral student on an N.S.F. fellowship, has almost no option but to go to the Institute, unless he decides to go abroad; he will often do this, even though his own scientific interests might rather have directed him elsewhere. Certainly such concentration is neither necessary nor desirable. The Institute project might serve as model in some cases. Anyway, each center should have housing units, adequate to its real needs; in most cases, this can hardly be achieved without a capital grant from Federal money. On the other hand, it would seem only fair that those institutions which already have some housing should be granted adequate compensation if they are to put some of it at the disposal of a Federally recognized center of which they might be no more than a component part.

The Chicago "Proposal" lays great stress on offices, these being, it says "the laboratory of the research mathematician". This seems an exaggeration. European mathematicians have not been doing so badly, even though they seldom enjoy such facilities. Even in this country, there are still many mathematicians who do most of their work at home. Let me not, however, contest this issue. Private offices in departmental buildings can certainly do no harm; for those who have to live in congested quarters, or who find their family more disturbing than students and visitors, they can do much good. Providing office-space requires money. Let there be money, by all means. Money cannot create good mathematics. Wisely used in moderate amounts, it can help mathematics greatly. My earnest hope is that it will be so used.

Yours sincerely,

A. Weil

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From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

17 March 1958

Dear Dr. Weil:

Thank you for your note of March 12th. A word about the cultural exchanges between the U. S. A. and the U. S. S. R. The agreement which you mention was negotiated between Lacy and Zaroubin, and I saw it as soon as it was made public. To me also it suggested that exchanges desired by the Institute would probably best be dealt with through the National Academy; I therefore at once sent the entire file of our correspondence about Bogoliubov and Landau to Bronk. He told me that he hoped to get to Moscow this spring, to speak with Nesmyanov about the whole program, and that he would discuss with him the Institute's invitations. I do not know whether this plan will really be carried out.

Bronk wrote me saying that whereas the Academy would always be glad to be helpful in arranging exchanges, he thought it important that academic institutions issue what invitations they wished quite on their own. Clearly he approved both the substance and the form of what we had done.

Within a month we hope to be taking off for Europe.

With all good wishes,

Robert Oppenheimer

Dr. André Weil
3 rue Auguste-Comte
Paris 6
France

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From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

Fac. Weil

AMERICAN JOURNAL OF MATHEMATICS
THE JOHNS HOPKINS UNIVERSITY
BALTIMORE 18, MARYLAND

March 12, 1958.

Dear Dr.Oppenheimer,

Many thanks for your kind letter. I am sending Miss Underwood the final draft of the letter to W.Weaver. If it does no good, it can at least do no harm. If you have any suggestion to make about the distribution list for copies of that letter, please give them to Miss Underwood (Montgomery thinks that the list should consist of Waterman, L.Cohen and MacShane).

Kolmogoroff is in Paris now; you will see him, as he stays until June. He seems to think that the cornerstone for all cultural exchange between the USA and the USSR is now the recent official agreement, and that therefore all invitations from the USA to Russian scientists should either be channelled through the National Academy, or at least that copies should be sent to the Academy with some arrangement for the invitations to be incorporated into the overall program for exchanges. I fear this will bureaucratize the whole thing too much. Nevertheless, it would be of definite interest if you could get a copy of the official agreement, and also get information from the National Academy about their views and intentions concerning the machinery to be set up. With this on hand, we could then discuss the matter again with Kolmogoroff when you come to Paris. For mathematics, I am thinking primarily of Shafarevitch and Ladijenskaja (who, incidentally, are the two Russian candidates for the Fields medal; but I was thinking of them long before I knew this).

With best regards

Very sincerely yours

A.Weil

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From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

THE INSTITUTE FOR ADVANCED STUDY
PRINCETON, NEW JERSEY

OFFICE OF THE DIRECTOR

28 February 1958

Dear Professor Weil:

Thank you for your good note, and for letting me have copies of the draft letter to Weaver. The letter is fine, and has caused us much pleasure. Of course, you know that in all its main points I agree; and if I have some misgivings about more or less administrative matters--the power, for instance, of the National Science Foundation to cause the armed services to desist from supporting pure mathematics--these seem quite minor, and there is in any case no reason why they should be reflected in what you write.

I rather hope that you will send the letter to Weaver. I am sure that he will understand that it is your letter, and not an expression of consensus of the Institute Faculty or anybody else. I write this despite the fact that, as you will have seen, or will shortly see, from the records of the Chicago meeting, there appear to be no present plans for new institutes for advanced study in mathematics, at Chicago or anywhere else. This was also confirmed for me by Whitney. But such, and other, follies are recurrent. It can do nothing but good for Weaver to have your letter to remember. In addition, I would not wish to deprive him of the pleasure.

I have asked Morgan to write to you about gas for your new house. The company has no incentive to contribute to the construction of a line, since there will only be a handful of houses on our land, and beyond lie the twin deserts of Institute farm land and Battlefield Park. I am quite clear that if you want very much to have gas, you should plan to have it bottled. As of today, the Whitneys will also be your neighbors.

Kitty was pleased with the message from your wife. If the silences from Lévy mean anything, we may indeed need help.

With all greetings,

Robert Oppenheimer

Professor André Weil
3 rue Auguste-Comte
Paris 6
France

Director's Office: Faculty Files: Box 36: Weil, André- Member, Professor and Emeritus 1957-1959
From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

For. Weil

MAX PHILIPPSON
REAL ESTATE CONSULTANT
1270 AVENUE OF THE AMERICAS
(ROCKEFELLER CENTER)
NEW YORK 20, N.Y.
—
JUDSON 6-0478

February 28, 1958

Institute For Advanced Study
Princeton, New Jersey

Dear Sirs:

Please forward the enclosed letter and the attached material by air mail to Dr. Andre Weil in Paris.

Thank you.

Sincerely yours,

Max Philippson

Max Philippson

MP:nb

Done 3/10/58

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From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

Verna:

This letter will not be mailed until we know whether Dr. Oppenheimer approves of its contents.

Caroline

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From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

THE INSTITUTE FOR ADVANCED STUDY

PRINCETON, NEW JERSEY

cc: √Dr. Oppenheimer

February 28, 1958

Professor André Weil
3 rue Auguste-Comte
Paris 6e, France

Dear André:

Caroline has sent you a copy of the report which came out of the Chicago conference. This report shows that any definite idea of an institute for advanced study at Chicago has been postponed as far as we can see. The principal innovation agreed upon at Chicago, to be recommended to the National Science Foundation, is the institution of block grants. That is grants for sizable sums of money in which one does not indicate in advance the specific use of the money but rather the general principles which will govern the use of the money. In our case the principal evidence will probably be the way we have used money in the past. At the present time we have one contract for next year with the National Science Foundation in which we originally had a sum of $18,000 to spend. We have applied for a similar contract with a total of $24,000. We have specified three of the men under the new contract.

I do not believe, at the present time, there is anything for the School of Mathematics to do. The question will certainly arise, if and when block grants are approved, should the Institute as a whole apply for a sum to be used for physics and mathematics jointly.

This means that the letter which you have written to Warren Weaver, in those matters which concern an institute for advanced study at Chicago, is unnecessary. There is one point which you raise in your letter which I think it would be unwise for the Institute to support. That is that Princeton University and the Institute present a joint front in their requests for large grants. The University is far from having one mind on the matter of contracts. There is the Tucker contingent interested in the theory of games, the Tukey group which I understand gets about $100,000 a year for various forms of applied mathematics of engineering type, the Feller group with Feller's interests, and finally there is the Spencer-Kodaira group. I do not believe it would be possible for the University itself to present one front, and even more I believe it would be impossible for us to present one front with the University. There are also the administrative difficulties arising from the fact that we cannot mix tax-free grants with salaries under contracts. The University has no such problem and moreover the University demands overhead of the order of twice what we have received on most of its contracts.

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There may be other matters in your letter concerning which you may wish to write to Warren Weaver in the future. But as I see it there is no present urgency, and it might be well for us to talk the general problem over before any representation is made to members of the Board of the National Science Foundation.

With best wishes to you and your family, I am

Very truly yours,

Marston Morse

MMcdu

COPY

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Fac Weil

AMERICAN JOURNAL OF MATHEMATICS
THE JOHNS HOPKINS UNIVERSITY
BALTIMORE 18, MARYLAND

Tuesday.

Dear Dr. Oppenheimer,

I enclose a first draft of a letter to the American Mathematical Society, concerning the American Journal, and should very much welcome any comments and criticisms which you might care to make. I am going to New York to-morrow, but shall be back ~~on~~ in the evening ~~Thursday~~ (I go back to France on Sunday).

I also return Bhartrhari, with many thanks. I hope that some day we shall read it together. I also enclose my obituary on H. Weyl (Chevalley wrote the part on continuous groups).

You will find in your mail the Chicago proposal, and my draft letter to Kimpton. Perhaps I may not send the latter after all; those people seem to have sunk to such a level of vulgarity (including many fine points which I alone can appreciate) that it is impossible to reach them there. But I wonder very much whether it is advisable that Whitney should go to their meeting next week.

On a more practical level: I hope Morgan will talk to you about the gas situation for the Maxwell Lane lots. Strömgren is also much interested.

Best regards

A Weil

Director's Office: Faculty Files: Box 36: Weil, André- Member, Professor and Emeritus 1957-1959
From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

Fac Weil

4 November 1957

Dear Dr. Weil:

Thank you for your note of October 31st. It is completely appropriate for you to tell the French authorities what your salary at the Institute will be. We have sometimes had to provide such information for professors in connection with their citizenship and visa problems in this country.

It may interest you that the physicists here have rather definitely decided to invite Bogoliubov and Landau for as much of the next academic year as they could come. We have no illusions as to the bright prospects of an acceptance; but we believe that no harm can come from our effort, and that it may, sooner or later, contribute to closer relations. Your colleagues in mathematics have also been discussing the problem, but have not, I believe, yet reached a firm decision.

It will be a very great pleasure to welcome you here next autumn.

Most cordially,

Robert Oppenheimer

Dr. André Weil
3 rue Auguste Comte
Paris V
France

Director's Office: Faculty Files: Box 36: Weil, André- Member, Professor and Emeritus 1957-1959
From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

CROSS REFERENCE

FILE: FACULTY----Weil, January, 1958

RE: Proposal to establisha center for advanced study in mathematics at the University of Chicago

LETTER DATED: correspondence dated January 15, 1958

SEE: INSTITUTE GENERAL----Chicago, University of

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THE UNIVERSITY OF CHICAGO
CHICAGO 37 · ILLINOIS
DEPARTMENT OF MATHEMATICS

Oct.31, 1957.

Dear Dr.Oppenheimer,

A small point has arisen, in connection with which I think it best to consult you.

As you probably know, I am still a French citizen. Moreover, I am still nominally a member of the French university system. As a consequence, I can get (for a nominal amount) the benefits of the French social security service when I am in France; and it has a few other small advantages. Of course I get no salary from the French authorities.

Now it appears (as a consequence of some new regulation) that, in order to keep this up, I am supposed to declare to the French authorities the salary I am to get from the Institute, according to the terms of my future contract. My question is: should I consider myself at liberty to do this ? Of course, in American Universities, salaries are an open secret. Still, officially, I believe that terms of appointment are supposed to be confidential. Is there any objection, on the part of the Institute, to my giving such information to the French authorities ?

Yours sincerely

A. Weil

A.Weil

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From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

Fac Weil

17 June 1957

Memorandum to the Faculty in the School of Mathematics:

Today I heard from André Weil that he will accept his appointment as Professor on July 1st, 1958, and come to the Institute during the autumn semester of that year.

He will probably teach in Chicago during the summer of 1958.

Robert Oppenheimer

Copy to: Professors Beurling
Borel
Gödel
Montgomery
Morse
Selberg
Whitney

cc. Mr. Morgan
Mrs. Barnett
Miss Underwood

Director's Office: Faculty Files: Box 36: Weil, André- Member, Professor and Emeritus 1957-1959
From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

17 June 1957

Dear Dr. Weil:

Thank you for your letter of June 12th. I am glad to have your decision to come here in the autumn of 1958. We will have a warm welcome for you.

The Institute will start making payments to your retirement fund in July of 1958. When I first wrote to you, I wrote that the Institute would contribute 5% of your salary each year to that fund; this we will, of course, do. The Institute may contribute more, since the Trustees have recently agreed to provide total retirement benefits of not less than $10,000 a year, and to make supplementary contributions when these are necessary.

I read with very great interest the notes you sent me about your conversations with a Soviet colleague. I have heard before of the major difficulty that successful scientists in Russia hold so many jobs that it is almost impossible to get protracted leave. We talked a little with Bogoliubov about the possibility of overcoming this difficulty, but we did not resolve it. I have not shown your notes at all widely, but I myself profited very much from reading them.

With all warm greetings,

Robert Oppenheimer

Dr. André Weil
3 rue Auguste-Comte
Paris 6e, France

Director's Office: Faculty Files: Box 36: Weil, André- Member, Professor and Emeritus 1957-1959
From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

THE UNIVERSITY OF CHICAGO
CHICAGO 37 · ILLINOIS
DEPARTMENT OF MATHEMATICS

June 12, 1957.

Dear Dr.Oppenheimer,

In my letter of April 29, I indicated that I was hesitating between two possible arrangements concerning the date when I should join the Institute; and you were good enough to tell me that both plans would be acceptable to you.

I have now decided to send my resignation to Chicago, to take effect on June 30, 1958, so as to be free to take up my permanent position at the Institute on July 1, 1958. This is the arrangement for which you indicated a preference, and correspondence with Chicago showed that it is also the most suitable one for all parties concerned. In all probability I shall spend the Summer quarter of 1958 in Chicago as visiting professor. In any case, I shall be in residence in Princeton during the autumn semester of that year.

Presumably there is an established administrative routine for transferring to the Institute the retirement fund which has accumulated for me in Chicago since 1947. I have no doubt at all that both institutions will know how to deal equitably with this matter; the less I shall have to do with it personally, the happier I shall be. Please let me know if and when you need my signature in connection with this or other administrative matters.

With cordial regards

Very sincerely yours

A.Weil

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From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

Fac Weil

FOR RELEASE 9 P.M., THURSDAY, MAY 30, 1957.

The Institute for Advanced Study announces the appointment of four new members to its Faculty: Dr. Armand Borel, Professor of Mathematics at the Federal Institute of Technology of Zürich; Dr. Bengt Strömgren, Director of the Yerkes, McDonald and Royal Copenhagen Observatories, and Sewell Avery Distinguished Service Professor at the University of Chicago; and Dr. André Weil, Professor of Mathematics at the University of Chicago, have been named Professors in the School of Mathematics. Dr. Millard Meiss, Professor of Fine Arts in the William Hayes Fogg Art Museum at Harvard University, has been named Professor in the School of Historical Studies.

The Institute for Advanced Study
Princeton, New Jersey

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From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

Weil

29 May 1957

Dear Professor Weil:

It is with pleasure that I can write to you that the Trustees of the Institute have agreed to advance the age of retirement for members of the Faculty to the June 30th following their 70th birthday.

When you are next in Princeton, I should be glad to discuss with you the financial provisions for retirement, which may prove slightly more generous and flexible than those described by me in my letter of appointment.

Very sincerely,

Robert Oppenheimer

Professor André Weil
3 rue Auguste-Comte
Paris 6e
France

Director's Office: Faculty Files: Box 36: Weil, André- Member, Professor and Emeritus 1957-1959
From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

Weil

27 May 1957

Dear Dr. Weil:

Your letter of May 20th arrived while Dr. Oppenheimer is again away from Princeton, this time for a brief absence only. I know how appreciative he will be of all you have written.

Sincerely yours,

(Mrs. Wilder Hobson)
Secretary to the Director

Dr. André Weil
3 rue Auguste-Comte
Paris 6e
France

Director's Office: Faculty Files: Box 36: Weil, André- Member, Professor and Emeritus 1957-1959
From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

THE UNIVERSITY OF CHICAGO
CHICAGO 37 · ILLINOIS
DEPARTMENT OF MATHEMATICS

May 20, 1957.

Dear Dr.Oppenheimer,

Best thanks for your very kind letter of May 13. Perhaps it is not inappropriate for me to say that I appreciate very much the friendly and warmly human tone of your letters, which is so different from the usual style of official correspondence.

I am happy to hear that Strömgren is also coming to the Institute. I have no objection at all against my name being included in the announcement you plan to make of your recent appointments, provided of course the announcement is of the unobtrusive kind which you mention, and provided no date is mentioned or implied for the time when my appointment will become effective. I have already informed the University of Chicago of my intention to leave them for the Institute in a not distant future. To decide between the two alternative plans which I mentioned to you in my letter of April 29 will require some further correspondence with them; this may take time, on their part quite as much as on my own; as the matter is not urgent anyway, this delay will, I hope, mean no inconvenience to you and the Institute.

I promised you a report on my contacts with Professor S. from Moscow; as to his name and personalia, I refer you to my letter of May 7. There is nothing secret in the whole matter, of course; and I am not asking you to keep it confidential, but am leaving it to your discretion how much of this may be repeated to other people. It might cause considerable embarrassment to Prof.S. if some of this came back to him, possibly in distorted form.

I first saw him in Paris; then we went together to Nancy, where both of us were giving talks; then I saw him repeatedly in Paris. We were alone together on several occasions, including several times when we were sitting in a café or restaurant. He talks excellent French, is in the habit of always talking very loudly, and obviously suffers from no inhibition about doing this in public.

In Nancy he told me (this conversation took place in a café) that the Stalin régime, in his opinion, had been necessary in order to win the war, but had later ceased to be so, and that, in his opinion, the changes that had taken place in Russia during recent years had been quite as far-reaching as the changes in Poland; if they do not appear as striking and as "revolutionary" (this is the word he used, interestingly enough), it is merely because they had been spread over a longer period. When I asked him for a concrete sign for these changes, he answered at once: "The fact that I am here".

I discussed with him at length, and in detail, the possibilities of inviting Russian mathematicians to America. I said that, in my opinion, a brief stay (a few weeks) might, in some cases, be better than nothing, but, generally speaking, was not likely to prove very useful, in view of the language difficulty, of the difficulty of adjusting oneself to a completely unfamiliar environment, etc.; that, according to usual practices in American Univer-

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sities, 3 months should be considered as the normal unit of time, and that 6 months would be better than 3.

He was quite emphatic in expressing his feeling that such exchanges should be encouraged and developed in every possible way, and that scientists should do their utmost to overcome bureaucratic obstacles on both sides. When asked whether he, personally, would wish his own students to get opportunities of going to America and would encourage them to make use of such opportunities, he emphatically said "yes". He thought that such exchanges should be reciprocal, and that it would be much easier for Russian scientists to accept invitations to America and obtain the necessary permissions if it was felt that Americans were interested in going to Russia and able to obtain permission for this.

A detailed discussion uncovered the following obstacles in the way of Russian mathematicians going to America. The first one is of a surprisingly trivial nature; he himself, although generally speaking a fairly well-informed person, was under the impression that the fingerprinting which is part of the formalities for an American visa is directed specifically against Soviet citizens; he was quite surprised when I told him that this practice, however objectionable it may be in some respects, is in no way discriminatory, and applies equally to the whole world. I suggest that, whenever an invitation is sent to a Russian, it should be specifically mentioned that the fingerprinting implies no discrimination against them and that all foreign scientists who go to America have to submit to it.

The second difficulty is serious. According to him, the vast majority of scientists in Russia have, in addition to their regular teaching, various subsidiary duties (administrative, or teaching in a military academy, etc.) from which it is exceedingly difficult to get away for more than a very brief period. He himself had come to France for one month, but, being the head of a department ("le chef d'une chaire", according to the quaint phrase he used), this was the utmost that he could do. This, he says, applies also to the younger people, as soon as they get into any kind of job. We discussed the matter in terms of specific cases; he mentioned several mathematicians who might be able to get away relatively easily, as well as others who would be quite unable to do so. I got the general impression, however, that the most promising field for invitations might be with the very young people - those who are about to complete their studies, have engaged in research long enough to make the invitation profitable, but have not yet an official position. The problem there is of course that such young men have no publications to their credit, or not enough to make it possible to judge their qualifications; in such cases, one would have to depend wholly upon the judgment of their Russian teachers. As to this, S. was very emphatic in assuring me that, if he or any of the best-known of his Moscow colleagues were consulted, they would give opinions which would be entirely free from any political bias. He expressed himself in such a way that I feel sure of his being sincere. He said, in fact, that this would be (so far as he knew) equally true in such subjects as physics and chemistry; he also said that he would not be so sure, even now,

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THE UNIVERSITY OF CHICAGO
CHICAGO 37 · ILLINOIS
DEPARTMENT OF MATHEMATICS

about the biological sciences, although the situation there had improved a great deal.

The last difficulty is the passport and visa problem. It was his feeling that the difficulty on the Russian side was not too serious any more, and that the situation was such that one can now start working on specific cases with real chances of success. In view of his position and general standing, I have little doubt that he knows what he is talking about.

In view of your recent letter, I think I shall try to arrange to go to Moscow some time during the coming year, and shall certainly explore the matter further. If I go, this will be an excellent opportunity for discussing concrete cases. I shall keep in mind the case of Bogoliubov, whom I remember well from my earlier visit to Moscow in 1935 (he and I were quite young at that time).

Very sincerely yours

A. Weil

A.Weil

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From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

Fac Weil

THE INSTITUTE FOR ADVANCED STUDY
PRINCETON, NEW JERSEY

OFFICE OF THE DIRECTOR

13 May 1957

Dear Professor Weil:

This will be in answer to your two letters of April 29th and May 7th. It should atart by repeating how very warmly pleased we are at your decision. It may be of interest to you that your colleague in Chicago, Bengt Strömgren, has just accepted a Professorship here. We have made one other appointment in the last months, Millard Meiss of Harvard. It would be natural for us to make an unobtrusive announcement of these appointments in the near future, and unless we hear that this would create problems for you, we shall probably do that.

I hope that my delay in answering your letter of April 29th will not prove troublesome. Either of the alternatives you suggest seems quite acceptable. I would prefer it if you could take up your permanent position here at the earlier date; and I feel fairly sure that those who will be working most closely wi th you would share this preference.

Professors at the Institute very often lecture during the period from early April to October, or the shorter period around the New Year, when we are not in session. Yang and Whitney are now in Paris; Dyson on his way to Berkeley; and I myself have just returned from Harvard. I think myself that these connections with colleagues and advanced students at other universities are a most healthy thing, and would wish to encourage them. No question has ever arisen about the appropriateness of being paid for such visits to other universities, or institutions, or laboratories, or seats of learning. The only rule that we try to keep is that if a man accepts an invitation which keeps him away from the Institute during a major part of our semesters, then he must be paid by those who invite him, and not by the Institute. If you were to teach in the summer of 1958 at the University of Chicago, that would in no way affect the salary the Institute would start paying you as of July 1st, 1958. It is impossible for me to judge, since I do not know the details of your arrangements at Chicago, whether this solution would be as satisfactory financially as coming here with a

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temporary membership in the fall of 1958. I am sure that whatever decision you make on the matter will be intelligible and acceptable to all concerned.

With regard to the questions you raise in your letter of May 7th, I write with equal interest, but less authority. It is true that Dyson and Pais went to Moscow from here a year ago, Dyson as a national of the United Kingdom, and Pais as a former Dutch national now a United States citizen. They went with a group of Americans who had the support and help of the National Science Foundation, an agency of the Federal Government. I think Dyson might have gone even without this reassurance. In fact, their visit to the U.S.S.R. was welcomed by many agencies of the Government.

This autumn, and especially after the struggle in Hungary, our Government reconsidered its former affirmative attitude toward scientific exchanges, and for a time discouraged them. I understand, but I have not seen the official papers, that the various departments have now agreed that such honest exchanges are indeed to be fostered on a more or less reciprocal basis. We had renewed permission to invite Soviet physicists to a congress in Rochester this April; Francis Low, a young theoretical physicist, is in Moscow now for a two-week visit; others are planning to go. Thus it seems to me that your going to Moscow would be consistent with the general views that the Government has adopted as to the desirability of such visits.

Clearly, if your visit were exploited by the Soviet Government for political purposes, or if you were quoted, or misquoted, in a way which seemed to advance those purposes, this would be a disturbing matter, and the immigration authorities might give weight to it. From all I can learn from those who have visited the U.S.S.R. in the last year or so, and from the behaviour of Soviet scientists in this country, I would have no reason to believe that such a development was at all probable; but in any case I would assume that you would know how to reduce its probability. I therefore hope that you will not be discouraged from going to Russia, if in other ways the visit looks interesting and profitable. I would think that any help that the Air Force might give you in carrying out the visit would be a further insurance against possible misgivings on the part of the immigration authorities. Perhaps they could fly you to Helsinki or Berlin.

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To come to your last point, we would be very grateful for a report on your conversations with Professor Sobolev, and for learning later of your explorations in Moscow. If you should chance to encounter Bogoliubov, who has turned his talents in analysis to problems in theoretical physics, perhaps you could explore with him whether a longer visit to this country, and specifically to the Institute, would be of interest to him, and might be possible.

With all good wishes,

Robert Oppenheimer

COPY

Professor André Weil
3 rue August-Comte
Paris 6e
France

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THE INSTITUTE FOR ADVANCED STUDY

PRINCETON, NEW JERSEY

SCHOOL OF MATHEMATICS

9/26/57

Dear Dr Oppenheimer

Here enclosed Weil's letters.

Many thanks for having let me see this.

Cordially yours

A Borel

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Fac Weil

THE UNIVERSITY OF CHICAGO
CHICAGO 37 · ILLINOIS
DEPARTMENT OF MATHEMATICS

3 rue Auguste-Comte, Paris (6e)

May 7, 1957.

Dear Dr.Oppenheimer:

The purpose of this letter is to ask for your advice in a matter which has no connection with our recent correspondence, except that it would have been more natural and proper for me to ask the advice of the University of Chicago if I had not now the prospect of joining the Faculty of the Institute in a not distant future.

Recently Prof.S.Sobolev, from Moscow, came to give a course of lectures at the Collège de France in Paris, chiefly on the invitation of our colleague Prof.Leray. Prof.Sobolev is one ~~the~~ of the best-known mathematicians working in the field of partial differential equations, and has a rather high position, I believe, in the Soviet mathematical world; he has had a Stalin prize. Incidentally, he is a party member. I had known him rather well in 1935, when I visited Moscow on the occasion of the topological conference and had much pleasure in renewing our old acquaintance. He speaks excellent French, is a definite extrovert, and I found him fairly outspoken in many matters.

He insisted very much that I should come to Moscow for a visit during the coming year, and gave me every assurance that he could arrange for this fairly easily on the Russian side. I am still a French citizen; my status in the U.S.A. is that of a permanent resident; I am now in France on a re-entry permit. The question is: is it possible that I might jeopardize my resident status in the U.S.A. by accepting an invitation to the USSR ? If I make enquiries from official quarters, they will of course tell me what the law is in this matter, viz., that no assurance can be given to me beforehand that I shall be admitted on coming back, and that the matter will lie wholly with the Immigration authorities at the time of my coming back. Such answers, of course, cannot help me. I believe that Dyson had a similar problem some time ago; you may know how he solved it, whether by just risking it, or by getting some kind of unofficial green light. I should appreciate it very much if you could let me have any information or advice which might help me in this matter. Incidentally, I had an opportunity, recently, of talking to the man in charge of the mathematics research program of the Airforce, and he assured me that they would not have the slightest objection to my making a trip to Moscow during the period of my contract with them, provided I informed them beforehand. But of course he could not speak for the Immigration authorities.

I took the occasion of my contacts with Prof.Sobolev to discuss at length the possibilities for inviting Soviet mathematicians to the U.S.A., and I got some very interesting and quite concrete information on the subject, much of which was quite new, at least to me. If you wish, I could send you a brief report on these conversations. Of course, if I went to Moscow, I should take this opportunity for exploring the matter further.

Very sincerely yours

A.Weil

Director's Office: Faculty Files: Box 36: Weil, André- Member, Professor and Emeritus 1957-1959
From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

1 May 1957

Dear Dr. Weil:

As you know, Dr. Oppenheimer is at Harvard to give the William James lectures. I have told him by telephone of your letter of April 29th, and he has asked me to let you know that he and his colleagues are all delighted by your decision. Dr. Oppenheimer will be back in about ten days and will be writing to you then about the questions you raise.

Sincerely yours,

(Mrs. Wilder Hobson)
Secretary to the Director

Dr. André Weil
3 rue Auguste-Comte
Paris (6e)
France

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Fac Weil

cc: RO- Harward
C.B Underwood

THE UNIVERSITY OF CHICAGO
CHICAGO 37 · ILLINOIS
DEPARTMENT OF MATHEMATICS
3 rue Auguste-Comte, Paris (6e)

April 29, 1957.

Dear Dr.Oppenheimer,

Many thanks for your kind letter. In the meanwhile, I have also received very kind and encouraging letters from Prof.Morse and Borel, in addition to the one from Montgomery; and I have had time to give further consideration to your offer.

I have now made up my mind to accept that offer. The only point about which I am still in doubt is when to join the Institute. As I told you, I wish to stay in Paris for the duration of my contract with the Airforce, which runs until June 30, 1958. This would leave me free to go to Princeton in the Fall of next year, with my appointment starting, as you suggest, from July 1, 1958, unless it should appear desirable for me to do another year's teaching in Chicago, after my year's leave of absence, in which case my Princeton appointment could be effective only on July 1, 1959. As to this, I have one basic question to ask, and a few remarks to make.

The basic question concerns the Institute's policy in the matter of remuneration for occasional visiting appointments, and in particular Summer teaching. Is it always permissible for a professor at the Institute to accept such invitations, provided of course his basic obligations to the Institute are not affected thereby ? Concretely, the question arises in my case in the following manner. Chicago has expressed a definite interest in my teaching there, if not for one year, at least for one quarter (i.e., the Summer quarter 1958) after my year's leave. This might be financially quite attractive for me <u>if</u> it is arranged as a visiting appointment in Chicago for the Summer quarter of 1958, on the assumption that my appointment with the Institute would have started on July 1. Would there be any objection to this on the part of the Institute ?

On the other hand, it might be found convenient, to the University of Chicago and to myself, to have my appointment there continued until June 30, 1959. Would the Institute be willing, in that case, to go back, for that year, to the arrangement which had been discussed in our correspondence of last February, viz., to have me come to Princeton, as a member, for the Fall term of 1958 (which would be my quarter off residence in Chicago for that year) ? I confess that the main incentive for me in such an arrangement would be practical and financial (it would be particularly advantageous if it is true that a member's stipend at the Institute is free from income-tax). But, with the prospect of having to buy a house in Princeton, I may be forgiven if I am anxious to strengthen my financial reserves, even at the cost of some little inconvenience to myself. At the same time, since there is no chance of a quick solution to the housing problem in Princeton, and since I shall have to leave my furniture in storage for about a year anyway, it would not make very much difference to me to spend most of that transitional period at one place rather than at the other. But I do not

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THE UNIVERSITY OF CHICAGO
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DEPARTMENT OF MATHEMATICS

know whether you will consider this scheme to be at all feasible. In any case, I should very much appreciate your advice in these matters.

I need not, I am sure, spend many words in assuring you that I greatly appreciate the honor done to me by the Institute, and that I am very much looking forward to the time when I shall join it permanently. I should be grateful if you would kindly convey the expression of these feelings to your Faculty and Board of Trustees.

Very sincerely yours

A Weil

A.Weil

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From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

THE INSTITUTE FOR ADVANCED STUDY
PRINCETON, NEW JERSEY

OFFICE OF THE DIRECTOR

20 April 1957

Dear Dr. Weil:

Thank you for your letter of April 9th. It came while I was lecturing at Harvard, and this is the first opportunity that I have had to respond. First, let me say that we have no reason to press you either as to the date at which you make your decision whether to come to the Institute, nor as to the date at which you actually take up residence here. These are matters which we would like you to settle in your own way, and in your own time. If you can be in residence for the autumn semester, it would be natural for you to assume your professorship on the July 1st of the corresponding year; if you can be in residence for a spring semester, January 1st would be the appropriate time; but these arrangements, though the simplest, are not necessary.

You are right that at the present time all professors at the Institute have the same salary and the same privileges. It has not always been so. It is my strong desire, and my intention, to keep it so in the future; but I cannot put this in the form of a promise binding the Institute forever.

The housing situation in Princeton is indeed complex. We are now building about one hundred units that should be suitable, and even rather pleasant, for temporary members. That is what they are for; they are not suitable for permanent occupancy, and are not intended for that, and can be made available to permanent members of the Institute, or to Faculty, only on an occasional or an emergency basis. I am sure, for instance, that if you wished for the first semester or year here to live in one of these units, that would be possible; but in the end you would have to find another solution.

The Institute has in the past given mortgages to Faculty members desiring to buy their own homes. The terms are generous: the mortgages cover up to 60% of the value of the property; interest is at 4%, and amortization on a relatively long term. All such arrangements are negotiated individually, but we have never in the past had any difficulty. In the second place, the Institute has recently adopted a new policy permitting the sale to a Faculty member of such homes as it has, at as little as two thirds of their appraised value. The reason is that real estate in this part of the country, and of this town, is quite expensive. We have a complex, but I believe quite just, arrangement for recapturing such homes when the professor is through with them. At the present moment, the only house we have is one given us some years back by Veblen.

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There are one or two other possibilities that may become real within the next year. Finally, the Institute has some extremely pleasant land on which houses can be built. We have in the past made this land available at well below the market price, and provided a mortgage for the man who wants to build along the lines outlined above. In exchange for this, we ask for first option to repurchase.

It seems to me unlikely that, even with this explicit discussion, the problem of finding a suitable home can be solved on paper. I do not believe that the problem of housing, formidable though it is, should in any important way affect your decision. It has never for long defeated or blunted anyone who has come here.

Four further points: The Trustees have recently extended the retirement age for the Faculty to seventy, and are developing a somewhat more generous pension policy than that outlined to you in my letter of April 4th.

It has not in the past been the policy of the Institute to pay for moving furniture; it was not done in my case, nor in the cases of more than a dozen men who have since come to join the Faculty. Recently we did have a request for such help, because of special hardship. The Faculty of the School involved was willing to divert a small part of its stipend funds for this purpose, and I was authorized by the Trustees not only in this case, but in others of genuine hardship, to approve such diversion. I do not know to what extent this meets your question.

Both Whitney and Yang will be in Paris this spring, Whitney at the Collège de France, and Yang, at least in part, at the Centre National de la Recherche Scientifique. Both, I know, would be pleased to talk with you about your future and ours.

Last year it seemed desirable to think in relatively concrete terms about the near future of this institution, and also to bring into somewhat closer common knowledge a few of the Trustees and members of the Faculty. The result is an internal report on our problems, which is not intended for wider circulation, and which we are sending you under separate cover, only in the hope that it may clarify for you some of the few points that are reasonably clear to us.

Very sincerely,

Robert Oppenheimer

Dr. André Weil
3 rue Auguste Comte
Paris 6e
France

Director's Office: Faculty Files: Box 36: Weil, André- Member, Professor and Emeritus 1957-1959
From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

THE INSTITUTE FOR ADVANCED STUDY

PRINCETON, NEW JERSEY

OFFICE OF THE DIRECTOR

April
20 ~~May~~ 1957

Dear Dr. Weil:

Thank you for your letter of April 9th. It came while I was lecturing at Harvard, and this is the first opportunity that I have had to respond. First, let me say that we have no reason to press you either as to the date at which you make your decision whether to come to the Institute, nor as to the date at which you actually take up residence here. These are matters which we would like you to settle in your own way, and in your own time. If you can be in residence for the autumn semester, it would be natural for you to assume your professorship on the July 1st of the corresponding year; if you can be in residence for a spring semester, January 1st would be the appropriate time; but these arrangements, though the simplest, are not necessary.

You are right that at the present time all professors at the Institute have the same salary and the same privileges. It has not always been so. It is my strong desire, and my intention, to keep it so in the future; but I cannot put this in the form of a promise binding the Institute forever.

The housing situation in Princeton is indeed complex. We are now building about one hundred units that should be suitable, and even rather pleasant, for temporary members. That is what they are for; they are not suitable for permanent occupancy, and are not intended for that, and can be made available to permanent members of the Institute, or to Faculty, only on an occasional or an ~~urgency~~ emergency basis. I am sure, for instance, that if you wished for the first semester or year here to live in one of these units, that would be possible; but in the end you would have to find another solution.

A The Institute has in the past given mortgages to Faculty members desiring to buy their own homes. The terms are generous: the mortgages cover up to 60% of the value of the property; interest is at 4%, and amortization on a relatively long term. All such arrangements are negotiated individually, but we have never in the past had any difficulty. In the second place, the Institute has recently adopted a new policy permitting the sale of such homes as it has, at as little as two thirds of their appraised value to a

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Faculty member.) The reason is that real estate in this part of the country, and this town, is quite expensive. We have a complex, but I believe quite just, arrangement for recapturing such homes when the professor is through with them. At the present moment, the only house we have is one given us some years back by Veblen. There are one or two other possibilities that may become real within the next year. Finally, the Institute has some extremely pleasant land on which houses can be built. We have in the past made this land available at well below the market price, and provided a mortgage for the man who wants to build along the lines outlined above. In exchange for this, we ask for first option to repurchase. It seems to me unlikely that, even with this explicit discussion, the problem of finding a suitable home can be solved on paper. I do not believe that the problem of housing, formidable though it is, should in any important way affect your decision. It has never for long defeated or blunted anyone who has come here.

Four further ~~Three final~~ points: It has not in the past been the policy of the Institute to pay for moving furniture; it was not done in my case, nor in the cases of more than a dozen men who have since come to join the Faculty. Recently we did have a request for such help, because of special hardship. The Faculty of the School involved was willing to divert a small part of its stipend funds for this purpose, and I was authorized by the Trustees not only in this case, but in others of genuine hardship, to approve such diversion. I do not know to what extent this meets your question.

Both Whitney and Yang will be in Paris this spring, Whitney at the Collège de France, and Yang, at least in part, ~~through~~ at the Centre National de la Recherche Scientifique. Both, I know, would be pleased to talk with you about your future and ours.

Last year it seemed desirable to think in relatively concrete terms about the near future of this institution, and also to bring into somewhat closer common knowledge a few of the Trustees and members of the Faculty. The result is an internal report on our problems, which is not intended for wider circulation, which we are sending you under separate cover, only in the hope that it may clarify some of the few points that are reasonably clear to us.

Very sincerely,

Robert Oppenheimer

Dr. André Weil
3 rue Auguste Comte
Paris 6e
France

a) The Trustees have recently ~~adopted~~ extended the retirement age for the Faculty to seventy, and are developing a somewhat more generous pension policy than that outlined to you in my letter of April 4.

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THE UNIVERSITY OF CHICAGO
CHICAGO 37 · ILLINOIS
DEPARTMENT OF MATHEMATICS

April 9, 1957.

Dear Dr.Oppenheimer,

The offer of a professorship at the Institute is indeed an honor, and I appreciate it all the more in view of the very kind and flattering words with which you have accompanied it. I am also very grateful to Prof.Montgomery for his letter, which came in just after yours.

There is no doubt in my mind that your offer is a very tempting one. At the same time, this is a serious decision for me to make, since it will undoubtedly affect the remainder of my scientific life; and I am sure that you do not wish me to make it hurriedly.

Your letter mentions a "descriptive booklet" about the Institute, which was to be enclosed in it. This was accidentally omitted. I doubt, however, whether there is much in it which I do not know already. It is true that for a long time, and until my recent stop in Princeton, I have had few opportunities of visiting the Institute; but, through my friends and colleagues who have been there, both in a permanent and in a temporary capacity, I have kept fairly well informed of its mode of operation and scientific activities, and have no doubt at all that it is the kind of institution where I could find congenial surroundings and full ecope for my scientific work.

The questions which arise in my mind, and an answer to which could assist me in making a final decision, are therefore of a more practical nature. In the first place, your letter suggests that the position of all professors at the Institute is the same in every respect, including salary. May I assume that this is so ? If this is a fact (and I, for one, would heartily approve of such a policy), this would of course dispense me from raising any further question in this connection. Also, I should certainly welcome some information concerning the housing problem in Princeton. During my recent visit there, I gathered that the Institute was building apartments, which promised to be very satisfactory in every respect; but, as I was not then aware that I might be interested in this on more than a short-range basis, I did not ask for any details, either concerning the general type or types of apartments to be made available to the Faculty, or concerning the rents. Now that the matter has become important to me and to my family, I should appreciate any information that you could send me on the subject. At the same time, it might conceivably become, if not necessary, at any rate convenient for me to buy a house in or around Princeton. In such a case, could I hope to get some financial assistance from the Institute to enable me to do so ?

If I decide to accept your offer, the question of timing will also arise. As you know, I have been given an Airforce contract, for the period from July 1, 1957, to June 30, 1958, during which

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THE UNIVERSITY OF CHICAGO
CHICAGO 37 · ILLINOIS
DEPARTMENT OF MATHEMATICS

period I am to stay in Europe, mostly in Paris. I should be most reluctant to give up this plan; nor do I imagine that you would wish me to do so. But of course the contract was granted to me through the agency of the University of Chicago. The matter may therefore require some administrative adjustment; and you would help me by letting me know your views about it.

There is a minor item of a practical nature, concerning the moving of my furniture (which I have left in storage in Chicago) from Chicago to Princeton, in case I accept your offer. Is it right for me to assume that in this case I should be entitled to some compensation from the Institute for such expenses ?

If further questions should arise in my mind, I shall make free to write you again, or to gather information from my friends who are now on your Faculty. I understand that Prof.Whitney is expected in Paris quite soon, and this will undoubtedly give an opportunity for further talks on the subject. But it is not my intention to procrastinate, and I expect to be able to make a decision fairly soon.

Very sincerely yours

A. Weil

A.Weil

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From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

Fac Weil

THE INSTITUTE FOR ADVANCED STUDY
PRINCETON, NEW JERSEY

OFFICE OF THE DIRECTOR

5 April 1957

Dear Dr. Weil:

It is a pleasure, and indeed a great honor, to offer you a Professorship in the School of Mathematics at the Institute for Advanced Study. In so doing, I also express the earnest desires of your colleagues on the Institute's Faculty, and the cordial hopes of its Trustees.

It is perhaps appropriate that I write to you briefly about what this appointment means, since the Institute is not in all respects like a university, and you may not know its ways completely. Some of your questions may be answered by the elementary descriptive booklet that I enclose; some I should attempt to answer in this letter. I am clear that in so doing I shall not answer all questions that may be important to you, and I would welcome an opportunity, either by further correspondence or in consultation, to tell you what you want to know.

A Professor's duties at the Institute involve, in the first instance, only the prosecution of his own work, with such collaboration with colleagues here and throughout the world as may be fruitful. Apart from this, as a member of the Faculty, we would ask you to assist in the selection of members in mathematics and related subjects, and in the consideration of all additions to the Faculty, and all appointments with long term at the Institute. Although we regard these duties as a matter of conscience, they are not in fact very arduous. They do make it desirable that the Faculty be in Princeton for a good part of our two terms, running from about October first to mid-December, and from about mid-January to early April. Where the course of a man's work or other compelling reasons make residence in Princeton inappropriate, we have found it possible to grant a leave of absence.

As a Professor at the Institute, your salary would be $18,000 a year. You would have available a fund for scholarly assistance; should you not require an assistant, the money so budgeted may be used by you as a grant for a member in whose work you are interested. Members of the Faculty have an allowance of a thousand dollars a year for travel for professional purposes; this is cumulative up to a total of $3,000. At present, the age of retirement for an active professorship--as a Professor

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Emeritus you would be a permanent member of the Institute and have continuing support for your work--is sixty-eight. At present, also, we follow the usual practice of contributing five per cent of your salary per annum to the purchase of a T.I.A.A. policy, provided you are willing to contribute an equal amount. These matters having to do with retirement and pension are now under study by the Trustees; such changes as they may make will surely not be unfavorable for members of the Faculty.

OFFICE OF THE DIRECTOR

It is my hope, and it is shared by those who have participated in this invitation, and above all by those who best know you, that you will find this Institute and this position a good base for your future work and life; and that we may help to contribute to their fulfilment.

Very sincerely,

Robert Oppenheimer

COPY

Dr. André Weil
3 Rue Auguste-Comte
Paris 6e, France

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From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

File Weil

THE INSTITUTE FOR ADVANCED STUDY
PRINCETON, NEW JERSEY

OFFICE OF THE DIRECTOR

5 April 1957

Memorandum to the Members of The Faculty:

At their meeting on April 4, 1957, the Trustees of the Institute voted:

1. To extend to the June 30th following a Professor's 70th birthday the mandatory age for retirement from the Faculty.

2. To approve the appointment of André Weil as Professor in the School of Mathematics. The letter of invitation has gone out to Professor Weil.

COPY

Robert Oppenheimer

Copy to: Professor Alföldi
Beurling
Dyson
Cherniss
Gödel
Kantorowicz
Kennan
Meritt
Montgomery
Morse
Pais
Panofsky
Selberg
Thompson
Whitney
Woodward
Yang

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From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

cc: Mr. Morgan
Mrs. Barnett
Miss Underwood

11 March 1957

Dear Dr. Weil:

It is with great pleasure that I learn of the good chances that you will be with us in the first term of the academic year 1958-59. I was indeed sorry that my commitments on the West Coast made it impossible for me to be here during your visit last week.

With every good wish,

Very sincerely,

Robert Oppenheimer

Dr. André Weil
3 Rue Auguste-Comte
Paris 6e, France

Director's Office: Faculty Files: Box 36: Weil, André- Member, Professor and Emeritus 1957-1959
From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

THE INSTITUTE FOR ADVANCED STUDY

PRINCETON, NEW JERSEY

SCHOOL OF MATHEMATICS

March 7, 1957

Dr. Robert Oppenheimer
Institute for Advanced Study

Dear Robert:

André Weil's visit here was most successful. He gave an excellent lecture and seemingly had a good time. He said to Selberg, Montgomery and me that he would like to come here for the first term of 1958. I assured him that this was much to our liking. He said he had the problem of making arrangements with the University of Chicago. By the time that 1958 comes around it is most likely that he will have received our invitation to become professor. However, I told him that I would report his decision to you and stated that without doubt you would write him expressing your gratification.

It seems to me that this would be in order so that he may proceed with his arrangements with Chicago with assurance that everything is all right.

Sincerely yours,

Marston Morse

Marston Morse

MMcdu

3 Rue Auguste-Comte
Paris 6e France

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From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

Weil

February 8, 1957

Professor Armand Borel
Department of Mathematics
Swiss Federal Institute of Technology
Zürich, Switzerland

Dear Armand,

Recently we invited Andre Weil to visit here for a term next year but he is unable to do so because he has already arranged to spend next year in France. We have now invited him to come for a term in 1958-1959. He will visit here on February 28 and give a lecture on his way to France. This correspondence led us to consider a permanent professorship for Weil beginning in 1958 and after discussion it develops that the mathematicians here and Oppenheimer are in favor of such an offer. We are therefore beginning the formal proceedings, which will take some time.

The first step is a joint meeting with the physicists followed by two meetings a couple of weeks apart of the entire faculty. The final stage would be at the meeting of the board of trustees in April. If you care to send along your views on this matter I am sure they would be gratefully received. All of this of course is confidential.

There has been no further news on your visa and in about a week I'll try some more prodding if nothing has been heard before.

Sincerely yours,

Deane Montgomery

DM:js

cc: Mrs. Hobson
Miss Underwood

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From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

THE INSTITUTE FOR ADVANCED STUDY

Weil

PRINCETON, NEW JERSEY

February 4, 1957

Dear Colleagues:

The following revision to our minutes will be made, if you approve:

"8. It was agreed that Dr. Oppenheimer should write to André Weil expressing the hope that Weil would visit the Institute when he is passing through on the way to ~~France~~ in order that the Director and the mathematicians could ~~talk with him~~ about the future. It was understood that he would be ~~invited~~ to spend either of the terms of 1958-59 at the Institute that he found convenient. Professor Morse will answer the letter to him informally in the same sense."

Sincerely yours,

M. Morse

Marston Morse

COPY

	Approve	Disapprove
Professors ~~Beurling~~	___	___
Gödel	___	___
Montgomery	___	___
✓ Oppenheimer	___	___
Selberg	___	___
Whitney	___	___

Please return to Miss Underwood.

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From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

/Caroline: made a copy for Morse & Montgomery. have been informed V.H.

THE UNIVERSITY OF CHICAGO
CHICAGO 37 · ILLINOIS
DEPARTMENT OF MATHEMATICS

Feb.4, 1957.

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

Dear Dr.Oppenheimer:

Many thanks for your kind letter of Feb.1. Actually, I shall be sailing for France, not in the Summer as you seem to have assumed, but on March 2. It would be quite an easy matter for me to stop in Princeton for a day - say, on Feb.28 - and, if you wish, give a talk at the Institute. I have to be in New York in the morning of March 1; but I could easily arrange to leave here on Feb.27.

Very sincerely yours

A. Weil

A.Weil

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From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

THE INSTITUTE FOR ADVANCED STUDY
PRINCETON, NEW JERSEY

SCHOOL OF MATHEMATICS

March 11, 1957

Dr. Robert Oppenheimer
Institute for Advanced Study

Dear Robert:

A letter from Chern was obtained by me without authorization from the School because I was curious to hear an explanation of some of the reports from Chern.

The principle which I think should be followed in this matter of gathering letters as credentials is that those letters should be put into the credential summary which have been authorized by the School as a whole. If one admitted every letter which every individual might write, one would get a very distorted conception of the candidate. Such a distortion would have been obtained if I had written, for example, to Leray or to Chandrasekharan.

Very truly yours,

Marston Morse

Marston Morse

MMcdu
Enclosure

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From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

THE INSTITUTE FOR ADVANCED STUDY

PRINCETON, NEW JERSEY

OFFICE OF THE DIRECTOR

28 March 1957

Memorandum to the Board of Trustees of the Institute:

At a meeting of the School of Mathematics on March 11th, the pure mathematicians proposed the election of André Weil to a professorship in the School of Mathematics. After detailed discussion, this nomination was unanimously forwarded by those attending the meeting to the Faculty of the Institute. It was presented to the Faculty at their meeting of March 12th, 1957, at which time there was extended discussion of Weil's mathematical achievements, his cultural interests, some episodes in his history, and his qualities as a man. The matter lay over with the Faculty until its meeting of March 28th, 1957, when the nomination was approved without dissent by the Faculty. I am therefore transmitting it, and the accompanying documents, to you for consideration at our meeting of April 4th. If you are able to reach a conclusion at that time, there would be some advantages in not postponing action until the autumn.

I shall want to discuss this appointment with you at the time of our meeting, since there are some aspects of it that have been fully considered by the Faculty, but are not adequately reflected in the papers that I am transmitting. We have included André Weil's article "Science Française?" in order to give you an impression of Weil's style when he is writing on lay matters.

Robert Oppenheimer

Robert Oppenheimer

attachments

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From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

cc: Dr. Oppenheimer

THE UNIVERSITY OF CHICAGO
Chicago 37 . Illinois
Department of Mathematics

March 1, 1957

Professor Marston Morse
Institute for Advanced Study
Princeton, N. J.

Dear Professor Morse:

I am deeply appreciative in your confidence in me in asking my opinion about Andre Weil, now under your consideration for a professorship at the Institute. As you would perhaps agree, he should be considered as one of the greatest living mathematicians. In regard to interest and achievements in a large variety of mathematical fields, he is perhaps surpassed by none. Being a close friend of his, I have always found him mathematically very stimulating.

Weil has the reputation of being hard to get along with. I do not know whether it is partly a misinterpretation of his uncompromising attitude with regard to quality and significance of mathematical work. He has definite opinions about mathematics, to which I am in general agreement. He does have the habit to express his opinions openly, and sometimes was not able to avoid being personal.

Weil is a good organizer. I think he was responsible more than anybody else for the success of the Bourbaki group. If he joins the Institute, he most likely will have ideas about its activities. On practical matters he will be insistent, but my experience has shown him to be generally reasonable.

I will certainly hate to lose him as a colleagues. My disappointment will be more than balanced, if he finds himself happier at the Institute and if his talents will be better utilized with you. With best regards, also to Deane,

Sincerely yours

/s/ S. S. Chern

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C O P Y C O P Y

ECOLE POLYTECHNIQUE FÉDÉRALE
Mathématiques Supérieures

Prof. Dr. Armand Borel

Zurich, le March 1st 1957

On André Weil.

André Weil's papers are distinguished by their depth and their universality, pertaining notably to number theory, algebraic geometry, algebraic topology, differential geometry, Lie groups, function theory of several complex variables. Many of them are important not only by the results they bring but also by their influence on the subsequent development of the subjects they concern.

But, as important as they are, these papers do not give a full account of the importance of Weil's role in mathematics. His exceptionally wide knowledge of mathematics and strong insight have often allowed him to point out deep connections and make suggestions which have led several mathematicians to very fruitful lines of research; being one of the rare persons who can see mathematics as a whole, he has contributed significantly to the trend towards unification which has been so strongly felt these last years, and this as well by his papers as by his suggestions or by the work of the group Bourbaki, where he has been one of the main influences.

To summarize, A. Weil is an outstanding mathematician both as a researcher and as a leader; this, in my opinion, makes him particularly well qualified to be a Professor at the Institute. I hope very much that he will be offered such a position and will accept it.

/s/ A. Borel

Director's Office: Faculty Files: Box 36: Weil, André- Member, Professor and Emeritus 1957-1959
From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

March 6, 1957

TO: Professors Beurling, Dyson, Gödel, Morse, Oppenheimer, Pais, Selberg, Whitney, and Yang.

FROM: Deane Montgomery

In the copy of Brauer's letter to me dated March first there is a misprint. The last word in the third sentence should read "century" not "country".

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From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

C O P Y C O P Y

HARVARD UNIVERSITY
Cambridge 38, Mass.

Department of
Mathematics

March 1, 1957

Professor Deane Montgomery
The Institute for Advanced Study
Princeton, New Jersey

Dear Montgomery:

André Weil is one of the most eminent mathematicians of our time. I do not usually like to use the word brilliant, but I feel that it applies to him. Weil's work ranks amongst the most fundamental and important done in this country. He is one of the extremely few persons who is a mathematician and not a specialist in one of the fields of mathematics. As a Professor at the Institute, he could have a tremendous influence on the development and growth of mathematics. I feel that Weil is the man for the Institute and that the Institute is the place for Weil.

Sincerely yours,

/s/ Richard

Richard Brauer
Professor of Mathematics

RB:mh

Director's Office: Faculty Files: Box 36: Weil, André- Member, Professor and Emeritus 1957-1959
From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

C O P Y C O P Y

COLLÈGE

de Paris, le Feb. 27, 1957

FRANCE

Chaire
D'Algèbre et Géométrie

Dear Professor Montgomery,

I am very glad to hear that you and your colleagues are wishing to have the Institute offer a professorship to André WEIL.

I think this is an excellent choice, the best you could make. Like most of the mathematicians of my generation (and of the preceding one too), I have the greatest admiration for the scientific work of André WEIL; I even believe that, among the living mathematicians (in all branches of mathematics), he has no superior, if any equal. No other seems to me to have the same global (and yet so deep) insight of mathematics, the same rare combination of creative imagination and of "brute force". It would be useless to try to list here his main contributions. Let me quote only his thesis, his book on locally compact groups, the "Foundations", his papers on class field theory, on complex multiplication, his proof of the Artin-Riemann conjecture for function fields of one variable. These are all contributions of fundamental importance, which have opened up new and very promising fields of research; to give a personal example, I know nothing in mathematics more exciting than WEIL's results and conjectures connecting the zêta function of algebraic varieties with topological properties of these varieties.

It would also be good to quote other contributions of WEIL (to topology, functions of several complex variables, etc), to say something about his influence on the mathematical schools of several countries (Japan, and also the U.S. and France), but you know all that better than I do. I hope what I have said is enough to express my admiration for him; he certainly is the best successor the Institute could give to C.L.Siegel and H.Weyl.

With best regards

/s/ J-P. Serre

Jean-Pierre SERRE
Professor at the Collège de France
39, Boulevard de la Chapelle
PARIS, 10.

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From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

COPY COPY

March 6, 1957

Dear Deane:

I am very enthusiastic about the idea of appointing André Weil to a position at the Institute, and I cannot think of a more appropriate choice. Like most mathematicians of my generation, I have a great admiration for Weil's work and for his masterful command of the main streams of mathematical thought. I believe that there is almost unanimous agreement among the mathematicians of today that Weil's work has had a profound influence on the growth of at least all of the following: the theory of topological groups, algebraic number theory, and algebraic geometry.

It should perhaps be pointed out also that the inspiration and training of many of the younger mathematicians in France can be traced back to Weil, via Bourbaki. In particular, it was chiefly Weil (during the early 1930's) who directed the attention of his colleagues in France to modern algebra, and who thus primed the development of the algebraic methods that have been so spectacularly successful in almost all branches of modern mathematics.

Although of at most secondary importance, it may be of interest that Weil's intellectual and cultural interests range also outside the field of mathematics. For instance, he has considerable facility with classical and ancient languages, such as Latin, Greek and Sanskrit. In part, this is in evidence in his work on the history of mathematics.

There can be no doubt that Weil would have a profoundly constructive influence on the mathematical life at the Institute, and that he would greatly contribute toward enhancing the position of the School of Mathematics of the Institute as one of the chief focal points of mathematical research.

Sincerely yours,

/s/ Gerhard

G. Hochschild
(Professor of Mathematics at the University of Illinois. Currently a Guggenheim fellow and member of the Institute.)

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From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

THE INSTITUTE FOR ADVANCED STUDY
PRINCETON, NEW JERSEY

SCHOOL OF MATHEMATICS

March 6, 1957

Dear Professor Oppenheimer:

I am sorry that I shall not be able to attend either the March 11 or the March 12 meeting.

If the matter comes to a vote on March 11, I wish to vote for placing before the full faculty a proposed nomination of André Weil to a Professorship at the Institute.

Yours sincerely,

Frank

C. N. Yang

CNY:esg

Professor Robert Oppenheimer
Institute for Advanced Study

Director's Office: Faculty Files: Box 36: Weil, André- Member, Professor and Emeritus 1957-1959
From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

C O P Y C O P Y

COLLÈGE
de
FRANCE

Chaire
D'Algèbre et Géométrie

Paris, le 27 Février 57

Cher Montgomery,

Bravo! Je suis enchanté, c'est la meilleure chose que l'Institute pouvait faire. Je souhaite très vivement que vous réussissiez auprès de vos collègues non mathématiciens et des "trustees".

Je vous envoie ci-joint une lettre plus "formelle", disant la même chose.

Bien à vous, et encore une fois bravo!

/s/ J-P. Serre

J-P.Serre

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From the Shelby White and Leon Levy Archives Center, Institute for Advanced Study, Princeton, NJ, USA

Inst Pen Univ. of Chicago
T/R Fac Weil

cc: Beurling
Borel
Gödel
Montgomery
Morse
Oppenheimer
Selberg
Veblen
Weil
Whitney

Proposed draft of a letter from André Weil to Chancellor Kimpton:

Dear Chancellor Kimpton:

I have before me a "Proposal for the establishment of a Center for Advanced Study in Mathematics" from the Department of Mathematics of the University of Chicago.

On page 5, I note the following: "This lack caused it [viz., the Institute for Advanced Study] to lose the distinguished services of Carl Ludwig Siegel a few years ago". This is the mildest of a number of uncomplimentary references to the Institute for Advanced Study contained in that proposal.

Name-calling is fun. But my Chicago colleagues might find it more profitable to inquire what caused the University of Chicago to lose the undistinguished services of one André Weil, whom they are, even now, frantically trying to replace. Should they tell you that it was the physical environment in Chicago, they would purposely be deceiving you. They know better.

Yours sincerely,

A. Weil

cc: M. H. Stone

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PROPOSAL

FOR THE ESTABLISHMENT OF A

CENTER FOR ADVANCED STUDY IN MATHEMATICS

Department of Mathematics
University of Chicago
January 15, 1958

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PROPOSAL FOR THE ESTABLISHMENT OF A CENTER FOR ADVANCED STUDY IN MATHEMATICS

1. The need.

Our inability to produce an adequate supply of high school graduates with a strong interest in science and proficiency in elementary mathematics is, quite properly, a matter of grave concern to the nation. It appears to be less aware of a serious weakness at the opposite end of the mathematics training program. We are ineffective in converting Ph.D.'s in Mathematics into research scholars of high quality and this also merits national attention.

We are not getting the full benefit of the talents and training of our Ph.D.'s. This was recognized at least 30 years ago when the National Research Council Fellowships (Rockefeller Foundation supported) were established to provide opportunities for further study and research for a small number of Ph.D.'s. The problem is much more difficult today. Mathematics has advanced so much, and grown so in complexity, that postdoctoral training is essential.

About 250 Ph.D.'s in Mathematics are being trained each year in American universities, and the number is increasing rapidly. These are talented people, frequently high school and college valedictorians, who have completed successfully the very difficult Ph.D. course of study which includes the writing of a dissertation embodying the results of original research. More than 10 °/o of these are able to obtain support for a year or two through National Science Foundation Fellowships, Office of Naval Research Associateships, stipends offered by the Institute for Advanced Study, and research associateships supported by government contracts at universities. Some of these and some others are able, after several years of teaching, to obtain

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sabbatical leave support of one kind or another for an additional year of advanced study. These people would all increase their capabilities for research if great mathematical centers existed to which they could go for advanced study and for contacts with the leaders in various fields of mathematics. Do such centers exist, and what are their facilities?

In 1933 there occurred an event of great significance for the development of capable research mathematicians. The Institute for Advanced Study began its operation with a stellar faculty consisting of James W. Alexander, Albert Einstein, Oswald Veblen, John von Neumann, and Hermann Weyl. The first group of visiting scientists numbered 21, and most of these were mathematicians who had already demonstrated ability in research. The faculty grew, and the Institute expanded into other fields. In 1957 there are 9 faculty members in mathematics and nearly 100 visitors, of whom more than 50 are mathematicians. The Institute still provides a first class faculty and is now able to provide excellent housing, and somewhat crowded office space, for about 100 people in several disciplines.

The United States also possesses several first class university faculties in mathematics. However, the Institute for Advanced Study is the only institution which can attract the mathematician who is able to spend a year of postdoctoral advanced study. It is the only institution whose faculty is not burdened with teaching in bachelor's, master's, and Ph.D. training programs, and the only place where housing and office space for visitors is readily accessible.

As a result the Institute is overcrowded, and its operation is less effective than in 1933. Its visitors do not have ready communication with its faculty, and many find a year at the Institute far less rewarding than

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expected. The nation needs more such institutions, and their establishment is a vital necessity. The manpower resources of the United States are certainly adequate to staff at least two more such centers, and the distribution of the Ph.D. training institutions in this country is such that it would appear to be wise to establish at least one such center in the Middle West, and one on the West Coast. The Ph.D.'s in these areas are less able to win fellowship awards than those from the Ivy League schools, and the support of their postdoctoral training, which could be provided by stipends from centers, is essential for the greater good of the country. In this connection let us observe the great plans of the U.S.S.R. for the establishment of centers, as was announced in the Izvestia Akademii Nauk of September - October 1957 (volume 25, no. 5) in an article entitled Forty Years of Soviet Mathematics. A translation follows:

"The concentration, which exists up to the present, of mathematical scientific work in a small number of centers primarily in the European part of the U.S.S.R., will be replaced in the near future by a dispersal of mathematical science throughout the (Soviet) Union.

'What is coming is a change of approximately the same magnitude as that which occurred in connection with the creation of new industries in the East and in Siberia. At that time the industrial revolution opened wide perspectives for the industrialization of our country, giving it new inexhaustible reserves.

'A net of universities, where new mathematicians will be trained, should extend to the most remote corners of our Union. A net of new computing centers and mathematicial institutes must be created, able to move ahead mathematics and all of its possible applications, and ready

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for that entirely new role which Science will play in a communist society.

"We are on the threshold of great changes in our country. These achievements are being backed by the entire Soviet nation, among which Soviet scientists and, in particular, Soviet mathematicians, will, beyond a doubt, play a leading role."

2. Function and operation.

We propose the establishment of one Center for Advanced Study at the University of Chicago, rather than the establishment of a new Center divorced from a university. The reason for this will appear in the following discussion of the functions and proposed operation of a Center at the University.

A Center for Advanced Study must have the following:

(a) A faculty of high distinction in research, and capable of communicating an understanding of the problems and techniques in their fields of research to the visitors who will come to the Center for postdoctoral training, and for the catalytic action which occurs in an exciting research environment.

(b) A faculty not so burdened with teaching and administrative duties as to be unable to spend time with visitors, to discuss their research programs, and to provide research stimulation.

(c) An excellent library and individual offices for visiting mathematicians. The private office is the laboratory for the research mathematician. The library provides some of his research material, and must provide an almost complete coverage of known results in mathematics. It must be readily accessible to his office, and must not be too crowded to be usable. The building housing the library and the offices for faculty and visitors must contain conference rooms and lecture rooms.

(d) Housing for visitors must be readily available. Adequate housing

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should be reserved for prospective visitors. One cannot expect the mathematician to come to an institution located in an area where he can expect to have great difficulty in finding adequate housing for himself and his family.

(e) A faculty with adequate salaries. It is not possible to create a Center without providing a salary scale comparable to that of the Institute for Advanced Study. An underpaid faculty is an unstable faculty which is subject to the upsetting influence of outside offers in this era where the number of distinguished mathematicians is far less than the number needed. The Institute has paid an annual salary of $18,000 to its professors for many years, and an annual salary of $18,000 to $20,000, for professors at centers, is surely warranted.

The main function of a new center will be that of assembling and supporting an outstanding faculty able to provide mathematical research of high quality, and to provide postdoctoral training and stimulation for visiting mathematicians. The faculty must have staunch support through adequate salaries, excellent library facilities, good office space for faculty and visitors, and through an assignment of adequate housing for the visitors. Visitors must be provided with stipends, and the research faculty should have assistants.

The description above is essentially that of the Institute for Advanced Study except that the number of visitors there has grown so large that communication between its faculty and its visitors has become more difficult than it was 20 years ago. However, the Institute lacks an asset and this lack caused it to lose the distinguished services of Carl Ludwig Siegel a few years ago.

A center needs participation in advanced graduate work. The active

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research mathematician needs the stimulation of contact, through giving a course, with bright young graduate students. He needs an audience, and the graduate class is an audience which will require him to organize his knowledge and thus understand his subject better. This audience is missing at the Institute for Advanced Study and is a vitally important advantage of the Center for Advanced Study at a university.

A center can then exist in its best form only in a university, and its faculty should inevitably be bound closely to the work of a department. While active participation in the departmental programs leading to bachelor's and master's degrees must not exist to an extent which would cripple the main function of the center, voluntary participation in these programs must be possible. The mathematicians of the center will even wish to participate occasionally in the teaching of college freshmen and in the growing programs of high school teacher training. While the emphasis in the duties of the faculty member of the center must be on research and postdoctoral training, he should be an active participant in training for the doctorate and should have the opportunity for occasional participation in beginning graduate and undergraduate teaching. Only this full opportunity, available only at a leading university, can provide for the proper expression of the talents of a group of leading research mathematicians.

3. The proposed center at Chicago.

Since its inception, the Department of Mathematics at the University of Chicago has been one of the great departments of mathematics in this country. It has a long fine tradition as a department with a superb faculty, a large and excellent body of students of mathematics, a first class library, and a fine office and classroom building. It has always been one of the major centers for the training of Ph.D.'s. Its Midwest geographic location is ideal,

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as the many excellent private and state universities in the area produce Ph.D.'s in Mathematics who are natural candidates for postdoctoral training at the proposed Chicago center. It cannot now function as a Center, even though it has a faculty of suitable stature, because it cannot offer office space, housing or stipends. Its faculty is burdened with teaching and administrative work and many outside science administration activities, and consequently is unable to devote enough time to the small number of visitors who do come on government contracts, as visiting lecturers, or as fellows. Thus the postdoctoral training capabilities of this great faculty are essentially wasted.

The faculty would like to experiment with a Ph.D. program providing less formal instruction and more intimate contact with graduate students through seminars and consultation. It has already constructed an excellent program of study providing a broad education in mathematics and leading to the degree of Master of Science. This program could be staffed by younger mathematicians and would be kept alive and even improved if a less burdened top level faculty were able to supervise it, to have time to study it, and to participate in it from time to time.

In order to establish a Center for Advanced Study at Chicago a two-pronged expansion of the present Department is necessary. The present faculty consists of ten active research men with tenure appointments, one very learned man on tenure formerly active in research who now acts as Departmental Secretary, one assistant professor, and three instructors. There are also two visiting lecturers. The Department is strong in algebra, topology and geometry, and analysis. It is weak in the important related areas of algebraic geometry, algebraic function theory and algebraic number theory, and needs to add strength in these areas. The Department also lacks strength

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in areas of mathematics close to applications. It is estimated that the addition of four or five professors in suitably selected fields would give the group the coverage the proposed Center should have.

In addition to the staff of 14 to 15 professors, provided by the first prong of the expansion proposed, the present Department needs five additional able young men at the instructor or assistant professor level. These men would take over most of the teaching in the master's program and so would relieve the professors in the Center of this work and release them for the postdoctoral training activity of the Center.

The Center should have its own budget. At least at the beginning the desirable close cooperation between the Department and the Center should be achieved by making the Chairman of the Department the Director of the Center. The following budget for the Center is proposed (with the assumption that the Departmental budget is to be maintained at at least its present level).

a. Estimated faculty salaries, $150,000 to $200,000

b. Stipends to provide one-half to full salary for 18 persons:

8 at	$ 6,000 =	$48,000
3 at	7,000 =	21,000
3 at	8,000 =	24,000
2 at	9,000 =	18,000
2 at	10,000 =	20,000
	Total for stipends	$131,000

c. Operating expenses for secretaries, telephones, equipment and supplies $ 29,000

TOTAL ESTIMATED ANNUAL BUDGET $310,000 to $360,000

In addition to this annual fund requirement the Center must be able to offer visitors office space and housing. If most of the space in Eckhart Hall were made available to the Department and the Center the essential needs of both would be satisfied. However, the Department Library, an essential research facility, is currently being used by undergraduates as a study hall.

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It would be difficult and undesirable to prevent this use. Eckhart Hall currently houses the Department of Statistics, the College and a part of the Department of Physics, and these people have nowhere else to go. Thus our ultimate goal should include the construction of a new building to house the Department of Mathematics, the Departmental Library, and the new Center for Advanced Study.

The Center must control the assignment of housing for visitors. It is proposed that the number of visitors be limited to 30, and so an ultimate total of 30 housing units is required.

The essential requirements for the establishment of a Center for Advanced Study in Mathematics at the University of Chicago have now been summarized. It is hoped that the national need for such centers will be recognized and that the support needed for the establishment of such a center at the University of Chicago will become available.

Proposal prepared by A. Adrian Albert
with advice from members of the Department,
in particular, from Saunders Mac Lane
and M. H. Stone.

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Report on André Weil

André Weil was born in Paris in 1906. There he also studied mathematics and got his doctor's degree in 1928. He then held teaching positions as follows: Professor at Aligarh University, India, 1930-32; Maitre de Conférence, University of Marseille, 1932-33; Maitre de Conférence, University of Strasbourg, 1933-40; Lecturer at Haverford and Swarthmore Colleges during the war years; Professor at the University of São Paulo, Brazil, 1945-47; and Professor at the University of Chicago since 1947. He was at the Institute for Advanced Study from January to May 1937.

Surveying the world of mathematics and mathematicians today, it is extremely rare to find someone who masters with real knowledge and insight more than one or two of the main fields of the subject. A person really able to look upon mathematics as a whole with comprehensive knowledge and deep understanding of all these major fields is again a quite singular occurrence. André Weil in this respect occupies a rather unique position among his contemporaries, and could only be compared with Hermann Weyl in his thorough and wide knowledge and deep insight in contemporary as well as classical mathematics.

His scientific production of papers and books deals with such a great variety of subjects as number theory and algebra, algebraic geometry, topological groups, analytic functions in several variables, differential geometry and even differential equations, and shows him to possess great power of penetration into the most difficult problems, ability to see

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connections between apparently distant parts of mathematics, and a high degree of originality in devising methods and techniques. Among his contributions are some that will undoubtedly always be rated among the finest achievements of mathematics in our time. His papers and books are written with a strong sense for the essential and fundamental and not least for mathematical elegance and beauty.

A few of the most spectacular results of Weil will be mentioned below; these are selected also because they lie closest to the present writer, in that the results, though mostly obtained by methods of algebraic geometry, have deep consequences for number theory.

Weil's thesis in 1928 at once established him as a mathematician of first rank. This paper (which by the way had the unusual distinction of being accorded a review of six pages in the Jahrbuch über die Fortschritte der Mathematik), dealt with the problem of the rational points on algebraic curves, or phrased differently it dealt with the properties of the set of rational solutions of an equation of the form $P(x, y) = 0$, where P is any polynomial in x and y with rational coefficients. Special problems of this category had been considered in mathematical literature far back, but the first results with some claim of generality were very recent. Bypassing the history for the simplest case when the curve has genus zero, we mention here only that H. Poincaré had attacked this problem for curves of genus one, his attempt although it failed, led him to certain conjectures about the structure of the set of rational points, which were later proved

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The results of Weil's thesis formed the essential basis for Siegel's result about the integral points on algebraic curves, which in a certain sense closed the history of diophantine equations with two unknowns.

A probably even more outstanding achievement was Weil's proof in 1941 of the so-called Riemann hypothesis for the zeta-functions of function fields over a finite constant field. These zeta-functions had first been introduced by Artin in the early twenties as analogues of the classical zeta-functions of algebraic number fields, and certain properties like that of the existence of a functional equation had then, by various authors in the late twenties and the thirties, been established for these new functions. The fundamental questions about the location of the zeros, which are still unresolved for the classical zeta-functions, were, after some very special results had been obtained by Mordell, Davenport and Hasse,

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first attacked in a somewhat general situation by H. Hasse who in 1934 was able to give a proof of the Riemann hypothesis for the case of an elliptic function field (or as one would rather say today for the zeta-function of a curve of genus one over a finite field). This result at the time was major progress.

Weil had the idea that algebraic geometry would have to form the basis for an attack on the problem in its full generality, namely for arbitrary genus, but in order to bring it to bear on the problem he first had to develop a unified theory of algebraic geometry that covered the case that the underlying number field had prime-characteristic as well as the case of characteristic zero; this in itself was a contribution of great importance. Weil's proof of the Riemann hypothesis for function fields over a finite constant field, has rather profound number theoretical consequences, for instance about the number of solutions of a general type of congruence, and also about the order of magnitude of certain types of sums that play an important role in analytic number theory.

In this connection it also deserves mention that Weil in the last years has introduced the more general concept of the zeta-function of an algebraic variety of higher dimension than a curve, and conjectured certain general rules for the distribution of zeros and poles of these, in particular also certain connections with the so-called Betti numbers of the variety. While these conjectures so far only have been proved for special classes of varieties, partly by Weil himself, partly by others, and undoubtedly lie

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In our view there is at present no one else in the field of mathematics that it would seem more fitting and appropriate to bring here as a faculty member. The appointment of André Weil as a professor in the Institute would greatly benefit the younger mathematicians that come as temporary members, and it would add very significantly to the scientific prestige of the Institute.

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Weil

March 7, 1957

TO: Professors Beurling, Dyson, Gödel, Morse, Oppenheimer, Pais, Selberg, Whitney, and Yang.

FROM: Deane Montgomery

Two or three of the solicited letters have not yet arrived and will be sent later if they arrive in time.

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THE INSTITUTE FOR ADVANCED STUDY

Princeton, New Jersey

SCHOOL OF MATHEMATICS

ANDRÉ WEIL

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Surveying the world of mathematics and mathematicians today, it is extremely rare to find someone who masters with real knowledge and insight more than one or two of the main fields of the subject. A person really able to look upon mathematics as a whole with comprehensive knowledge and deep understanding of all these major fields is again a quite singular occurrence. André Weil in this respect occupies a rather unique position among his contemporaries.

His scientific production of papers and books deals with such a great variety of subjects as number theory and algebra, algebraic geometry, topological groups, analytic functions in several variables, differential geometry and even differential equations, and shows him to possess great power of penetration into the most difficult problems, ability to see connections between apparently distant parts of mathematics, and a high degree of originality in devising methods and techniques. Among his contributions are some that will undoubtedly always be rated among the finest achievements of mathematics in our time. His papers and books are written with a strong sense for the essential and fundamental and, not least, for mathematical elegance and beauty.

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Atle Selberg

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C O P Y

C O P Y

ECOLE POLYTECHNIQUE FÉDÉRALE
Mathématiques Supérieures

Prof. Dr. Armand Borel

Zurich, le March 1st 1957

On André Weil.

André Weil's papers are distinguished by their depth and their universality, pertaining notably to number theory, algebraic geometry, algebraic topology, differential geometry, Lie groups, function theory of several complex variables. Many of them are important not only by the results they bring but also by their influence on the subsequent development of the subjects they concern.

But, as important as they are, these papers do not give a full account of the importance of Weil's role in mathematics. His exceptionally wide knowledge of mathematics and strong insight have often allowed him to point out deep connections and make suggestions which have led several mathematicians to very fruitful lines of research; being one of the rare persons who can see mathematics as a whole, he has contributed significantly to the trend towards unification which has been so strongly felt these last years, and this as well by his papers as by his suggestions or by the work of the group Bourbaki, where he has been one of the main influences.

To summarize, A. Weil is an outstanding mathematician both as a researcher and as a leader; this, in my opinion, makes him particularly well qualified to be a Professor at the Institute. I hope very much that he will be offered such a position and will accept it.

/s/ A. Borel

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HARVARD UNIVERSITY
Cambridge 38, Mass.

Department of
Mathematics

March 1, 1957

Professor Deane Montgomery
The Institute for Advanced Study
Princeton, New Jersey

Dear Montgomery:

André Weil is one of the most eminent mathematicians of our time. I do not usually like to use the word brilliant, but I feel that it applies to him. Weil's work ranks amongst the most fundamental and important done in this country. He is one of the extremely few persons who is a mathematician and not a specialist in one of the fields of mathematics. As a Professor at the Institute, he could have a tremendous influence on the development and growth of mathematics. I feel that Weil is the man for the Institute and that the Institute is the place for Weil.

Sincerely yours,

/s/ Richard

Richard Brauer
Professor of Mathematics

RB:mh

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C O P Y

C O P Y

THE JOHNS HOPKINS UNIVERSITY
Baltimore - 18, Maryland

Department of Mathematics

February 28, 1957

Professor Deane Montgomery
The Institute for Advanced Study
Princeton, New Jersey

Dear Montgomery:

In reply to your letter of February 21st concerning the proposed appointment of André Weil to a professorship at your Institute, I am very glad to give you herewith my estimate of him as a mathematician. In my opinion, André Weil is one of the greatest mathematicians in the world today, and both in the breadth of his knowledge as well as in the depth of his insight he has very few peers. His mathematical achievements are of the very highest quality and importance, and his influence on the present generation of mathematicians, particularly the younger ones, is both decisive and extensive. In his own main field of algebraic geometry, if indeed one could assign so universal a mathematician as Weil to any one field at all, virtually an entire school has grown up under his leadership during the past ten or more years; and by personal discussions as well as by correspondences, he has exercised an influence on his co-workers in the field far beyond that of his own published works. I am aware that, in saying all this, I am merely stating what is more or less generally known of Weil by his scientific reputation among the mathematicians; I only want to say here that, having the good fortune to have been in scientific contact with him for quite a few years, my own impression of him fully confirms this high reputation.

I should like to add one further remark which I think shows the special importance of a man like Weil to your Institute. By his abilities as well as by his temperament, Weil is a born leader of research. In a conventional university, the only young people whom a professor usually comes in contact with are, beside possibly a few colleagues, mainly the graduate students, who are just learning the fundamental techniques of research and hence are usually not in a position to profit very much by contact with a man like Weil. It is only at an institution such as your Institute, where every year scores of young mathematicians of more or less proven abilities from all over the world gather for mutual inspirations and exchanges of ideas, that the potential abilities of Weil as a leader of research can be fully realized. While I believe that mathematical research should be free and not too much directed or guided by a single man or a group of men, however great, this by no means contradicts my opinion that personal scientific contact with a master mathematician like Weil would be of great benefit to many visiting members of your Institute. In view of this as well as the fact that your Institute is now without doubt the mathematical center of the world, I believe that the association of André Weil with your Institute would be of great benefit to the future development of mathematics.

Yours sincerely,

/s/ W. L. Chow

W. L. Chow
(Chairman, Department of Mathematics)

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C O P Y C O P Y

NORTHWESTERN UNIVERSITY
Evanston, Illinois

The College of Liberal Arts March 4, 1957

Professor D. Montgomery
School of Mathematics
The Institute for Advanced Study
Princeton, New Jersey

Dear Montgomery:

It is always hard to talk on the respective merits of various mathematicians, but of one thing at least I am sure: it may probably be argued with some plausibility that there are some mathematicians whose talents and achievements make them the equal of André Weil, but not that any one now living is superior to him. His towering position in the mathematical world of today is too well-known to need much comment; he himself used to say that a mathematician deserves to be called great if he has had at least one great idea in his life; by that reckoning, his position is secure among the greatest, for he has originated perhaps half a dozen of such big ideas. One of his most admirable characteristics (which, to my knowledge, he only shares with Chevalley and Serre) is the extraordinary fact that in the vast expanses of present-day mathematics, there is hardly a territory in which he does not feel at home and in which his knowledge is not as deep as it is broad, grasping securely the basic principles and the main problems of the theory, and leaving to lesser minds the secondary details or the pseudo-problems. Furthermore, his published work, tremendous as it is, is only part of the enormous influence he has exerted on his generation; and it may be said without exaggeration that he has done more than anybody to pull French mathematics out of the rut in which it was floundering in the 1920's, and to help bring it to its present activity.

Sincerely yours

/s/ J. Dieudonné

J. Dieudonné

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COLUMBIA UNIVERSITY
IN THE CITY OF NEW YORK
New York 27, N.Y.

Department of Mathematics

March 2, '57.

Professor D. Montgomery
The Institute for Advanced Study
Princeton, N.J.

Dear Montgomery,

I have, as you perhaps know, a very high regard for André Weil and his mathematical powers. While I do not consider myself at all competent to pass judgment on his work, I shall try to indicate below the factors on which this regard is based.

Weil has a deep and, at the same time, an unusually broad understanding of mathematics. This is rather rare in these days of specialization. He also has a very pronounced flair for elegance. One can say that mathematical beauty is created by combining depth with elegance. In Weil's work both these qualities are present in a considerable measure. Perhaps his best work is devoted to the study of arithmetic properties on algebraic curves, or more generally, on algebraic varieties. I can think of only three mathematicians (Siegel, Artin and Hasse) whose work in this, or a related, field, is of a higher standard.

Although the connexions between number-theory and algebraic geometry have been the principal object of his researches, Weil has taken an active interest in many topics, for example, in the transcendental theory of algebraic varieties, in complex manifolds, differential geometry, topological groups and fibre spaces. His book on Topological Groups, which was written before 1940, contains an able and elegant account of what was known at the time and has been of great value to the younger mathematicians. Another book on Algebraic Geometry made the foundations of the subject secure and gave a complete account of intersection theory. Although the style of this book is too dry for my taste, it does provide the reader with a solid base to build upon. In fact, Weil wrote this book largely to be able to construct his beautiful theory of Abelian Varieties and to do this in a way which would be above suspicion. This theory, in its turn, gave him the proof of the Riemann hypothesis for function fields which is regarded as one of the two most important achievements of Weil, the other being the proof of a conjecture of Poincaré.

I think I have said enough to show why I count Weil among some of the ablest mathematicians alive. He is extremely well-informed and shows great ingenuity in transplanting ideas from one field to another (e.g. his theory of fibre spaces in algebraic geometry). He has excellent mathematical taste and is quick in distinguishing the good from the mediocre. He is very receptive to new ideas and can swiftly reduce them to their essence.

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C O P Y C O P Y

(For example, the notion of transgression in topology, which was implicit in the work of Chern, was, I believe, first formulated by Weil.) He is constantly bubbling with fascinating conjectures in widely different fields. It is obvious that contact with such a man can be of immense value to younger mathematicians, as indeed it has been in many cases.

I have known Weil as a friend for several years. Outside mathematics he has a sensitive appreciation of art and literature and it is always a pleasure to talk to him. However, one cannot help feeling that if this remarkable man, with his rare intellectual gifts, had a moderating touch of humility in his make up, he would have been a finer personality.

Yours sincerely,

/s/ Harish-Chandra

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March 6, 1957

Dear Deane:

I am very enthusiastic about the idea of appointing André Weil to a position at the Institute, and I cannot think of a more appropriate choice. Like most mathematicians of my generation, I have a great admiration for Weil's work and for his masterful command of the main streams of mathematical thought. I believe that there is almost unanimous agreement among the mathematicians of today that Weil's work has had a profound influence on the growth of at least all of the following: the theory of topological groups, algebraic number theory, and algebraic geometry.

It should perhaps be pointed out also that the inspiration and training of many of the younger mathematicians in France can be traced back to Weil, via Bourbaki. In particular, it was chiefly Weil (during the early 1930's) who directed the attention of his colleagues in France to modern algebra, and who thus primed the development of the algebraic methods that have been so spectacularly successful in almost all branches of modern mathematics.

Although of at most secondary importance, it may be of interest that Weil's intellectual and cultural interests range also outside the field of mathematics. For instance, he has considerable facility with classical and ancient languages, such as Latin, Greek and Sanskrit. In part, this is in evidence in his work on the history of mathematics.

There can be no doubt that Weil would have a profoundly constructive influence on the mathematical life at the Institute, and that he would greatly contribute toward enhancing the position of the School of Mathematics of the Institute as one of the chief focal points of mathematical research.

Sincerely yours,

/s/ Gerhard

G. Hochschild
(Professor of Mathematics at the University of Illinois. Currently a Guggenheim fellow and member of the Institute.)

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MASSACHUSETTS INSTITUTE OF TECHNOLOGY
Cambridge 39, Mass.
Department of Mathematics

February 25, 1957

Professor Atle Selberg
The Institute for Advanced Study
Princeton, New Jersey

Dear Professor Selberg:

I received your letter of February 19, 1957, asking my opinion on the mathematical accomplishments of Professor André Weil. It is of course very difficult for me to give a full account of the merits of such a prominent senior mathematician as Professor Weil, so I shall write here only some features of his versatile works in which I myself am mostly interested.

Ever since his doctoral thesis on rational points on algebraic curves, Professor Weil seems to have always been interested in algebraic geometry and its application to arithmetic problems, thinking that algebraic geometry, in particular, the theory of algebraic curves, is a bridge connecting the two classical branches of mathematics, arithmetic and analysis, and may eventually provide a most essential key to the solution of the famous Riemann's hypothesis. Led by a similar motive, Professor E. Artin already proposed about thirty years ago the study of zeta-functions of algebraic curves defined over finite fields. The proof of the analogue of the Riemann's hypothesis for such functions had then become one of the most fascinating problems in arithmetic and algebra, and it had been studied by many mathematicians until Professor Weil finally announced an ingenious idea of solving the problem using the theory of algebraic correspondences of algebraic curves and the theory of abelian varieties of characteristic p. But, in order to carry out such a proof, Professor Weil had then first to give a new solid foundation to classical algebraic geometry so that it can also afford to handle algebraic varieties defined over fields of characteristic p, and his complete solution of the problem including such a fundamental work on algebraic geometry has been published in the form of three books since 1946. I think this is one of the most important contributions to mathematics in this century, for Professor Weil thus not only obtained a deep result related to various domains of mathematics, but, in doing so, he also opened a new scope in algebraic geometry which has since become a very active branch of mathematics where now many mathematicians are working successfully on the foundation

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laid by him. Having published those results, Professor Weil is still continuing vigorously his research on arithmetic and algebraic geometry, in particular, on the problems concerning various zeta-functions derived from algebraic varieties, proposing at the same time new interesting conjectures, the proofs of which are now principal targets of research in this domain.

Besides his work on arithmetic and algebraic geometry I have just mentioned, Professor Weil has also made many important contributions to other branches of mathematics, particularly, to the theory of topological groups and to the theory of complex manifolds, and, just as in arithmetic and algebraic geometry, those results of his are now regarded as forming basic parts of the respective branches. But, perhaps more significant is the fact that all of his works have inspired younger mathematicians to launch further research of the subjects he originated and, in fact, with fruitful results; in such occasions, he is always willing to give his valuable suggestions on the subjects to his younger colleagues.

I believe Professor Weil will continue to make great contributions to mathematics as one of the leading mathematicians in the world.

Sincerely yours,

/s/ Kenkichi Iwasawa

Kenkichi Iwasawa

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C O P Y

C O P Y

COLLÈGE

de

FRANCE

Chaire
D'Algèbre et Géométrie

Paris, le 27 Février 57

Cher Montgomery,

Bravo! Je suis enchanté, c'est la meilleure chose que l'Institute pouvait faire. Je souhaite très vivement que vous réussissiez auprès de vos collègues non mathématiciens et des "trustees".

Je vous envoie ci-joint une lettre plus "formelle", disant la même chose.

Bien à vous, et encore une fois bravo!

/s/ J-P. Serre

J-P.Serre

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C O P Y C O P Y

COLLÈGE

de Paris, le Feb. 27, 1957

FRANCE

Chaire
D'Algèbre et Géométrie

Dear Professor Montgomery,

I am very glad to hear that you and your colleagues are wishing to have the Institute offer a professorship to André WEIL.

I think this is an excellent choice, the best you could make. Like most of the mathematicians of my generation (and of the preceding one too), I have the greatest admiration for the scientific work of André WEIL; I even believe that, among the living mathematicians (in all branches of mathematics), he has no superior, if any equal. No other seems to me to have the same global (and yet so deep) insight of mathematics, the same rare combination of creative imagination and of "brute force". It would be useless to try to list here his main contributions. Let me quote only his thesis, his book on locally compact groups, the "Foundations", his papers on class field theory, on complex multiplication, his proof of the Artin-Riemann conjecture for function fields of one variable. These are all contributions of fundamental importance, which have opened up new and very promising fields of research; to give a personal example, I know nothing in mathematics more exciting than WEIL's results and conjectures connecting the zêta function of algebraic varieties with topological properties of these varieties.

It would also be good to quote other contributions of WEIL (to topology, functions of several complex variables, etc), to say something about his influence on the mathematical schools of several countries (Japan, and also the U.S. and France), but you know all that better than I do. I hope what I have said is enough to express my admiration for him; he certainly is the best successor the Institute could give to C.L.Siegel and H.Weyl.

With best regards

/s/ J-P. Serre

Jean-Pierre SERRE
Professor at the Collège de France
39, Boulevard de la Chapelle
PARIS, 10.