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Mr. L. R. Hafstad, Director,
Reactor Development Division,
U. S. Atomic Energy Commission,
1001 Constitution, N. W.,
Washington 25, D. C.

Dear Larry:

In connection with your impending discussions with the Board of Governors of Argonne National Laboratory I have become increasingly aware of the fact that this is another one of those cases which we discussed in Washington recently in which we at the Laboratory take it for granted that many items of past history are well known to you folks in Washington when actually there is no way in which you could have acquired the pertinent information. I have been under pressure from several sources since the war to write a history of the Metallurgical Laboratory and the Metallurgical Project. This I have refused to do because if such a history were to be written it would be of value only if it were a completely honest presentation of the Project's short-comings and difficulities as well as of its successes. In my estimation there were too many unhappy items that such a story would have to tell were it to have any pretence of objectivity and I certainly have not relished seeing them in print. My attitude has been that the sooner such headaches were forgotten the better. However, the whole idea of the national or regional laboratory, in the first instance, grew out of an attempt to provide a mechanism which, in case of a future national emergency, would in large measure circumvent precisely the sort of difficulities which I have not wanted to make a matter of record. It is obvious, however, that you face a somewhat baffling task in attempting to evaluate a national laboratory program which from the

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point of view of the Commission has very largely sprung from the waves complete with Participating Institutions, University Corporations (whose sole charter is to spend AEC money), strongly entrenched inter-laboratory groups in certain technical phases of the work, etc. I must confess that the diversity in appearance of many of these units suggests very definitely that they, like Topsy, "just growed". However, this is not true for in the beginning there was a very definite pattern and each piece fitted properly in its place. I must grant that in the past three years there has been more than a little deviation from the original design, but nevertheless the basic design is still there. This being the case it seemed to me that perhaps as a matter of background material, I should sketch out for you how it happened that things have developed the way they have. One thing must be kept clearly in mind and that is that this story is being presented by W. Hilberry as an individual and not as the Deputy Director of Argonne Laboratory. For better or worse, I fear it is true that the present pattern is to large extent my own handiwork.

Organization of the Metallurgical Laboratory.

My introduction to the Atomic Energy business was certainly characteristic of that of many of the hundreds who followed me on our project and I suspect the same thing was true of the other large war-time projects such as the M.I.T. Radiation Lab, the Proximity Fuse Project, etc. On Saturday night December 19th I received a terse telegram from Compton simply stating that he had an important war assignment for me, would I please arrange for a leave of absence and report immediately. Sunday morning I arranged for a leave of absence by telephone and Sunday afternoon I was on the train for Chicago. I returned to New York long enough to get my university affairs in order and then reported back to Chicago on January 1, 1942 for the first full meeting of those who were in one way or another involved with the chain-reaction business.

The group at that meeting was made up of the key staff members of the various independent research units that had previously been engaged on the work. The principal

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groups were those from Columbia, from Princeton and from the University of Chicago and were almost entirely physicists. I would say that there were just three major points of agreement at that meeting. The first was that there was a real chance that the chain reaction could be made to work. The second was that the work must be centralized so that all those engaged in it could be together at a single location. A third point of agreement was the willingness of everyone present to accept Compton as project leader. In some instances I am convinced that this was agreement with reservation, but by this time most of the men working on the project had become convinced that someone of Compton's prestige was required to head up the project if it were to be given the sort of Government financial and priority support that all realized it would have to have if it were to have any chance for success. On just about every other subject there were vital differences in point of view, a condition that has remained understandably chronic ever since. While understandable this situation was frequently critical because it stemmed from deep personal convictions of the individuals involved and since the accepted arbiter, experiment, was many times unavailable because of lack of time or of special materials, the very depth of conviction made decisions almost impossible to obtain, particularly decisions that would stick. The first four weeks of January were spent in trying to reach agreement as to where the project should be centralized. Three things were vitally needed. The first of these was physical space in which to work, properly equipped or readily equipable with the necessary utilities and other facilities requisite for laboratory operations. The second thing needed was at least a skeleton organization capable of handling the administrative and maintenance functions characteristic of a research and and development operation and capable of being expanded to care for the needs of what even then was obviously going to be a large technical group. The third requirement was for a corporate entity qualified and willing to accept the financial responsibilities involved in a large secret operation.

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One of the large industrial research laboratories would have provided the proper foundation for the contemplated operation as far as the above requirements were concerned. However, all such laboratories were already overloaded with their own specific responsibilities and definitely unavailable. Moreover many of the physicists involved in the project would have shied away from the industrial implications involved in such a choice had it been a possibility. On the other hand none of the universities really possessed either adequate space or adequate business organization to support the project even as then envisaged. As the search for a proper location went on, it became increasingly clear that the requirement for firm contractural support on the part of the sponsoring organization might very well prove more important in the long run than adequate initial available space or organization. Considerations of military vulnerability argues strongly against location either on the East or West coast. However, the decision to center the work at the University of Chicago was finally made not on this basis but almost entirely on the basis of the contractural considerations involved. Compton's long service with the University and his close contacts with both the University administration and the Board of Trustees provided him with an assurance of firm contractural support without necessity for disclosure of the objectives of the project to any but a very minimum number of individuals within the University organization. Four weeks had passed and the group as a whole were no nearer agreement as to where the work should be centralized than they were on January 1st. Except for the above contractural situation and the fact that Compton knew how to make use of what business organization the University had available, there was actually little choice between the various proposed locations. Consequently, Compton finally made the decision to center the work at Chicago. This decision was accepted. It was unpopular then to many of the individuals involved and I suspect that it is still considered by some not to have been wholly justified.

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With this decision made, we proceeded immediately to start the organization of the Laboratory. To say the least it was a painful procedure. Arrangements were made for the various research groups to continue their work at their then present locations until each investigation could be brought to a convenient stopping place at which time it was to be transferred to Chicago.

Realizing that the full research potential which could conceivable be brought together would be needed for the actual investigations that had to be carried out and that the administrative load of the Laboratory, particularly in its early stages, would be so great that who ever was assigned to this position would have no time for research activities. Compton appointed R. L. Doan as Laboratory Director. Doan had had experience directing a research laboratory for the Phillips Petroleum Company. He did an excellent job from the administrative side in getting the Laboratory into high gear in a very short period of time. However, he was never really accepted as Laboratory Director by the majority of the technical staff. This was partly his own fault but in greater measure, I believe, it was due to a carry-over of the university concept of "The Professor and Head of the Department". This attitude is one which is, of course, deeply ingrained in a great majority of scientists and, as you know very well, is still an acute problem in any organization whose backbone is made up of scientific staff members. The dilemma that if an individual is to be effective as an administrator he immediately commences to lose stature as a scientist or if he maintains his stature as a scientist he almost invariably fails in truly meeting his administrative responsibilities, if partially realized is very rarely acknowledged by scientists because it is not the university way of doing things. While even in the normal university department this tradition can cause trouble and occasionally prove disastrous, in a large organization it is always a crucial matter. This was one of our first major headaches, it was a continuing headache, and it is a problem that is still with us. Like so many other problems which arose it grew naturally and in-

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evitabely out of our traditional graduate-level educational policies.

The second major headache in the organization of the Laboratory was, of course, the matter of acquiring suitable and adequate scientific staff. In our case this was aggravated by the fact that several of the other major projects had already gotten into full swing before we started to organize for an all-out effort. However, there would have been little difference in method had this not been the case. Staffing was almost entirely a matter of wiring, phoning or writing to someone we knew personally who had at least some of the qualifications necessary for some particular job that needed to be done immediately. In other instances it was some individual who some other member of the Laboratory knew or knew of by reputation. (We found, very shortly, that we did not know what the potential in scientific personnel even here in the Midwest really was, nor did we have any mechanism readily available by which we could determine it.) The result of this pseudo-nepotic method of staffing was that increasing the staff instead of broadening the Laboratory's viewpoint of the problems and thus providing a better base for decision or compromise, actually in most instances simply added additional strength to support one or another of the current sharply differing points of view. In some instances the differences between certain groups in the Laboratory went so far that results obtained by one group would not be used by another until they had repeated the investigation primarily on the basis that they really didn't know the folks in the other group well enough to be confident of their results. Our traditional training of the young PhD in individual self-sufficiency and in the requirement that he exert in all cases his own individual judgment upon occasion produced some very wierd results.

Even more fantastic in general were our efforts to staff our administrative operations. Here again we turned to our scientific confreres to take over little relished but obviously important administrative assignments. This was done consciously

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on the basis that this was a scientific operation and that these administrative tasks must be undertaken by someone with a thorough understanding of the "scientists requirements and the scientists point of view". Thus we had a physicist in charge not only of scientific, but of hourly service personnel. For some considerable time our procurement was in the hands of a leading meteorologist. I myself handled many of our subcontracts and it took the Manhattan District boys, after they took over, something over a year to unscramble the ensueing mess. Needless to say, our operations organization may have appeared ideal from the point of view of the scientific staff, but I don't quite understand how the Business Manager of the University of Chicago, Mr. Harrell, succeeded in living through those early days as I look back on them now.

Still another major headache was the matter of actual physical facilities. A university is designed partly for research, true, but primarily for teaching. University buildings, therefore, are in general not efficient for research and development operations on a large scale. You are aware of many of the make-shifts to which we were driven in finding space not that was suitable but that could be used. New Chem was about the only place in which it was possible to expand into temporary structures. The effort that went into modifying and remodeling totally inadequate space to provide quarters which could be used under the most rudimentary of safety considerations, was enormous. That the work was accomplished as efficiently and as safely as it was, is an amazing tribute to the men who carried it out in the surroundings in which they were compelled to work. Needless to say, while it was probably the fastest way to get the job done, it was by no means a cheap way.

Finally there was the problem of Laboratory organization. While we had organization charts galore in a very real sense the Laboratory had little or no organization. There were certain major groups such as the theoretical physicists, the experimental

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physicists, the chemist, the health division and the engineers, but the principal attribute of organization which is some semblence of discipline was almost entirely lacking. It was obvious that if the job were to be completed in time, one could not take time out to convert a large collection of rugged individualists into organizationally minded co-workers. It was essential to find some way, however inefficient, of using the staff as they were to the best advantage possible. It was obvious, of course, that no single individual could direct in a true technical sense as diverse a project as that into which the Metallurgical Project swiftly developed. The actual technical direction was accomplished by weekly meetings of the Division Directors, section chiefs and other key personnel who met and argued about the various phases of work. They would present their various ideas of what ought to be done next both by themselves and by others and frequently the arguments waxed hot and heavy. After the meeting each went back and did as he pleased. The major control during most of the Metallurgical Laboratory operations was on the distribution of space and consequently on the manpower which could be crowded into it. Occasionally the group as a whole would decide that certain lines of work should be curtailed. Even such decisions, however, were not easy to execute. Only in cases in which the time schedule required a firm decision which the group as a whole could not bring itself to make or in case of major revolts within the ranks did Compton make administrative decisions on technical matters. Technical decisions made by Conant & Company were usually hotly contested, and actually were few in number. This procedure, strenuous as it may have been, did produce results and extended as it was over some three years of active operation, it established a tradition which is still an integral and vital factor that must be reckoned with in the Atomic Energy Commission research operations.

Organization of the Metallurgical Project.

While the Metallurgical Laboratory was still in the throes of acute labor pains, it became obvious that the magnitude of the job was such that facilities other than

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those at the Metallurgical Laboratory should be used wherever possible, particularly, since the activation of a new facility usually made available additional technical staff. For example, the transfer of Seaborg and his group from California to Chicago left available facilities at Berkeley which could be used effectively in the chemical program. In addition Dean Latimer was available to head up the work there and for a variety of reasons there were a considerable number of the original Seaborg group who found it advisable to stay at Berkeley. Consequently, a contract was arranged with the OSRD for the Berkeley group to participate in the chemical investigations then being initiated. In similar fashion the difficulities in the production of metal of sufficient purity served as a challenge to Spedding to initiate a program at his own home base at Iowa State College. The work at Washington University, St. Louis, making use of their cyclotron facilities, was done under another contract in this same network, although in this particular instance it was largely a service rather than a research task. At a later time facilities at Battelle Memorial Institute were included in the program and still later a contract was set up at M.I.T. With the organization of Clinton Laboratories in the discussion stage, it was obvious that some sort of central clearing house was going to be required to tie these diverse units together into an effectively functioning organization. To accomplish this end, Compton was established as Project Director and I served in a variety of capacities from Assistant to a final title of Associate Project Director. In general the project office was made up of just the two of us though for a part of the time we had a third member of the office; Donald Hughes served in the capacity of my assistant for a matter of a few months and Dean Walter Bartky of the University of Chicago stepped in when the exodus to Hanford took place and served as my alternate during my absence, taking care of the routine project requirements.

The technical direction of the overall project was handled on exactly the same

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basis as the technical direction of the Metallurgical Laboratory proper. At the bi-weekly meetings of the Division Directors and section chiefs, representatives were present from the outlying centers. They made their reports and entered into the general discussions on the same basis as the Division Directors and other key staff members from the Metallurgical Laboratory itself. While this method of operation definitely appeared to be unwieldy and inefficient, it very definitely proved to be highly effective in an area in which so much depended upon a real research approach as apposed to a more purely developmental attack.

From the administrative point of view, all contract proposals originated in the Project Office. Official work programs were drawn up based on the discussions in the bi-weekly technical meetings, on the facilities and manpower available at each installation and on the Project Office's guess as to what new areas might become active during the contract period. In the same way the budgets for these various contracts were drawn up in the Project Office as a result of discussions with the various laboratory directors, again making due allowances for their chronic inability to foresee their full financial needs for the period in question. These programs and budgets were then transmitted to the various Laboratory Directors for submission to the OSRD or later to the Manhattan District for the purpose of negotiating the direct contracts between the institution involved and the Government.

Actually the Project Office as such had only the most meager of legal or contractural basis. It was never officially recognized in any of the contracts. It functioned with a shred of respectibility on the basis of a clause in the University of Chicago contract which held the University responsible "to do all things necessary" to accomplish the required results. This was intrepreted to mean that unless we could arrange these auxiliary contracts, it would be necessary for the University to expand

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its facilities and staff to accomplish the same ends at Chicago. In order to have the picture straight it was at one time seriously considered that the auxiliary contracts be made sub-contracts under the University of Chicago prime contract. Inter-university relationships and university pride being what they are, this suggestion never went beyond the exploratory stage for it was obvious that in all cases the universities preferred the standing they obtained by having a prime government contract rather than a sub-contract from another university. Actually the extra legality of the Project Office was never challenged, and it functioned as effectively as it could have done under direct contractural recognition.

Organization of Clinton Laboratories.

From the very start it was apparent that the Laboratory's task fell into three major phases. The first of these was the establishment of a controlled, self-sustaining, nuclear chain reaction in its simplist possible form for the purpose of studying its characteristics both as a physical and as an engineering phenomenon. The second stage involved the construction of a pile which would develop sufficient power to produce significant quantities of fissionable material for use in the weapons research program. This phase also involved the construction of a chemical plant capable of separating the fissionable material from the fuel elements. The final phase, of course, involved the design of full-scale production facilities. All three phases were started simultaneously. The Palos Park Site was obtained from the Forest Preserve Board for the purpose of handling phase one and phase two of the program. An immediate search was started for a suitable site for the production facilities. Exploration of necessary power and water supplies with the authorities in Washington who were allocating power indicated that only three major areas could be considered with any hope of obtaining suitable power. These were the Grand Coullee area in the Pacific Northwest, the Boulder Dam area in the Southwest and the Norris Dam area in Tennessee. Since many of the technical staff felt that the design and construction

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of the production facilities should be a Laboratory project, the two western locations were ruled out because of geographical separation. In April and May of 1942, T. V. Moore, R. L. Doan and I explored the Tennessee area and chose the present Oak Ridge Site. As plans for the pilot scale pile and chemical plant went ahead, it became obvious that they were anything but small scale endeavors and the public relations problem of possible hazards immediately materialized. Conant's committee decided that on this score and to insure maximum flexibility in the operation of these pilot plant facilities they should not be built on the Palos Site but should be constructed in Tennessee. In order to make effective use of these facilities, when built, it then became obviously necessary to erect laboratory facilities in connection with the pile and chemical plant.

This decision was unpopular to say the least. It involved the removal of the pilot plant facilities to a relatively remote and inaccessable location and moreover involved the organization of a new laboratory. Moreover the site had been chosen on the basis of production plant requirements which in many respects were the very antithesis of those which one would have used in the selection of a laboratory site. It was obvious very early that difficulities would be experienced in staffing the Clinton set-up. These difficulities were aggravated by the attitude of the staff of the Metallurgical Laboratory. The Metallurgical Laboratory staff wanted Clinton Laboratories organized as an annex of the Metallurgical Laboratory with the various divisions at Clinton reporting through Associate Division Directors to the Division Directors in Chicago with no central laboratory administration at Clinton Laboratories other than that required for purely business administration. This was obviously an administrative monstrosity which could not work and which would have made staffing utterly impossible. After a bitter struggle it was finally decided to establish Clinton Laboratories as a separate organization within the Project and reporting to the Project Office.

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Plan for University Institutes.

With Clinton Laboratories finally organized, staffed and operating, there was a lull in our organizational agonies during the winter and spring of 1944. It appeared to me that we would be definitely remiss in our responsibilities to the nation unless we made some effort to capitalize on the welter of nightmares through which we had lived and came up with some sort of a solution which would at least partially guarantee against a repetition of the situation in which we had found ourselves. From what I have already said, it is obvious that the major difficulities were lack of facilities, lack of administrative machinery and personnel, and finally lack of scientific personnel cognizant at least of the rudimentary requirements of cooperative effort. Of the three the last seemed to me to be by far the most serious. Many of the physicists knew essentially no chemistry and in many instances scarcely knew what a chemist did. The chemists were better cognizant of the field of physics than the physicists were of chemistry, but still there was little basis for professional cross-contact. Much the same thing was true of the biologists.

Our experience had shown that particularly in this field of atomic energy research there was a tremendous overlap both in interests and in scientific requirements between these various educational disciplines. Consequently, I proposed to Compton that after the war an Institute of Atomic Energy Research be established, staffed with physicists, chemists, biologists and engineers, housed in one building and serviced by its own business administrative staff. Such a building would furnish space in which a research and development operation could be started in an emergency and moreover it would be space designed for the purpose. Expansion could be cared for with temporary construction. The administrative organization would

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furnish a functioning skeleton group complete with procedures and contacts which could be expanded to take care of an emergency operation. Moreover, there would be established a scientific staff which due to the proximity forced by cohabitation would have established a basis for cooperative effort. Such a scientific staff accustomed to work with each other could then form the nucleus of a rational organization which could be expanded to meet emergency needs.

Such a proposal was drafted in the spring of 1944 and presented to President Hutchins of the University of Chicago. The present Nuclear Institutes are direct outgrowth of that proposal. I suspect that most of the objectives outlined here have long since been forgotten. Nevertheless, with the exception of the fact that the purpose of simpler financing, the original plan has been implemented, the three institutes are housed in a single building and are effectively staffed and an administrative organization is functioning. Similar set-ups have been widely copied so that in so far as this mechanism provides a partial solution for the need for mobilization of national scientific effort in time of emergency, this has been accomplished.

Staffing Hanford Liaison.

Early in the negotiations with DuPont concerning their assumption of responsibility for the engineering design and operation of Hanford it was agreed that at the start-up time of the Hanford piles the Metallurgical Project would arrange for the necessary scientific staff to carry through the start-up procedures. For legal and administrative reasons DuPont insisted that such staff be transferred to their payroll for the period of service at Hanford. In the spring of 1944 it became necessary to recruit this staff. The recruiting was difficult because of DuPont's insistance on DuPont employment, and the actual service period left much to be desired both on the part of the Metallurgical Laboratory staff members and on the part of DuPont. While the DuPont organization was far from blameless in the ensueing difficulities ~~still the~~

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inherent intransigence of our scientific people provided the major source of friction. This experience simply served to bring home once more the essentially delicate character and tremendous magnitude of the problems involved in an efficient mobilization of scientific manpower for a major scientific enterprise on an emergency basis.

In July of 1944 I was assigned to Hanford as Associate Project Director of the Metallurgical Project to serve as the official liaison between the DuPont Organization and the Metallurgical Project. It was an excellent vantage point from which to work on this general problem. The Metallurgical Laboratory personnel on duty in Hanford were, as I have said before, directly on the DuPont payroll and consequently not only not my responsibility, but in actual point of fact I attempted as far as possible not to interfer in any way in their relationships with the company. However, their continuing difficulities were very apparent and in one or two acute cases it was necessary for me to step in to the picture. While part of the difficulty was undoubtedly the fault of the DuPont Company, the primary source of friction was the same chronic intransigence on the part of our Metallurgical Laboratory folks that had caused so much of our difficulty in Chicago and Oak Ridge.

It was furthermore a vantage point in that it proved to be much easier to have a thoroughly objective view of both the Met Lab and Clinton Laboratories when one was stationed two thousand miles away and consequently removed from the welter of day-to-day operational responsibilities.

Moreover it was impossible not to compare the way in which the Metallurgical Laboratory and Clinton Laboratories operated with the way in which the DuPont organization itself operated. If there ever was a smoothly functioning stable organization, the DuPont organization was it. I found myself upon occasion wishing that our own

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Laboratories could have the same sort of clean-cut operation and disciplined behavior. On the other hand it was clearly obvious that the DuPont type organization failed to fully capitalize the potential individual contributions of its members. In the long-run I was forced to admit to myself that while administrative comfort would certainly be served by a well-oiled administrative machine and while such a mechanism very definitely provided a means of obtaining an official "record", it nevertheless was less efficient in producing research results than our own inadequate and halting method of doing things. It seemed to me then and the time and experience since has only confirmed the feeling that it should be possible to superpose eventually on our basic method of operation enough of the techniques of standard industrial practice to produce a system which would preserve our success in attaining research efficiency and still remove the majority of the intolerable administrative headaches. We have definitely not arrived at the goal yet, but I believe we are really on the way.

With the successful start-up of the first Hanford pile at the end of September, it was obvious that the Hanford Project was going to be a success. The other units came in as scheduled and in December the chemical plant went in operation with efficiencies far beyond our best hopes from the very start. This simply meant that the Metallurgical Project from a contractural point of view at least had come to an end. The phrase in the contract, "to do all things necessary for the design of a successful production plant", presented instructions which had now been fulfilled. It was obvious both at the Metallurgical Laboratory and at Clinton Laboratories that the sustaining drive had gone out of the operations because the objectives had been met and no new objectives were readily available. True, there were alternative processes which justified continuing work both in reactors and in chemical separations. There were also serious questions with regard to the results on reactor materials of long

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continued reactor operation. However, the urgency was definitely gone. In response to an urgent call from Los Alamos, a considerable number of the best research people at the Metallurgical Laboratory transferred to the still urgent bomb research. This further increased the sense of aimlessness among those remaining. It was obvious that the reason for the Metallurgical Project as distinct from the Metallurgical Laboratory was at an end. What the future of the Metallurgical Laboratory, the Clinton Laboratories, the Laboratory at Ames, etc., should be, was very unclear. During the late Fall of '44 we suggested to the Manhattan District that the whole business be wound-up and the laboratories be closed out. This suggestion was flatly rejected. The counter-proposal was made that the staffs be reduced but that the laboratories be maintained as stand-by organization, for possible necessary trouble-shooting in connection with continued Hanford operations. It was pointed out in reply that stand-by objectives were insufficient and that if the laboratories were to be maintained in any sort of effective operation, new objectives must be developed. We were promptly instructed to develop such objectives and present them for discussion and possible approval.

Thus we were faced with the question of making some attack on the problem of what the long-term future should be. The success of the Hanford plants meant that from the industrial point of view a questionable atomic energy research project had become an atomic energy business. What the future might prove concerning the usefulness of the project as a military business was unknown although there was little doubt in our minds but that this also would be successful and that the project would be an important military business as well.

If the latter point should prove to be true, as it was so proven in July of that year, it was clear that whatever we might mean by the expression "atomic energy business" it was certainly going to remain a Government monopoly for a considerable period

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of time. This meant that in terms of planning we had two phases to consider. The first of these would be the interim period until a long-term Government organization could be established to assume responsibility for the operations and the second phase would be that after such a government body was established. The problem then was to produce a plan that would keep the project facilities operating as going concerns during the interim, with as little loss of effectiveness as possible and to mold them into a pattern which would yield the maximum probability of supplying the long-term permanent requirements of a stable Government organization with an operating organization which could be assimilated with a minimum of reshuffling at the time of transfer.

As far as the project activities were concerned it was clear that the future program broke into two major segments. The first of these involved research in its broadest sense. This would include long-term investigations of nuclear physics and nuclear chemistry and of the affects of radiation on living systems at the basic level; it would involve more direct studies of the actual fission process and its chemistry and in biology studies of tolerance doses of radiation and proposed therapeutic methods; and finally it would require actual construction of reactors built primarily as research experiments to determine the scientific and technical characteristics of the wide variety of possible operating designs. Beyond this there lay the purely developmental studies which were essentially the responsibility of engineers rather than scientists. There was the whole field of development starting with the information made available by the successful operation of a reactor built purely as an experiment, reducing it to an operable prototype unit including all of the operational philosophy essential if such a device is to be industrially significant. On this basis we started an analysis of the project's operating units to see what sort of a long-term plan we could develop that appeared to make sense.

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From the start the most clear-cut case seemed to be that of the Clinton Laboratories. One of our major sources of trouble at Clinton from the start had been the reiterated complaint that it was not a proper place for a research center. To these complaints there was really no reply, because actually the site had been picked out as a production location and most of the requirements that we had established for such an operations area were definitely the converse of those which we would have established for a research location. The entire matter of isolation, poor transportation, lack of cultural facilities, etc., were items in point. On the other hand these same requirements would in large measure satisfy the needs for a development laboratory and engineering personnel are in general schooled to operating in such locations without complaint. With a strong program of production of radioactive isotopes already underway this gave an obvious starting point to justify a transfer from university contractural sponsorship to industrial contractor sponsorship. The University of Chicago had accepted the contract in the first instance under protest and felt very strongly that it should get out of the Clinton operation just as soon as possible. It seemed like a very rational proposal that Clinton Laboratories start out with the radioisotope production business and the development work associated therewith and then slowly grow into a reactor development center in the sense outlined above as new reactors were built and tested by the reactor research groups elsewhere. With this in mind Monsanto was approached early in the spring of 1945 concerning the possibility of their taking over the Clinton Laboratories' operation. This was arranged and the actual transfer was made that summer. In the meantime Compton accepted the Chancellorship of Washington University and as of July 1 essentially dropped out of the Project except as consultant to Monsanto. At this point the plans very definitely started to go awry. The staff at Clinton Laboratories had by then acquired very proprietary interests in the laboratory itself and objected to transfer elsewhere as had been proposed in our memo of February. They insisted on

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maintaining Clinton Laboratories as a research and educational center. With Compton's new responsibilities at Washington University there would be certain obvious advantages in a strong liaison between Washington University and Clinton Laboratories. As a result it was finally decided to attempt to make Clinton Labs go as a research and training center and instead of personnel leaving Clinton Laboratories there was a significant transfer of personnel from the Metallurgical Laboratory to Clinton Laboratories. The research program at Clinton Labs went ahead and in addition there was established the Clinton Laboratories' reactor training school in which Compton was actively interested. This transfer was both a blessing and a curse as far as the Metallurgical Laboratory was concerned for it removed from the Laboratory some of its strongest scientific staff. That among these same individuals were some of our real intransigents definitely eased the administrative problems.

On the whole, although administrative life was much simpler after the transfer, the Metallurgical Laboratory really suffered a serious loss at this time, because the majority of the personnel that transferred to Clinton Laboratories were reactor people and it was precisely from this group that the heaviest inroads had been made the preceeding fall with the transfer to Los Alamos. I may have been completely wrong, but I did not fight this transfer too severely because I felt quite certain that many of the individuals involved, come the end of the war, would not remain for any great period of time either at Clinton Laboratories or the Metallurgical Laboratory, and that actually as far as the Metallurgical Laboratory was concerned it was very largely a matter of when the loss would be taken and not whether it would be taken. As things have developed I am not sure whether this was right or wrong but I still have a feeling that it was probably right.

With a decision reached on Clinton Laboratories for better or for worse, the next problem was what to do with the rest of the operational units in the Project.

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In particular the case of the Metallurgical Laboratory was quite critical. Serious personnel losses, the knowledge that university facilities would have to be surrendered in the not too distant future, with the Palos facility only on temporary lease and with no clear objective for the Laboratory, the morale was at an exceedingly low ebb. It seemed clear that despite these factors there was a very real productive potential available in the Metallurgical Laboratory which should be preserved. Several things had become much clearer during the stay at Hanford than they were at the time the University Institute proposal was made. For one thing, even from the point of view of scientific mobilization, it was obvious that a laboratory like the Metallurgical Laboratory could accomplish many things that no single university institute could accomplish. It was clear for example that the magnitude of the operation should be larger than would be possible for any single university. Furthermore, temporary transfer of personnel between one university and another are not always easy to arrange. It was clear that many of the things which made this difficult between universities ceased to be factors in possible interchanges with a laboratory like the Metallurgical Laboratory; in particular, problems of university prestige would not be involved. An effective program of such transfers would extend the emergency mobilization possibilities to cover a skeleton staff representing the whole scientific potential of the region involved rather than the specific staff at one organization and consequently would come much nearer to meeting the real requirements of such an emergency situation. Another item that had become clear during this period was that the facilities required for this work simply could not be supplied to specially favored universities. Because of political pressures it was obvious that it would prove impossible for any Government agency to grant as expensive and unique a device as a nuclear reactor, for example, to one university without being willing and able to supply equivalent facilities to any other equally qualified university. The continuation of the Metallurgical Laboratory as a Government facility offered a clean-cut solution to thisproblem, because the Government could establish such unique facilities at its own laboratories without political

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involvement, and then through a cooperating program with any qualified university make these facilities available for broad use. These considerations pointed immediately to the concept of a regional laboratory which later events indicated should probably be considered as a national laboratory. Over and above these considerations was the growing realization that the atomic energy program was very definitely becoming a business. Unfortunately the full implications of this were not clear to me at that time. I was only sure that it meant that these regional laboratories must have broader coverage and be considerably stronger than we had previously considered necessary. Mental inertia being what it is it was only natural that in approaching the problem of organizing these regional laboratories previous work on the university institutions should have been used as a pattern. Had the full implications of "being a business" struck home this might not have happened. There was one further consideration which also entered into the original regional laboratory thinking, and that was essentially a matter of insurance. We were certain that we were facing a transfer into long-term operation under a Government agency. To most of us this immediately brought to mind the Bureau of Standards which stood out in our estimation as a tower of mediocrity. We were definitely searching for some mechanism which would at least give us a chance of sufficient strength to fight any proposal which would appear to start us on the down-grade with a Civil Service Employment system guarantee of hitting the bottom. The Participating Institution program concept seemed as if it might supply a potential tool which could be used to protect ourselves if the worst came to the worst.

From the spring of 1945 through July 1946 I was more or less a free agent on this particular job. Particularly after Compton dropped out on July 1, 1945, I, as Associate Project Director, was the whole works. During the ensueing year I spent most of my time essentially as a consultant to the Manhattan District on this problem

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of organizing these laboratories on some sort of a stable basis. One of the first tasks was a primilary draft of a possible way of setting up the Metallurgical Laboratory as a regional laboratory. This was a continuing problem through May of 1946. In the meantime I had several long discussions with Pollard at Oak Ridge concerning the role which Clinton Laboratories might be made to play in connection with the Southern universities. The obvious need here was a matter of strengthening the research potential in the Southeastern states. Use of facilities at Clinton Laboratories on a cooperative basis with the Southeastern universities in many instances might make it possible for them to attract a caliber of research man which previous lack of facilities had made it possible for them to approach successfully. It did not seem to me that the actual research contributions from this area in the immediate future were likely to be large, but it did appear that if a successful scheme could be worked out centered around Clinton, that eventually the national potential in this field could be considerably increased and that certainly from the point of view of training technically qualified manpower, such a program would very materially help solve the overall problem. The Oak Ridge Institute of Nuclear Studies was the outgrowth of these discussions.

Using the M.I.T. contract as a base of operations, I held a series of conferences with Slater and Zacharias concerning the organization of a regional laboratory in the Northeast. Here there was a very considerable research strength in the sciences and also a very considerable engineering potential. Most of the universities were private institutions and consequently it was possible for them to organize a cooperative corporation to supply the contractural base necessary for the organization of a regional laboratory. It was obvious that in this set-up not only was the training phase of the program important but that the actual research and engineering contributions which the group could make to the atomic energy business were of great potential significance. Brookhaven National Laboratory grew out of these discussions.

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My discussion with the group on the West Coast at Berkeley concerning the possible establishment of a regional laboratory in that area got no where because the entire Berkeley interest was justifiably centered around the Radiation Laboratory which had existed long before the war and had very definite objectives for its work in the post-war period. There was no one at that time sufficiently well entrenched or sufficiently well informed at Palo Alto or in the Los Angeles area to feel that they were in a position to push ahead with a regional laboratory at either of those locations. Tolman was too completely occupied with affairs in Washington to be able to contribute to the possible local program at that time. Before I could take any other active steps in the matter the problem of the Metallurgical Laboratory became extremely active and I became so involved that no further effort in organizing a Pacific coast laboratory was expended. (In November of 1945 the Manhattan District appointed an advisory committee consisting of Compton as chairman, Eshbach, Daniels, Spedding, Gustavson, Tate and Loomis to study the problem of the future of the Metallurgical Laboratory and present their recommendations. The first meeting of this group was held on December 2, 1945 and I (Hilberry) presented to them my original draft of the proposal for a regional research laboratory. This group immediately started active study and the plan which I submitted went through draft after draft before finally being approved both by the Committee and the Manhattan District as the "Plan of Organization and Statement of Operating Policy of Argonne National Laboratory". These so-called bylaws were presented to the first meeting of the Council of Representatives of Participating Institutions on June 6, 1946 and were ratified by these representatives appointed by the Presidents of their respective institutions. At this same meeting the members of the Advisory Committee were elected to constitute the first Board of Governors. Dr. Zinn accepted the Directorship of the Laboratory on this new basis and the Argonne National Laboratory began its official existance on July 1, 1946.)

During 1945 and the first half of 1946 the Laboratory essentially floundered

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around as far as program and program philosophy were concerned. Beginning with July 1, 1946, however, based on the program assumptions set forth in the bylaws, a definite pattern began to emerge. The Laboratory definitely confined its operations to activities which were in their estimation distinctly direct Atomic Energy Commission responsibilities or which could only be undertaken with the unique facilities which the Laboratory possessed. More particularly the emphasis was placed within this larger area upon investigation which required the use of the Laboratory's reactors or which were closely associated in some fashion with the fission process including its many implications.

As in the case of the program so also in the case of site and facilities, both opinion and action vacillated throughout 1945 and well into 1946. It was obvious that the Laboratory had to move out of University facilities and certain of the activities would have to move in a very short time. In particular the activities housed in Ryerson which included the Laboratory's Central Shops, the Instrument Research and Development work and the Information Division, and the activities in Drexel House which housed the Laboratory's Health Services were under immediate and yet quite reasonable University pressure to move. Active effort was immediately begun to acquire a new site and to design essential facilities for that site. Very early, while ideas as to the future of the Laboratory were still extremely unsettled, a proposal was made to shrink the entire operation to one which could be housed on a small area at Palos Park. This was at a time when Gen. Groves was visualizing the Laboratory as a pure research organization, not concerned whatsoever with any secret or programmatic studies and consequently as being entirely outside of security restrictions.

As soon as the future status of the Laboratory began to clarify it became obvious that this plan of action was totally inadequate. The next step was to explore what could be done with a somewhat larger area within the Palos Site but here again the

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limitations of the site were such that they would have provided definite and serious curbs on the development of the Laboratory, as well as providing difficult terrain for Laboratory construction. With this in mind we started a search for another site and at least five independent suggestions were made indicating that the present DuPage Site would be ideal. Many government owned properties within a radius of two hundred miles were surveyed and other non-governmental areas were studied, but each time the considered opinion of those concerned was that the DuPage Site, from every point of view, was by far the best available. By the time that the Laboratory had been re-established as Argonne National Laboratory it was clear to those within the Laboratory itself that the Palos situation was unsatisfactory as a site completely independent of all political considerations. Steps were then started to acquire the DuPage Site by the Manhattan District and with the appointment of the Commission the responsibility was handed over to them and the site was acquired in early 1947.

In the meantime the Laboratory was trying to evolve out of its experience working in the war time facilities a philosophy of building construction which would make it possible to operate safely and efficiently, neither of which had been possible throughout the history of the Laboratory up to that time. The year 1947 was largely spent in such study and planning. In the fall of 1947 the situation with respect to Ryerson and to Drexel became crucial and the Laboratory proposed building certain temporary Quonset structures on the DuPage Site to house the Central Shops, the Reactor Engineering Group, which was working on CP4 and which had outgrown its quarters at Site A, and in order to centralize the Reactor work certain facilities for Metallurgy. The move of Metallurgy from Site B to Site D would make space available in Site B into which the Health Service activities could be moved, thus taking care of the Drexel situation.

On January 1, 1948 the Commission assigned to Argonne National Laboratory the

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responsibility of serving as its center for the overall reactor development program. This immediately required that the temporary structure construction program at Site D be reorganized to take this new responsibility into account. The first requirement was an increase in the areas required both for Reactor Engineering and for Metallurgy and some revision of the space required for the Central Shops. In order to get the Laboratory administration on a sound basis, since the Laboratory Director had to be at Site D, and since the administrative activities were poorly housed in the city, a distance of 25 miles away from the Director, it seemed advisable to make provisions for all of the administrative activities in temporary quarters at Site D until the permanent administration building could be completed. This also made it possible to put in the necessary Health Service facilities at Site D which the enlarged program required and to house the Information Division which had been, up to that time, somewhat homeless. This program was approved and design and construction started.

In the meantime, basic design studies had gone far enough with respect to the permanent buildings and the requirements for the site had been developed sufficiently so that decisions had to be made as to the size of the buildings which were to be built which meant in turn the size of the Laboratory staff to be accommodated. Early in 1948, therefore, a study was made for purposes of site layout to determine the maximum size of Laboratory staff which should be considered. This study was put on the maximum basis because it was to be used for a determination of building spacings, utility layouts and other similar site planning activities. The problems of each division were studied on the basis of scientific administration primarily, although program considerations were also included, to determine the maximum number of scientific staff members which could be handled in a division before administrative difficulities would cut the operating efficiency. On this basis it appeared that a laboratory having as many as twelve hundred staff members, including both senior and junior scientists could be tolerated before the individual divisions would experience serious administra-

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tive inefficiencies. This staff of approximately twelve hundred scientific staff members would probably mean an overall Laboratory population of forty-five hundred to five thousand people. This represented a maximum beyond which planning would be useless, but up to which area for expansion and provision for utilities, roads, etc., should probably be provided. This proposal was presented to Carroll Wilson, Bacher, Fisk and Gen. McCormack on February 15, 1948. It was agreed that such a figure might make sense in terms of basic site layout and provision for possible future expansion, but that Laboratory design should be based on a figure approximately 75% of the maximum figure presented. This corresponded to a laboratory of some nine hundred scientific staff members and a total population of somewhere between three thousand and thirty-five hundred people. The Laboratory then proceeded with its planning.

On this basis the Laboratory developed its requirements for net operating space, In discussions with the Atomic Energy Commission, these net operating areas were extended to gross operating areas on the basis that net area would prove to be approximately 80% of gross. While considerable planning had been done particularly for the Chemistry Building and design had not progressed far enough to show the serious unreality of this assumption. With further design it became evident that the ratio of net to gross for buildings designed for atomic energy work is much nearer 40% or 45% than 80%. In the meantime, however, the gross figures were the ones that had landed in the record rather than the net figures.

Due to an even worse miscalculation in the construction estimates, proposed construction figures were entered into the record which bore no relation whatsoever to reality.

With unrealistic gross area estimates and even more unrealistic construction cost estimates frozen on the record in the form of firm ceilings for the Laboratory's facilities, the Laboratory found it essential to commence discarding any structure that it could conceivably get along without. The first to go were the administration building

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and the lecture hall. Following this the permanent medical building was discarded and the proposed guest house. Then it was decided that the central shop building and the metallurgy buildings should be discarded and the temporary structures then under construction revised to serve as permanent quarters for these activities. The same decision was made to cover the cancellation of the administration building and to provide for the health service activities of the medical division. Then the Laboratory started cutting the size of the buildings from that originally proposed even though the ratio of gross-to-net had already provided drastic cuts in the available working space for the various divisions. Each of these successive cuts, of course, meant further cuts in the proposed size of the scientific staff.

All of the original staff planning had been done on the arbitrary basis that working facilities would be provided for one temporary staff member from the Participating Institutions or other outside organization for each two permanent staff member. The successive cuts in available net area, therefore, meant either that the facilities available for workers from the Participating Institutions must be considered as drastically reduced or that the number of permanent staff members upon whom the success of the Laboratory's operation really depends must be cut. In many instances this would mean that the permanent staff would be no larger and in some cases actually smaller than that presently functioning within the Laboratory. The realization of this fact is one of the reasons that the whole problem of the Participating Institutions has arisen at this time. Another effect of this slow whittling attrition has been to increase the cost of the temporary structures very considerably above what was originally contemplated. Granting that the original cost estimate was completely in error, it is still true that the necessity for considering the East Area as permanent rather than as temporary increased their cost significantly.

Associated with the staff planning difficulities as connected with the Participating

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Institution program, various other items have arisen in the past two years which bring the existing bylaws into sharp clash with actual operating practice. For example the dates set for the Board of Governors meeting were bases on Manhattan District requirements for presentation of program and budget and for contract negotiations. The bylaws were set up so that the Board of Governors could meet their responsibilities with respect to review of program and budget on the then operating schedule. These dates of submission have now been changed and for the Board of Governors to act on the new time schedule would require summer meetings which are exceedingly difficult to arrange when dealing with university personnel. Thus it is true that the Laboratory has submitted budgets and programs which could not be reviewed by the Board before submission and thus presents a definite challenge to the existing bylaws. The cuts in construction budget allotments for given fiscal years and the consequent slowing of the construction program has produced another point of difficulty as far as the Participating Institution program is concerned. It was obvious to the Board of Governors at the start that the Laboratory's facilities were so poor and so crowded that for some period of time the participating programs would have to be largely a matter of planning. However, as time has passed and new facilities have not been completed, the continuing lack of active cooperative programs on any significant scale has raised the natural question in the minds of the Board of Governors as to whether the situation will ever really get better, particularly in view of the continuing cuts being made in the size of the facilities being provided. On the basis of the nine hundred-scientist laboratory, three hundred of those places were ear-marked for the Participating Institution program. This left a total staff for the Laboratory's permanent group of six hundred as compared to its present staff of nearly five hundred, thus allowing very little leaway for future expansion of some of the scientific divisions in which such expansion should definitely take place. The three hundred staff memberships represented an average of ten per university, to be spread over all of that university's scientific and engineering divisions.

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This includes, of course, graduate students as well as staff members. I believe it is clearly recognized by the Board that this number is now going to be much smaller than three hundred; possibly much nearer fifty or one hundred, which would mean two or three graduate students and staff members per university on the average and probably a maximum of four or five for any given institution. I suspect there is some question in the minds of the Board of Governors as to whether a program of this size in comparison with the remainder of their duties at their universities is of sufficient significance to justify their time and effort. Finally on the part of the Laboratory the operating experience has done much to clarify our own ideas of the Laboratory's responsibilities. In the spring of 1946 it appeared as if the cooperative programs might very well be the major responsibility of the Laboratory. This was largely, I fear due to my own short sightedness in not realizing fully what the demands of the atomic energy business were going to be. It is now obvious that our first responsibility must be to establish the atomic energy business on a sound basis which means that we must produce research and development results and produce them swiftly and effectively. This means that while the Participating Institutions program is no less important than before, in an absolute sense, relatively it has become much less important than our major task of making the business grow.

Present Concept of a National Laboratory.

Atomic energy activities now are very definitely a government monopoly business, and from all indications will remain such for a period of five to ten years at the very least. It is important business both on the basis of its economic implications and its military implications. I need make no comments on the military implications. On the economic side, however, something should be said. While the United States probably does not need, at the present time, the additional resources which atomic energy provides, much of the rest of the world does. They cannot afford not to go ahead with strong programs in the field. If we do not push ahead because our need to do so is not

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critical we can be sure that very shortly we will have lost our leadership and with it we will also have lost part of our economic advantages. No matter how firm an international covenent may be made for the control of atomic energy, it can always be violated. While our association in such a covenent may keep us current on the latest knowledge in the atomic energy field, knowledge per se is no substitute for first-hand experience and in case of a military violation of such an international atomic energy control covenent we should find ourselves completely at the mercy of those nations who had the experience as well as the knowledge. I really believe that come what may the atomic energy program as far as the United States is concerned is vitally important business.

Unfortunately, it is also big business. The research and development activities alone are big business although they are only a small segment of the overall operations. This means that even in the management of the research and development activities, all of the trials and tribulations of big business must be faced and must be handled efficiently.

The atomic energy program is peculiarly a research and development business. It is much like trying to run a modern Edison Company power plant ten years after Faraday's first experiments on electromagnetic induction. We are faced with doing major engineering jobs on the basis of inadequate understanding of the fundamental scientific facts which underlie them. Consequently, research and development must play a much larger role in the atomic energy business than in a standard industrial operation if we are to have any chance to achieve success.

The research and development responsibilities of the Atomic Energy Commission fall into the following classes as far as I can see it. First, there are those researches which require unique AEC facilities, but which really are none of its fundamental business. These are relatively few but obviously must be taken care of. Then

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we run into a series of research and development areas which are AEC responsibilities and for which, due to its monopolistic control, it must assume major responsibility. The fact that universities and other research institutions may also be interested does not actually change the fundamental nature of the AEC obligations. The first of these areas is the fundamental research on the character of the nucleus itself. These investigations are of interest in universities as well as in AEC installations. They are, however, extremely expensive in many instances and university finances being what they are, interest in the field can very well be throttled to a very low level unless some sort of support for this work can be provided. A continuing flow of research results in this field from the national AEC laboratories can do much to sustain the interest in the universities and both attacks are definitely needed. The next phase of the research program is that dealing directly with research based on the fission process as far as the physical sciences are concerned and with research based on the interaction of radiation of all kinds with living systems as far as the biological fields are concerned. These are obviously direct AEC responsibilities and must be carried ahead in their own facilities. In many respects these are of less interest to the universities than the former catagory and consequently much more likely to be neglected, unless the national laboratories keep the fields active and exciting. The third phase, I believe, should be called research development. It involves the design and construction of reactors primarly for the purpose of scientific and technical study of the reactor itself. While the type of reactor under study may be under investigation because of the possibility that it will meet some future industrial or military need, the particular reactor as actually built is constructed as an experiment and not as protatype for future use. The same thing applies to chemical processes, metallurgical processes and in the field of biology to clinical studies in diagnosis or therapy. Finally there is the field of industrial development which starts in where the experimental reactor studies stop and attempts to develop a true industrially

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or militarily useful prototype reactor. This latter phase of the work, in my estimation, is the sort of thing which should be done in an industrial type laboratory under industrial sponsorship. The Knolls Laboratory would be definitely such an installation, and if the Westinghouse Company succeeds in building up a similar installation in Pittsburgh, this would supply another such center. This latter type of work definitely is not the business of a national laboratory in my estimation.

I should like to emphasize that in all of these areas over which the Government holds a virtual monopoly, and in particular in those areas whose research results are vital on either a short or long term basis to the development of the atomic energy business, the AEC is under a fundamental responsibility which it cannot avoid. This does not mean that the AEC should set itself up as a national science foundation for to do this would be to commit political sucide I am almost certain. I must, however, provide other direct or indirect support for this research which is vital to its development if not even to its continued existence.

With this in mind I would say that the first responsibility of a national laboratory is to see that the above research tasks are really accomplished and accomplished effectively. In order to do this the laboratories must conduct this research in their own facilities and theymust carry it on in all of the above areas. Research on reactors is such that it requires the backing of a really capable fundamental research organization in order for its own work to be effective. This applies throughout both the physical and biological sciences. There is no use designing a beautiful reactor for which no satisfactory chemical processing equipment can be developed. There is no point in developing reactors at all except for purely military purposes unless our biological knowledge is extended to the point at which these reactors can be used as economic tools with assurance that they present no biological hazards or at the least, hazards which are thoroughly understood. In this same connection the national research

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laboratories must stimulate (not support) research in their associated institutions. Any financial support for work in the Participating Institutions should come directly from Washington and not from the laboratories. Our experience to date indicates that were the Laboratory itself to become involved in institutional support, we would be opening a "Pandora's box" which could well spell our demise. There are other measures, however, which the laboratory can take to stimulate research in its associated institutions. Perhaps the best method is to maintain such a flow of high quality research results that the field remains one of challenging interest to scientists generally. Such research can be further stimulated by cooperative programs between investigators in these institutions and those in the laboratory, the work being carried out either at the Laboratory itself or at the home institution of the cooperating investigator. And finally the Laboratory can supply services, material and advice which will make it possible for the institutions to maintain this type of work. This latter point must be considered very seriously. The financial situation is such that the ability to obtain financial help is coming to have more effect on the character of the research being done in universities than the interests of the staff members. If the Laboratory can cooperate closely with investigators in these institutions and essentially give them the service and supplies and advice that they need to continue their work in this field it will do much toward ensuring a continued active flow of vitally needed research results. I should say that the second responsibility of the national laboratories is to guarantee the essential supply of highly qualified and experienced scientific and technical manpower. Again I am convienced that the best way of accomplishing this is to keep the field challengingly active by a continuing flow of high-quality research from the national laboratories. In order to accomplish this goal the best attack is to keep available at the national laboratories the latest and best research tools in the field. At Argonne the recent approval of the research reactor is a major accom-

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plishment in this direction. I personally feel that within the next five years when the Radiation Lab and Brookhaven have bevatrons in operation, Argonne will find it necessary to request that the AEC supply it with a copy of one or the other of those instruments. It is the existance of facilities of this kind that challenges the interest of the best research people and it is only through this kind of challenge that we can hope to produce the necessary supply of interested and trained personnel. As far as the actual training itself is concerned, this falls into two major catagories. In the first place the laboratory does its best job of training with its own employees who can be counted to spend a period of from three to five years in the Laboratory. It is hoped that we can maintain a flow of young PhD's through the Laboratory on about this timing basis. Theother way in which the national laboratories can take a direct part in the securing of an adequate supply of technical manpower is through cooperating programs with the Participating Institutions in their graduate level instruction. The Laboratory can accept temporary staff members from the Participating Institutions' faculties and thus strike at the problem at its origin. The Laboratory can also accept properly qualified graduate students for thesis work in the Laboratory and thus turn out scientists and engineers with suitable training. Finally the Laboratory can loan staff members for temporary service at the Participating Institutions as visiting professors and thus make a direct contribution to the training of technical manpower. Finally the Laboratory can continue its policy of presenting within its own facilities specialized courses to meet unique Laboratory needs or designed to utilize the services of instructors with unique training or the intructional potentialities of unique laboratory equipment.

The third responsibility of the national laboratories is to provide the type of emergency scientific mobilization which was contemplated in the university institute

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idea. I think it is obvious that the above Participating Institution programs automatically accomplish this end. The fourth responsibility of the national laboratory is to aid in the general educational program. I believe that the laboratories should not do this directly because they are not primarily educational institutions and should not be. However, within certain limits, they can render assistance in this field to qualified educational organizations. Actually Argonne now does this by making available speakers, preparing modest exhibit materials and contributing some general lay scientific publication. I believe that in general this is as far as Argonne Laboratory, at least, and I suspect as far as any of the national laboratories should go.

Participating Institution Program.

As a result of the experience of the last three years I would say that certain things have become fairly clear. I believe that active participation on the part of the Participating Institutions in the overall program of the Laboratory is utterly impossible. Even the subject of the basic research program is an exceedingly complex one. To be really effective in this connection it would be almost essential for the Board of Governors to pass on the hiring of each new technical employee because, as you know, the complexion of a research program is to large measure determined by hiring philosophy. This is one reason why we are so extremely careful in our hiring of scientific personnel to determine just insofar as it is humanly possible what the deep seated research interests of the individual applicant really are. The complexity of the research program at the most fundamental level is further complicated by the fact that with even the most rabid of the pure fundamentalists the challenge of a long range reactor program very definitely influences the research interests of the individual. In spite of the fact that the laboratory administration is in continuous contact with the work we must rely to great extent on the Division Directors for determination of program and for evaluation of results. Were the Board of Governors to

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give us advice on our research program which would be really valuable, it would be practically a full time job for them.

What has been said about the program is even more true about the budget. The national laboratories' budgets are extremely complex affairs. They involve all of the faucets of the budgets of any big business enterprise. For the Board members to be in a position to have any adequate judgment as to the soundness of one of our budget proposals would require far more time than any of them have available. I would say that it would take literally weeks of study before the Laboratory would feel that their advice was of value even in connection with the research program. Actually the Board members are all of them very largely scientists and by that very fact largely unqualified to pass on purely fiscal and operational matters.

What has been said concerning the budget can be repeated for the overall Laboratory operations. The handling of a department in a university is so completely different from the handling of an overall program which has to include fully as much attention to mangement matters as to scientific matters that there is really no comparison. The original group of scientists which one is almost sure to find on our Board of Governors is completely unqualified to have any judgment in the broad management field.

Having made the above observations of what the Boardof Governors cannot do, about which I feel very strongly, let me say just as emphatically that there are worthwhile things that the Board of Governors can do. The Laboratory's responsibility for assuring an adequate supply of trained personnel is important. The Laboratory needs the active cooperation of the Participating Institutions in its educational program. This is the business of the Participating Institutions and the Board of Governors are qualified to have sound opinions. The character of the program is

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not so complex but that the Board of Governors can make a real contribution in a reasonable period of time. Success in this phase of the program would be of a tremendous help not only to the Laboratory but to the AEC as a whole.

The Participating Institutions can also be of vital help in connection with the Laboratory's efforts to stimulate research in the associated institutions. Here the Laboratory needs the active cooperation of the Board of Governors in giving them advice on what the Laboratory can do for the Participating Institutions. Again this is legitimate business of the Participating Institutions and the Board members are qualified to give reliable help. Moreover it is help which the Laboratory must have if it is to assure success in keeping the fields of nuclear investigations and associated research active.

Finally the Board of Governors can help the Laboratory by consulting on matters of general research policy. All of the Board members are or have been research men and consequently they are qualified to speak on matters of research requirements. Most of them have wide contacts in the scientific world in particular and they could give the Laboratory invaluable support in the recruitment of qualified scientific personnel.

To summarize the following seems to be the situation as far as I can see it. In the first place the jam we are in is to large extent my fault, but this doesn't help matters very much. Neither the Laboratory nor the AEC can afford to allow any outside body to assume authority in connection with the Laboratory's research and development operations. On the other hand in connection with the Laboratory's educational program in its broadest sense neither the Laboratory nor the AEC can afford not to have the active support and cooperation of the Participating Institutions if the Laboratory is to be successful. I think what needs to be done is very clear but

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whether this position can be established to the point of convincing the Board of Governors I simply do not know.

May I again stress that this is purely a personal, not an official statement. Moreover, may I request that it be given as limited a circulation as possible. Many of the old timers would be sure to read into what I have written many things I have not said and to draw conclusions which are not intended. It could only lead to grief and even if my view was distorted it is not important for the present considerations because right or wrong it was the view on which the actions were predicated and it is, therefore, necessary for an understanding of what has happened.

Sincerely yours,

NH:ep

Norman Hilberry

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