Jean:

I do not want this.

Perhaps the Director's office would know of someone who is working on Dr. Oppenheimer's biography - ~~could~~ could use it. Seems a pity to throw it away; but unless someone else knows of some use for it, I'm afraid you must do so. G. K.

Noted
JR-K.

SCIENCE, VALUES, AND THE HUMAN COMMUNITY

Robert Oppenheimer

I think that my main function must be to initiate a conversation between us. Both those of you who come from abroad and those who visit in this country can hardly help but reflect seriously about any one of the three words in my title. Any one of them would seem to me to pose many more questions than we are likely to have answers for. As a matter of fact, only one of the words is the same in the title that I see now, as in the title to which I tentatively agreed some months ago. The word which is the same is "science." I suffer a little from my absence at the meetings which preceded this one, and there is always some danger that points that I make will go over ground that is familiar and which you may have disposed of before this. I hope that is not so, and I would look to you to interrupt me if you have a solution to any problem that I raise or an answer to any question that comes to mind.

It is after dinner, and I hope it will be all right if I tell you a story which is new to me, which I heard this winter, and which, in a way, really is not a bad text for the themes that we are going to be talking about. It has to do with a man who was driving into a moderately large American city. He had an appointment in town. When he came to the outskirts of the city, he lost his right rear wheel, and he got out and noticed two things: one was that he was stopped just outside an asylum for the insane, and the other was that the reason he had lost his wheel was that the bolts that had been holding it on were missing. So he started to look for them. He looked in the ditches along the road, with no luck. And a voice asked from across the fence, "Can I be of any help?" And he didn't answer. And then he looked in the tool chest of his automobile to see if he could find these bolts. And again the voice said. "Are you in trouble? Can I do anything for you?" And the man looked up and said, "Sure, I'm in trouble. I've got a date in town that's important and I've lost my wheel and I can't find the bolts to hold it on." And the man said, "I don't think that's so bad. You've got four wheels. They've each got several bolts. Take one off each, and you'll be

all right." And the driver looked up and said, "Say, man, you're not crazy." Across the fence the answer came, "Sure, I'm crazy - but I'm not stupid!"

Sometimes I have a little of the feeling that in surveying the wondrous effects of science on the world, this story is not wholly irrelevant to them. One could formulate the question suggested by the title perhaps with a certain added precision as "the effects of the development of science on the nature of our culture, and therefore, of course, on its educational problems." One could go about this by a comparative method, either in space or in time. One could take the institutions and the science of this country, and compare them with England, or Japan, or Turkey. One could do it in time, saying, usually, "Why are things so much more difficult? Why is everything so much worse than it was?" I do not propose to do either of these. And in particular I have a very nervous feeling about most of our educational and cultural comparisons with the past. It is true, that in the 18th and 19th centuries in England, there was a form of limited liberal education which had, both at the time and in retrospect, great charm and coherence. It was not, in my opinion, really serious higher education at all. Its purpose was the molding of a common sensibility and a common coherence, for a very small group of men destined to play a big part in the history of their country, and indeed in the history of the world. For its purposes it was very valuable. But I do not think that anyone today can seriously suppose that reading Horace, and even learning him by heart, is a unique or necessary condition for being a learned man or for being an educated man. That problem was different from any that we face, if only because of the small number of people involved. And it was different in the intense, cultivated effort at provincialism - almost the very opposite from what we think we are up against today, where we would very much like to have people learn the differences between cultures, knowing that if that is to be useful knowledge, they still must also have a true culture of their own.

I also know of my own knowledge that in most fields of learning the education which is available in an American university today is incontestably superior to that which was available when I was a young man, when I went to college 30 years ago. The comparison in physics is almost as night and day; the comparison in fields that are in

natural science, but not in physics, I cannot make with the same directness, yet I have no doubt that what I say is true. There are exceptions. I would say that a man 30 years ago could learn perhaps more warmly, and with a greater interest, the early stages of the study of philosophy than may be easy for him today. I think that in some fields the difficulty of the field is transplanted into its teaching. I do not know that this is true everywhere, but at my school I think I was lucky to be studying philosophy there in the 20's and not in the 50's. But in almost all subjects, it is quite the other way around. I am not saying that the young people of today get a better education. This is a vast subject, and we won't be through with it. But I am saying that their teachers know more, and teach more, and on the whole I think teach better. The variety is greater, the expertise and mastery are greater, the civility is greater, and the civilization is greater.

Nevertheless, I do have the impression that what 30 years ago in this country hardly appeared as a revolutionary and wholly novel problem, wholly new problem, and what in some parts of the world may today hardly appear quite in those terms, has by now caught us up in a situation unparalleled in human history, and with difficulties which are not easy to resolve by turning to the past - though, in truth, they cannot be resolved by forgetting it. It calls for, above all, a new definition of, and a new attitude toward culture and toward communication, and toward education, and toward learning - an attitude of accepting with a glad heart something of which men have for millennia spoken, and to which they have looked with a kind of horror.This is what I would like to talk about.

I have the impression that we are in this situation very seriously in this country, and that it is going to grow. I have the impression that nothing desirable can stop it. I have the impression that in Europe the problems that I am here going to outline are somewhat less articulated, and somewhat less pressing, but are latent and are part of Europe's trouble, as they are of ours. I have the same impression of Japan, though I speak with an exaggerated ignorance as I get further away from Princeton, New Jersey. I have another impression that the problems we are grappling with, cognitive and cultural problems, are destined to become worldwide, and that nothing but tragic alternatives are

true alternatives. This has, in part,to do with something I have already mentioned, and something that must have overwhelmed those who are visitors. But I believe that though this is a serious element in it, it is not the heart of the matter: that is, the increasing number who go to increasingly later stages - one might hope, higher levels - of education. This is surely something which in all your countries is underway, though it is in very different stages in various parts of the world and probably more advanced here than anywhere. There is no one from Communist countries here. But I think I am right that the proportion of people who spend, let us say, their time up to their 21st or 24th year in primarily educational pursuits, is probably larger in this country than anywhere else. It is clearly going in that direction throughout the world.

The educational and cultural problem is vaster than it has ever been before. In part because of that, and in part because of the underlying economy, this promises very, very much leisure in man's life, very much opportunity for cultivation of the mind, very much opportunity for reading, for playing music, for painting, for drama, for the dance, and for learning. It is already a different world from what it was a hundred years ago. This, again, I think is more acute in the United States; but surely it is our desire that this problem, if it is to be formulated as a problem, be a universal one. Surely, only things that we universally would deplore would prevent that. Thus the scope in terms of the number of people involved is unparalleled. In addition to that, and again along lines which to me seem undesirable to reverse, there is a kind of unhierarchical character to our society, something noted long ago by all thoughtful visitors, especially by de Tocqueville, a willingness to leave to the voluntary hands of a large population the ordering and organization of people, of interests. You see it in our cultures: I am sure that wherever you have been, there have been quite voluntary groups that put on plays, better, I think in most ways, than what you could see on Broadway; and voluntary groups that played music; and voluntary groups that talked about problems of science, and problems of politics. This enormous unorganized and uninstitutionalized, and totally nonhierarchical segment of this country is quite typical of us. But I do not think it is something which

the older, more ordered societies of Europe and Asia will long be without. We are, in any case, deeply dedicated to it, and an American society, ordered from the top, and ordered uniquely, ordered monolithically, in which everybody knew who was the best scientist, who was the best composer, which was the best band - would be a repugnant one. In fact, the attempt by the magazines, by the press, by the television outfits to create such an order is resisted by the strongest tool of the American public, which is their laughter. They know that these ratings are transitory, and they pay very little attention to them.

The other side of the story is perhaps best put brutally. In the 17th century the sense of a new world began to spread, and the feeling that the old order had been shot out from under was sharply articulated. In the late 16th and 17th centuries some of the finest poetry of the times was written. And people began to talk about the possibility which in the 18th century, especially in France and England, you will find referred to with awe and apprehension. Such a time might not be so far off when the sum of human knowledge would double every half century. It is, surely, very hard to know what one means by "the sum of human knowledge", if one means the Gospels, or the greatest literature. It is very doubtful whether such phrases have any kind of meaning. They are not in any direct sense knowledge. They are something much rarer, something very much more valuable. But they are apart from it.

It is arguable whether the doubling time of knowledge now, today, is eight years or eleven years, but it is something like that in terms of what man knows. And this has been achieved by the development in the full, broad, noble sense of the word "science." It is most strikingly true in the natural sciences, and can even be measured by volume of publication, because, as you all know, really trivial things a society will protect itself from. A group of physicists or a group of biochemists will manage their journals in such a way that what is in those journals is worth reading to the specialist. The quality, I would say, in all branches of science in which I have either knowledge, or competence, or interest has gone up as the quantity has followed this fantastic rate of increase.

This knowledge is, of course, not without its order. In fact, it is all about order. Its whole purpose is to relate experiences to each other, and to show that not everything in human experience is arbitrary. But it is not orderly in the sense that there are a few general premises from which you can deduce everything else. And it never will be. The conviction which the physicist has that the facts of chemistry could be deduced from physics, if someone tried, does not mean that chemistry is a part of physics. And it should not be at all. The interesting parts are those which no physicist would ever have thought of deducing. We may be confident in the light of the great insights into the origin of life and the nature of genetic material, and the coding techniques which all living organisms have and which dominate such primitive things as perception - we may be confident that there will be a total lack of gap between the biological and psychological description of man; still this will never reduce the parts of psychology to biology.

This enormous house of science is a related, but it is a very subtle and beautifully related thing. It could not be more different from the early 19th century nightmare of La Place that one has only in a static moment to be temporarily omniscient, and all history will follow this moment of insight. It corresponds to an entirely different ideal of human knowledge, in which from the very beginning it is understood that if you pursue one means of sorting out experience, you exclude many or all others; and from the very beginning, starting with the most elementary, common-sense things, marching off in different technical and specialized directions, adopts and cultivates a view of knowledge as something which, in its nature, cannot be total; which is always partial, and selflimited. But all of this means that the problem of acculturation, and the problem of community, and the problem of education - and they are all related problems - have a character quite different than they have ever had in the past.

I have often been led to notice with interest that fifty years ago William James used a metaphor for the nature of the inter-relations in our cognitive world. He saw

this not as global, not as a kind of description which one would give of a finite, closed object, a temple or such, which one might survey and describe and come back to again and again, until no further detail needed to be added, and it was there for all time. He saw it rather as an affair of networks or interconnections of relevance, some explicit, some remote; some recognized and some not recognized analogies, perhaps formal analogies of the kind that mathematics uses so much; perhaps logical analogies; often analogies that are verbal and rather thin; sometimes even effective analogies which play so enormous a part in the arts. It is a set of interconnections, not themselves exhaustible, between rapidly growing and highly specialized and enormously fruitful ways of knowing about the world. I would like even to extend what I say to the historical subjects, taken broadly, where the subject is what man does and where a very different kind of order and a very different kind of criterion of understanding has to apply, let us say, than in biology. Even here, the growth of knowledge, taken on a world-wide scale, is very much faster than at any time in the world's history. India - I do not know; I think that we have Indians here, and I think that India will have to write its history from sources that may be very hard to gather. It is a very long story, yet it will be largely done before this century is out, I am sure.

What then is the receptacle of all this knowledge? The people to whom it is entrusted and who create it are themselves not individual men. They are communities of men. They are the specialized professions, often increasingly specialized. We have a very strange picture of a world united by little bands of people who know a great deal about some field of natural knowledge, and with warm but often not very intense relations with neighboring fields. Again in the image of a network you have a picture of intimacy, of cognitive intimacy, an intimacy of understanding, clarity and usually goodwill and cordiality within these communities. You notice with hope, and then with melancholy that they do really stretch through all parts of the world. You think of them as holding the world together, and you don't think the bonds are strong enough

for the times we live in. And then you almost think that as far as the world of learning goes, we live in a kind of generalization of the old medieval guilds, a kind of syndicalism which is a cognitive syndicalism, a syndicalism in which the true community, the truly intimate collaboration of men, is best exemplified by these groups of specialists who understand each other, help each other, and of whom everyone here is in some sense or another a member.

One other thing before I make matters a little sharper. I believe that there is a good chance that what have been the protosciences of man are rather close now to becoming the many, many different sciences of man. I do not think that in the world of fifty years from now there will be a subject called psychology, any more than there is now a subject called natural philosophy. I think that different ways of studying man will lead to things which for convenience will have different names, will be in different buildings, and will have different professors. But I believe that we are on the threshold of an enormous enrichment in what we know about man. I do not mean that in a certain sense we will ever know more than we can find from the greatest art. But that is again something very different. That is not the kind of knowledge I am talking about, the kind that we cherish and of which some of you are students. That I am not trying to exclude it from the sum of human life must be evident. But that is not quite what I have in mind. I have in mind the same homely, in the end rather forbiddingly dull knowledge, that we get to have about such magic things as the stars, and about life itself. We never answer the questions which people thought would be answered when they said, Will we ever understand life? or, Will we ever understand what makes the stars move in their courses? We understand other things, and we answer other questions. And so it will be with the sciences of man. But I do have the impression that all the way from history to biology that great arc is about to catch fire, and that our preparedness to deal with that, and to see that it does not throw us off balance, and does not even further corrupt and corrode the vitality of our society may be a very important reason for being clear about this, even now.

Two traits of this world I would stress, two things which seem to me to mark it quantitatively surely, and qualitatively in my opinion from past times - certainly from the Athens that we love, certainly from the high times, let us say, in Elizabethan England, or the Enlightenment, or the great days of the Renaissance - all of them times of change, of great discovery, of unmooring. The two things I would single out are, first, the overwhelming predominance of things that are new over things that are old. This is of course a consequence of the fact that knowledge doubles every ten years. It expresses itself in the fact that no professional man can really be any good if he does not either by formal schooling, or by his own efforts, or by reading, really keep at school constantly. No engineer can leave school and hope to be a competent engineer with the problems of twenty years from now if he has not gone back, or done the equivalent. It is of course also a thing that happens not just in cognition. The application of knowledge changes the face of the earth. It is brilliantly illustrated in towns like Sao Paulo in Brazil, which simply grow as you watch. It has been very much illustrated on the face of Europe. And it is true here. People don't live as they did ten, twenty, or thirty years ago in the practices that their parents engaged in or the sensibility which their parents had; they live in a new and rather alien soil. This comes partly from the mechanical changes, partly from the changes brought about by communication. It comes about partly by the actual change in the substance of what people know. This is one imbalance, and I mean it not in terms of something that man cannot stand, but in terms of something that man is not used to and for which his tradition has not fully prepared him.

And the other is the proportion of what is known, that is known only to the specialized groups, is so very large. And the proportion that gets back into the common knowledge of man is so small. I have a prejudice - perhaps our English visitors can correct me - that England is the best country that I know in terms of the extent to which special knowledge is put into the common pool of the educated man. It is not put in

very deeply; it is not put in with terribly much reverence. That is the price you pay for putting it in at all. But it is almost not done at all in this country. And there is, as a result, a deep attrition of the common culture. The common culture does not have the kind of resources which it should be getting from these expeditionary forces that are going off in all directions, learning many new things. They do not enrich the common culture because almost nothing comes back. The transmittal back is entrusted too much to woefully superficial and often meretricious popularizations which do not get the meaning, or the beauty, or the weight of the experience communicated; and which do not, in a certain sense, engage or involve the general public. When I say this, I know that in a large part of science, one specialist does not get a good feeling for what goes on in a contiguous speciality; and as I have been forced to note and see with amazement, there is today practically no understanding among physicists of what modern mathematicians are doing. There is, I won't say boredom, but suspicion that this is a game that will never be a part of their world. And I am afraid that the other is true, that the mathematicians think the physicists are bothering about rather foolish things, which if they ever were really cleared up, the mathematician would understand very clearly. But he is not going to think about them for a long, long time.

Both of these things, the predominance of novelty and the absence of common knowledge, or at least the thinning of common knowledge, and the enormous growth of specialized and available knowledge, give one a sense that what in the past people have meant by values would have to suffer. Because whatever values are, they rest in areas of life which are familiar and deeply intimate. They have a dual character mostly; one can think of exceptions. On the one hand they are commitments, commitments as to where one stands, and where one acts, and what one will be, and what one treasures. On the other hand, they always involve memories; the former is perhaps more true of strictly ethical values, and the latter perhaps more true of strictly aesthetic values. I deplore the fact that quite the same word is used for them, but in a summary speech which did not have that word in its original title at all, I ask you to allow me this slight confusion.

Values involve this double element, all of them: the commitment to the past and commitment to the future. With the enormously rapid change in our lives, partly cognitive, partly technical and practical, it is not unnatural that one would have a sense of evaporation, and emasculation, and vagueness in values. Our past is not as manifestly meaningful for our future. The problems that we meet in our future are not like those which we suffered, and from which we learned, in our youth. There are some analogies. But the danger is that if people today articulate what they think about the good and the beautiful, their words will have a kind of awesome vagueness, an awesome lack of the specific, the robust, and the intimate. The business of human life has to be the business of using what we were to become what we shall be. And it means that just the things that you have been close to, that you have felt as well as learned, that you have learned to do and learned about, that you have learned as arts, that you have been with for a while - that these have not changed too rapidly - are still present, are still relevant to your colleagues, to your associates, to your community, to your society, and to your future.

One could say, of course, if human life is so much a matter of familiarity and values, and so much a matter of intimacy:"Why cannot all this rapid change be stopped?" We are dealing with a human institution, it is ever learning and changing. There are some reasons why it has hit the United States peculiarly hard, reasons which historians would recognize, which we may talk of later. It is obvious that it can only be stopped by two kinds of things. It was once stopped, I think. There was a great Muslim Renaissance for three centuries. About the year 1100 something happened, and there was no more Muslim Renaissance. And what stopped it can be done again. It was military conquest and religious orthodoxy. But without some horrible combination of these devices, it is clear that men's curiosity, their adventuresomeness, their cupidity, will all conspire to favor the conditions for the wonderful growth of knowledge with which we live. It is even so that I think that there are some countries (and I speak with great tentativeness, I think that perhaps it might be said of France) where the pace of change is something which

people would have liked to resist, that it would have been natural not to let mechanization and large-scale industrialization go very fast in France. France lives, among other things, as a very varied country, and I think those in the country who were activists would probably have prevailed. But France lives next to other places like Germany, which is very active indeed. And it is quite clear that in no real sense is this an open course for the people of France. I am quite sure that many who lived in China a century ago would have been utterly appalled at the notion that China was to change in a radical way. Yet it seems to me to be very unlikely either that it will go happily or that it will stop. I think, in other words, that it is our mission, if we have any influence, to avert an apocalypse, and to avert the kind of fanatic ordering of belief and knowledge and activity, which could put some fixity back into our cultural lives and our practical lives as well. I think that we have to accept the situation that I have outlined.

But I think above all we have to recognize that the notions of education, and the notions of culture which we inherited and which were natural a century ago, and still were more natural several centuries ago, are today quite misleading and quite sure to lead us to doing the wrong thing. We have to accept this, and it has to bring us to cherish the thing which is so in danger in the world, which is just the touch of intimacy, of craftsmanship, of skill, of true, deep understanding - ranging all the way from simple things in human life to the most recondite things that one can learn in mathematics, or biology, or history: love of the expert, love of style, love of technical competence. We have to do this at the same time that we know that whatever little we happen to have that is familiar and intimate is only one of an incredible number of things, most of which are rather remote from us. We learn about them through friends; we learn about them through reading; we learn about them by luck. We won't learn them unless we are very fortunate, and very talented. This is the difficult balance between a kind of openness and skepticism, a welcoming of the new, a welcoming of what is unfamiliar, and a passionate devotion and appreciation of intellectual excellence among all excellences, of intellectual intimacy among all intimacies.

This view has implications for higher education. I do not propose to take much time laboring them. It does mean that a basic thing is that no man should escape our universities without knowing how little he knows, with some sense of the fact that not through his fault, not through his sloth - though he may be lazy and may not be very bright - but inherently in the nature of things, he is going to be an ignorant man, and so is everyone else. It would be nice if this great achievement could be complemented by another great achievement, and that is that although he is ignorant of almost everything, he is not quite ignorant of everything. And I think that my own feeling here is that education is indeed a necessary thing, if these two aims are to be accomplished. I would say that if I had to guess, if I had to advise a young man, if I had to make a rule, I would know that the rule would be wrong most of the time; and my advice would only be safe if it were ignored. But I would tend to say, try to learn something very well indeed, learn how to do it, not just what it is, learn it as a practitioner. And stop while you are doing it long enough to see the beauty of it. But don't quite stop with that. Learn something else that is also quite different. Get some sense of the span of things human, the span of things that the intelligent man can cope with. And I would say that at least insofar as science is concerned, I would trust such an approach far more than that of general education, which is essentially a description (almost, though it may be very good journalism, a sort of journalism) - a description of what has gone on in great broad areas of science. I know that one has to have some sense of the connection of things, and that if the network of our culture is many dimensional and infinitely complex, one needs at least a few projections of this network on planes that can be looked at. Survey courses and general accounts of things play a necessary part only because without them one cannot navigate. One has to learn to use the dictionary; one also needs to know what ichthyology is. But I do not think that this is education; I think that it is rather a kind of general preparing of people to navigate in the network, or about in the network.

I believe that the young people in this country, and I would deeply welcome your own comments, who know so much more about it than I - that the young people in the age

range from prepatory school to graduate school - are in a mood where an experience of this kind would not be entirely unfamiliar to them, would not seem entirely foolish to them. Compared to any times I have known, the young people in this country are less optimistic really; they are less confident of the future; they are less - do you know the phrase, "pie in the sky"? There is an old song, which goes "There'll be pie in the sky by and by." And the important part of it is the "by and by"; this looking to the future as the time of happiness and reward. They are very much less Puritan. They are in an important sense very much less Protestant. They are in an almost equally important sense, therefore, somewhat more Medieval - to find this in the middle of the turmoil of the 20th century is rather odd. But it is, I believe, also a form of protection. The reason why there is more music and art on the campuses of this country than ever before is partly because people are living for the moment; partly because to some extent they have lost confidence in the ability of society, as it were, automatically to reward the diligent; partly because in a kind of external conformity, an enormous internal freedom and spontaneity is often born.

I want to say one other thing. The communities that make, cherish, and foster knowledge and the arts are international communities. And in this age we can see clearly, not surely what to do, but that the sovereign, unlimited, all-powerful nation state is a pretty deadly and pretty impossible final form for the organization of mankind. I think that one should look with peculiar hopefulness to the innocent, international communities which either in the gathering of knowledge or in its application do bind people together from all over the world. That is indeed what, for the last twelve years or so, so many of the physicists throughout the world have been shouting, usually a very muted shout, as in the case of Niels Bohr, but a very deep and heartfelt shout. This is what animated some of the first efforts in this country after the war to suggest ways of coping with the new problems of atomic energy. The creation of vital, strong, international communities must precede the creation of international organs of comparable

strength and vitality. And this, in turn, is probably one of the things which must precede the formal regulation of the national will. That national will today is, in fact, subject to constraints which as little as thirty years ago would have been regarded as unthinkable.

Fulbright Conference on Higher Education

Sarah Lawrence College, Bronxville, New York

June 14, 1957

TALK AT PHILLIPS EXETER ACADEMY

Robert Oppenheimer

21 November 1955

Thank you, Mr, Saltonstall.

Members of the Academy and guests. I know that I am here not merely as one of the long chain of thoughtful men who have addressed you from this platform, but as a kind of guinea pig in a new attempt to bring to the school people who will take seriously the questions that are in your minds and who will talk seriously about the questions that are in theirs. I am really overwhelmed by the courtesy, the candor and the seriousness with which you have greeted me, and I thank you for it. I do not know whether this scheme is any use at all to you, but I have learned and I am learning a great deal in my visit and I am grateful for that--things that I would not have found easy to learn in any other way. I also would urge you--those of you who control the policies of Exeter--not to be discouraged if I have been a bad sample. I think the idea is a good one and if I have not come up to your expectations, try somebody else.

You have asked me a great many questions; some I thought I understood; a few I knew how to answer. Many seemed to me wholly beyond the scope of any human wisdom. One or two I thought other people could answer but I could not. One set of questions recurred again and again. It usually took a fairly vague form; namely, what, if anything, do science and religion have to do with each other. Sometimes I talked about it in a way I was not too ashamed of. Once or twice I said things I should not have said. I regret that and toward the end of this talk I hope to be able to come back to it.

Some of you are going to be professional scientists, not the majority. You are, I think, very fortunate. You will have the experience of seeing and--in greater or smaller measure--of discovering some of the wonderful order of the world of nature. There is not a higher pleasure, and I wish you luck. You will see things of breathtaking beauty and you will feel as I do that you wish you could tell other people about it; yet you will not be able to tell very many other people because very many other people will not be able to understand.

To the preponderant majority who are not going to go into science as a profession I have a wish which may seem rather visionary. You will be very busy preparing for your life; but I hope you will find it possible to take time out either here at Exeter, or in the University, or in later life, to do something that is not going to be easy. That is to dig into one field of science. Here I use the word very broadly; to me, archeology, serious historical study, psychology, anthropology seem part of the great effort to comprehend nature. I hope you will take time out to look into some part of some area of science independently enough to have the relevant experience, which is to see first that there is something going on that you do not understand, to have a sense of impotence and darkness about it, to find your way gradually into seeing what it is really all about, of seeing how it ties up with things you have known before, to see its order and its beauty. It is something you will never forget; you will all be richer men for that experience and maybe more understanding of the fellows who keep on doing that sort of thing as their life's work.

For science is not an inhuman activity; it is one of the great testaments to man's power and his reason, but it is always aware of its limits. He who practices it ought always to be aware--that its powers, though great, are limited, that he is not like God, that he is yet something rather special in his own right. You cannot work at reducing something new and unfamiliar to something ordered and understandable without understanding more about man and his place in the world as a result. It may not make you humane or tolerant or open-minded; it may not make you disciplined or moderate in your general life; but it will tend to do that.

Science is part of the human effort, a very great deep part of it, and it does not stand on one side of a football field with humanity over on the other side. Humanity is a way of doing everything, not least of doing science. This is a very old view. It is the view of a great majority of the thoughtful men who founded the United States of America, who wrote the Declaration of Independence, who wrote the Constitution, and who gave meaning to these documents. It had been an old story by the time they came along. I think it is probably true that the Greeks were the first, though maybe the Sumerians were before them, to introduce into human culture the notion of demonstration, the notion of establishing something that followed from something else with reliability and certitude. Many cultures have still not got that notion; yet they want to profit from the fruits of science, though they have not yet truly learned the simplest of its ingredients. In the 13th century there were incredibly serious fellows studying in Paris--all over Europe, but Paris was the headquarters--who made the first great effort in that time to establish a kind of learning independent of revelation--not, certainly, that they intended to be hostile to or contrary to revelation, but independent of it--and they discovered that Aristotle had missed some very simple points. They discovered that a body if you did not act on it did not remain at rest but remained in motion. They discovered what we learn about in school as momentum and what they called impetus and they laid the foundations for that revolution in our view of the physical world for which Galileo was to get into difficulty centuries later, not because he discovered it, but because he explained it in the vulgar tongue of Italian.

The 17th century saw what was in many ways the culmination of a rational view of the mechanical world, old fashioned and we know not really very complete; it had made an immense impression on the thoughtful people of Europe, who saw the hope of a world of improving material life, who saw the hope of a world of increasingly large understanding of man's troubles, his problems, his position; who, in what they too called the enlightenment, created the temper and the philosophy of those who founded the United States. You may think of Jefferson and Franklin--to both scientific things were of immense interest; their correspondence and their life is filled with the observations of nature and theories about it; they saw in science not something inhuman and hostile. They saw a lot of things. They saw first in practical terms that it was a strong tool against misery and poverty and squalor. They rightly understood that science would contribute to the well-being and the civility of life in America. They saw it in intellectual terms as a guard against ignorance and superstition, and against the fear based on them. They saw it in political terms as a guard against tyranny, barbarism and repression and bigotry, that they associated with centuries past, and above all with the religious wars, the inquisition, and what they thought of as the dark ages. It would not have occurred to them that science was an unfit occupation for a gentle man, it would not have occurred to them that it was anything but a proper and natural activity for a Christian. For them it was incompatible only with authoritarianism, with repression which they associated with the past and by which they were determined that this country should never be blackened.

I like to think of how they would respond if they came back now and had a look at things. We shall have to leave out of it that they would have had to go to Exeter, not as you do, for four years, but for twenty, before they could understand what was going on. We shall pretend that they understood without anybody helping them. They would certainly take an immense pleasure in some of the things that we are beginning to understand. Think, for instance: we are pretty clear now how the organic material on which life depends got made on earth. We think we see how these chemicals can have developed in such a way as to simulate some of the elementary properties of life. This is a very early stage of the story, and there are more questions than answers. We think we know what there is in all living matter which instructs its tissue to be a man or a tulip or a polliwog, a quite complicated kind of order that has to tell tissue how to develop and that is contained in the most subtle and beautiful little piece of machinery. I do not think we know the whole story at all; and I think we even are missing some key points; but we know enough to keep Jefferson and Franklin enthralled about this. In other words, it will not be so terribly long before, to the question "how did life originate and what gives it its stability, its mutability, its variety, its form?" we shall have a pretty sensible answer in terms of things which we can do or make up or understand. I think that the old men would have been incredibly excited by the story of the discovery of what atoms are really like, and what part they play in chemistry and what very, very odd sorts of laws govern their behavior. I think they would have been excited by the nucleus and some of the things it does and some of the properties it has. They would have been very excited by what we do not know yet, the ingredients, the simplest and most primitive material elements of matter. We are just beginning to get some insight into this.

I think they would have been delighted that for the first time we can understand and predict the trade winds that blow in the tropics and the moderate westerlies that blow in our own latitude and a lot of other things, the existence of which were not known to them. I think they would have been excited by the study of human perception by means of low frequency cortical currents; these tell you where in the cortex something is going on when you see or hear, and when things separate and when things come together.

I think they would have had an incredibly exciting time of it just catching up with the science of the day. And they would have been overwhelmed by what the application of science has done to man's life, by the extent to which it has lengthened his life, made it possible to alleviate and cope with his pain, and his trouble, made it possible to extend his powers, made power itself really quite abundant. And this is something that is now about to spread over all the world; it will reach unprecedented proportions, so that the work of the muscle is more and more going to be something for the football field and the mountain climber and the sailor and less and less a necessity of keeping body and soul together.

They would have been excited by the fact that we could get from any part of the world to any other, and that we could communicate quickly, and that we could store and use information so that even the managerial jobs of running a factory will not long have to be in human hands. All of this they would have said went far beyond their expectations. I do not know what they would have said of what we made of it all. I would like to know.

But there are three points--and these are in some ways rather troublesome points--of what science has brought to us that I want to bring up tonight. They are things that are connected, but I am not smart enough to explain or understand the

connection. They are things of which you might say I ought to tell you what to do about them. I think you would be right, but I do not know what to do about them. But I think that they need to be described and recognized, and that when they are described and recognized perhaps we will find how best to conduct ourselves as men in facing them. They are not things that science has brought as though science was a goddess bringing things in a horn or on a tray as in the old pictures. They are things which we would not have without the great increase in understanding, in technique, in knowledge and in power which rightly is called science. We probably would not have them either if we did not have the kind of ordered, friendly society in which science has flourished. We probably would not have them either if enormous investments in material things had not made their development possible. But it is right enough to say that they have come to us from science.

They constitute a difference between the present and the past; but, all such differences need to be talked about with a certain care. We look at the past and idealize it; and anyone who wants to make the past sound good, talks about the evils of today; anyone who wants to make today sound good, talks about the evils of the past. I know that I am over-simplifying it, but I think these traits are here. I suspect they are here to stay. I cannot imagine any happy way in which these things will disappear. They are respectively in the material, in the political, and in the intellectual realm.

As far as material things go, the first obvious thing that has come out of science is that it has given us the power to do a lot of things that we should not. There has always been that power, but the scope of it is so different that it is really a change in the world. We know how to do things. The exercise of this power will produce evil, evil so evil that we do not have to argue about it. The knowledge is with us; it cannot be lost; it leads to power; the exercise of that power is disaster.

The first example--it is first perhaps because it is most prominent, it is certainly first for me because of my closeness to it--has to do with new weapons, atomic weapons if you will. It does not make any problem for a man who has always believed in non-violence that there should be these new weapons because he has always said "do not use force to oppose evil;" but most of us have not taken that line. I cannot, I think there are times when you cannot cope with evil except by fighting it. Wisdom may reduce such occasions, prudence may help you to avoid them, largeness of spirit may tell you that you are not really in such a situation, but there are times when a man or a people cannot accept something that is bad without opposing it. But coming as it does after the debauch of two total wars in this century--wars which had the character of wars of extermination of an enemy--and coming as it does at a time when we are faced with something remarkably like an enemy who is tough and extremely unconciliatory and very dangerous, the advent of these machines raises problems that are new in scope; whether it is true today or will not be true for two or three years, whether it is true in the most literal sense or only in a figurative sense, these machines are too dangerous to use in full. I mean that there will be more explosive weapons in the possession of the various countries than man can stand, either in his political and human and civilized life or perhaps in his life organically as a species; one of the things that has come about is that we have powers that we simply must find some way not to use. We must find some way to make the means and the end proportionate to each other, some way to use, if we have to, force, but not force out of all proportion to any thinkable human end. This is not an easy thing; it is made a little more grim when we think about how we come to make decisions and policies.

I would not speak so of this if I thought it were the only example. There are, of course, other kinds of things. There are powers that come to us from science where we actually do not know enough to know whether we do good or harm. Let me give an example. An example that is much in the public eye is the possible long-range harmful effects of radiation on human heredity. We do not know how grave these effects are. All we know is that day after day we do things just as damaging as what we are now doing with radiation. We know that we do comparable damage, maybe more serious, when we drink coffee. We know that when we use antibiotics we will produce changes in the flora that attack and decimate us, which may thus become really much more difficult to control and live with than the things we started with. But these are cases where the primary requirement is to find out more. They are not cases where the primary requirement lies in the ethical field of decision taking.

But these latter problems will get worse and not better, I think. They will get worse because we will know more. Psychology is an incredibly young science; it has been approached all kinds of different ways, some of which do not make much sense. But there is a little bit of light; and as the years pass and people get into it, they are going to know a great deal more about why men think as they do, feel as they do and act as they do. And with that knowledge will come a much higher degree of certitude about how to cause other people to do one thing or another. This is going to raise problems of decision which to me seem even more fearsome than those of the atomic bomb; and it is perhaps good that we have something innocent like the atomic bomb to practice our wisdom on.

You see the beginnings of it in propaganda which is the marriage of a very crude lore about how people behave, very inaccurate, very ill-founded with almost no theory, a sort of mid-wives tale of how people behave--with the devices of communication which technology has made available. And propaganda even today is a pretty miserable thing. It is a terrible thing in the communist countries; and there are moments when it is not such a good thing in this country either. But that is based on very little knowledge and as knowledge increases, power will increase, and with it the largeness of the decisions which men are called on to take.

To whom are all these powers given? I do not know. I think perhaps the best answer is after all that they are given to no one in particular. It is, of course, formally true that power is centralized in many cases in the head of a government. The President by statute has as his possession--so to speak in his jewel box--all atomic weapons, and unless he says something about them, nothing can happen. I do not know what the arrangements are in Russia, but surely the few men who as a committee at the moment rule that country must have a similar right, and the British Cabinet does. But it is not so simple. We know that what the President does is influenced by what we think. We know that the President is influenced by the extent to which he is advised and how he is advised. We know that as a good President he is sure to be responsive to the heart and mind of this country. We do not know that about Russia. But we also know that at least the rulers of Russia are dependent on good competent technical advice. We know that people in other lands that do not at the moment have these objects can do things which limit very much our own freedom to act. We remember the assassin who murdered the Austrian archduke in Sarajevo and all that came of that.

In this country we have always believed that the only answer to political power was dispersion, balance, variety. We have always believed in a multiplicity of instruments of power in states, communities, agencies of the Federal government, balancing each other, limited in powers. We have always believed really in the virtue

of the community and its free association to create a new political agency and a new source of influence if it cared enough about something to do so. We have always believed in the value of respecting what a minority feels strongly enough. And this has got us through a sight of troubles, not without terrible grief; but it is surely the most moderate and the most hopeful and the most imaginative political system. And for us it is the right one. We hope it will be for the whole world.

But it is, of course, a little bit fluid and a little bit hard for a man who sits in his office or who teaches at Exeter or for a boy who is a student here to know exactly how he should bring influence to bear, that we do the right thing in a crisis and not the wrong. You vote for the lesser of two evils; but that is not very much either. I remember my own experiences in trying to bring to bear the best knowledge I had on the decisions of government. This was one of the many committees, in 1952; we were thinking about--we actually finally wrote something about--armaments and policy. We worked very hard at it. While we were in the middle of it, we ran into a friend who said he remembered a story and he thought maybe that story would serve as a parable for this committee. It was the story of the depression years, 20 years ago, of a man who came into a Western Union office in New York. He was poorly dressed. He was not very clean and he was not well shaven and maybe he was not even entirely sober; and he started writing on these Western Union blanks a long, long telegram addressed to San Francisco. He tore off blank after blank and finally he signed his name and called the girl over to send it. She took it and started counting it up with the tip of her pencil the way they do; and he fished in his pocket and pulled out what appeared to be his last few dollars. And when she came back she told him the price and she looked at the amount of money he piled on the counter and she began to look very grieved because he did not have more than about a third of what it cost. He said, "Missy, Missy, don't you worry. You just send it as far as it will go." There is a great deal of this in our political life. We just send things as far as they can go.

There have been changes in our political world from the time when the Constitution was drafted, changes to which we have been only in part responsive. One is that technology itself calls into being really gigantic organizations, which have to have a certain stability. One is that, because of the complexity of technical things, competence and expertness are vested in people who have not and who probably should not have authority, and people in authority are ignorant--and not always adequately aware of how ignorant--of the very technical things on which their decisions have to rest.

And the third, which is perhaps the decisive thing, is that the system of government we have, the system of living together that we have, is meant to work better on the whole than any absolute system. It is meant to be less prone to error, more capable of correcting error, more capable of bringing the good sense and wisdom of people as a whole to bear on what we are up against. I am sure it is. I think the more we go on and see how well, despite the difficulty of the problems we have, we adapt to changing situations and on the whole what wise and sensible behaviour we, as a people, have shown, the more we are clear about that. But the trouble is that in the present situation it is not enough to be right 99% of the time. The other per cent is important too. And that is a very new dimension of our problem and one for which no political system could be perfectly designed.

And the third thing that science has brought us that was not planned comes really from how successful it has been. It is a terribly specialized business today

and men who in one field get deep into it, devote their life to it, love it, make maybe some great discoveries in it will really not know too much of what is going on in another. The words, the ideas, the experiences, let us say of the biologist, the geneticist, the physicist, of the astronomer, of the historian, the psychologist--these are largely separate languages; they are separate lives; they are separate experiences and separate worlds--their thoughts and ways of thinking--that come out of common sense and common life like the fingers out of a hand, and there is nothing much between the tips of them. And that means that our community is broken up quite a lot. It is not a single, open transparent community in which everybody really has a pretty good idea about everything. There is a community in which some people have a very good idea about some things and they interlock with others who have a good idea about some others and it has the structure of an immensely complicated network, but not the structure of a nice ordered world in which you can find about anything you want; you may, and the great thing about freedom is that that is always open to you; you should, and the great thing about virtue is that you occasionally do; but you cannot find out about everything. That is the meaning of mortality. You have not time enough, or strength enough; and if you try you will have a smattering of, and not the knowledge which has some close, deep, intimate, honest, technical familiarity with what it is all about.

The very complexity of the world has taken us farther and farther from the classical Athens, where the citizens, though they differed with each other, though they gossiped about each other, were yet all in the same world, farther and farther from the 13th century, when all of Western Europe was united in a simple Catholic order, when the same words, the same symbols, the same language united from one end of the land to the other and when everything could be referred to a single view of the world.

The rate of accumulation of knowledge is just indescribable. I will stick my neck out and say that in the sciences, speaking broadly, in any 10-year period, more is found out than was known in all the past. You may say "how do I know how much is found out, how much is known? How do I measure this?" And I could answer that you are well advised to caution me. But I mean more in terms of what it takes to teach, more in terms of the number of pages it takes to write it down, more in terms of essential knowledge, not in trivial and superficial knowledge which the teachers and popularizers can get rid of, but more in terms of things that anyone who understands would recognize as central and vital. To double knowledge every ten years is to create quite a problem; I have no doubt that part of what I have been talking about and listening to these last days is the consequence of that problem. There is a heck of a lot more to teach and a heck of a lot more to learn; although the life span has been growing longer, it has not grown that much longer. And you see this is an important thing for other reasons. We love novelty and we love new things. But we appreciate, we understand them and we balance them and we make something of them in terms of familiar experiences, in terms of the old ideas. We learn in a funny way as a kind of growth and we can not learn great gobs of novelty. We have to learn a little bit that is new and see how it fits in and change our ideas and get a better understanding; learning itself is a process with a natural human rate, a natural human pattern. It comes to us very fast today. The change in our intellectual climate, the change in material climate from all the gadgetry and all new machines are both such that continuity with tradition, solidness in our ideas, and firmness are not so easy to come by. Science has fragmented and science has tended to disorient and disorganize the intellectual firmness of the people who make up this, our country, and this, our world.

These then are three points. I want to say what they mean. I think these changes are here to stay, because I cannot imagine anything other than a disaster that will stop the accumulation of knowledge, that will alter the fluid and free character of our political institutions or that will make us willing, either to act in desperation or to accept evil without a struggle. I think that we shall not really have as individual men or as communities of men total knowledge of the essential things that are known to man. We will always have this trait of ignorance and partialness. We shall not have political arrangements which can be proof against disaster; and the disaster will be quite without measure and limit.

In important ways this is not a new situation at all. In some ways, living at the edge of trouble has always been how it is for men. You cannot read the scriptures without seeing that; and there is very little of the major secular literature of our culture that is unaware of it. But it has been partly forgotten in the last century that this is our condition, that we have power but it is not enough, that we have knowledge but it is not enough, that we have freedom but it is not enough, that this is what we are born to and what we have to make do with. And this is what makes us men and gives us the privilege, the duty to try to help one another.

Now, I have not spoken of one thing; perhaps I would not but for the mistakes I have made. This is the question that has come to me so often, usually in the not very precise form, what about science and religion. But it has been an earnest question, and when we pursued it I have not been disposed to treat it lightly.

I think it is clear that the world I have described, that these traits which seem to me rather notable in it, that this is a world compatible with living in it as a Christian. Indeed I think it is a world that rather obviously calls for the great Christian virtues, its ideas, sensibility, its precepts, its practices. It is clearly a world that also calls for those traits which we associate with the Stoa, with stoicism, which has in Europe for two millennia become blended with Christian faith and Christian manners.

It is clear to me that for as long as we can see in this world there will be living Buddhists, Christians, Hindus, agnostics, perhaps in increasing number. It is clear that they will surely continue to strive to find some harmony between their tradition, their revealed knowledge--their revelation, their reason and their experience. The only thing that I think is not compatible with the world that I have described is that all of these: revelation, reason and experience be combined into a single, all-embracing unit, a monistic hierarchy and structure of certitude. I do not say that with joy, but because I have to say it.

This is what I should have said earlier to you. I am repentant that I did not. I am at the end of my time. I may have spoken a little sadly; but I do not have the feeling that this is bad news. I have the feeling that there is only one true danger, and that is to go into our life or through it without understanding what we are up against, what is asked of us and by what we can reasonably be judged.

I want to end by repeating what I said: even talking to you in this lecture, I have enjoyed being with you; and I shall continue to; I am very grateful for being asked.

EDWARD R. MURROW

This is "See It Now", edited and produced by the partnership of Murrow and Friendly, presented by the Aluminum Company of America. This is the computing machine at the Institute for Advanced Study, Princeton, New Jersey. It is virtually the only piece of scientific equipment at this Institute. There is no student body here ... just chalk and blackboard and books and scholars ... less than one hundred of them in all. Einstein, Bohr, Oppenheimer, Merritt, Woodward, Thompson, Lowe, Hetty Goldman, Kantorowicz, Beloff. In the past, the Institute has contributed much to the security and knowledge of this Republic. The men of this Institute probe far beyond the existing frontier of mans knowledge. This is a brief report on the work and purpose of the Institute as seen through the eyes and mind of one man... it's Director, Dr. J. Robert Oppenheimer ... a physicist. So that this conversation may be conducted without interruption, we ask your attention now to this report from our sponsors ... the Aluminum Company of America ...

COMMERCIAL

DR. J. ROBERT OPPENHEIMER -- EDWARD R. MURROW

MURROW

Dr. Oppenheimer, why don't we begin by your telling me a little about this Institute for Advanced Study -- how it began?

OPPENHEIMER

Well, I - I will try. Of course, it began at the time when I wasn't anywhere near; and it's already a subject for historical research. I -- I'm about to find someone to see if he can find out how it began.

MURROW

I have heard you describe it as a "decompression chamber."

OPPENHEIMER

Well, it is for many people. There are no telephones ringing, and you don't have to go to committee meetings, and you don't have to meet classes and the -- it's especially for the few people who are here for life - the first years are quite - quite remarkable, because most people depend on being interrupted in order to live. The work is so hard and failure is - is of course, I guess, an inevitable condition of success. So they're used to having - to having to go - to attend to other people's business. When they get here, there is nothing of that, and they - they can't run away. It's to help men who are creative and deep and active and struggling scholars and scientists to get the job done that it is their destiny to do. This is a big order, and we take a corner of it. We do the best we can.

(MORE)

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OPPENHEIMER (CTD)

We - we suffer from limits of money, of wisdom, of space, and we know that if we get big, we - we will spoil everything. Because the kind of itimacy, the kind of understanding, the kind of comradeship that is possible in a place of this size is hard to maintain in a place ten times as big. But we are here as an institution. I don't mean in our individual capactiies, but as an institution, we are here to take away from men the cares, the pleasures that are their normal excuse for not following the rugged road of their own - own life and need and destiny

MURROW

Well, would - would you tell me something about the permanent members of the staff - the faculty?

OPPENHEIMER

Some are pure mathematicians. This was the first and I guess the most illustrious field in which the Institute entered, and there's Hassler Whitney, who is a man who spent most of his life at Harvard, an extraordinarily imaginative and creative man, whose field -- it won't mean much - whose field is essentially Topology.

MURROW

What exactly is that?

OPPENHEIMER

Well, it is a study of those relationships which do not have to do with measure and size but with shape and arrangement. Let me digress. We had here this year a Swiss - a French Swiss psychologist. He's almost a philosopher - called Piaget - whose work has been on the way children learn to think - how they learn notions of cause - notions of time - notions of necessity - notions of number -- all the things that Kant thought you were born with.

OPPENHEIMER (CONT'D)

Well they -- now you are not born with them. You learn them - and he made one discovery which is not surprising, but a little odd. He found out those things which in normal mathematical instructions are the most highbrow, are the things the children know first. Children know first whether objects are inside each other or separate - whether they can be deformed into each other, and these are the notions that Topology deals with. They learn later that the length of a line is so, or that a figure has seven sides and not six -- all the things that have to do with number come later, but the things that have to do with relatedness -- the fact that a doughnut can't be turned into a sphere without tearing it - you see. This kind of thing the child knows, and these - these logical notions - I mentioned only trivial ones - are the basis of Topology. Then -- then there are two men - one - one a Norwegian and one a Swede, but there is no conspiracy here - who are - who are analysts and have done very great work in analytic number theory. Selberg and Buerling. There is Marston Morse, who's a very distinguished and - and large mathematician - worked in many different fields, and I hate to say it, he is almost a "statesman of mathematics". There are - there's Deane Montgomery, who solved a famous problem of Riemann which I'm not going to define, and astonished us all a few years ago. There are two physicists, and there will shortly, I think, be more - both young - one a Dutchman, Pais, and one an Englishman, Dyson - both of whom in quite different ways are struggling with the problems that interest me.

(MORE) ta

OPPENHEIMER (CTD)

There's Panofsky, who is a historian of art. He has two kids - two boys - both physicists, and they were very, very bright boys, and one of them is first in his class at Princeton. The other is second. They call one the "bright" Panofsky and the other the "dumb" Panofsky. And they used to tease him ... they said, but of course they didn't know how good they were - if they were good, they would be physicists, but they could always fall back on the history of art if they were failures. There is a - an old friend of mine and a man I think of as quite a great man, really. That's Harold Cherniss, who's that wonderful blend of scholar and philosopher that you don't find very often. His field is Greek. His passion, Plato and Aristotle.

MURROW

And Professor Einstein is still here, too, isn't he?

OPPENHEIMER

Oh, indeed he is. Indeed he is. He's - he's one of the most lovable of men.

MURROW

Does he ever call you up on the telephone?

OPPENHEIMER

Sometimes. I think he calls me when he reads in the newspapers something about me that he doesn't like and he calls me up and says: "That's all right. That's just right."

fad

MURROW:

Niels Bohr is here now, as well?

OPPENHEIMER:

Oh, we have many many people visiting; and now I will tell about those that are exciting. I mentioned Piaget - to me - but there are many, many -- too many to talk about. Bohr is here, he's come over and over again, and we have with him an arrangement which we reserve for our best friends; namely, he can come whenever he wants -- and he spent this semester and will be leaving just in a few days. We have George Kennan here. He's been here off and on, also, and has an arrangement not so unlike that of Bohr's. He's looking at a very odd episode (maybe - maybe tragic) and that is what the Americans were doing at the time of the Russian Revolution and in the year that followed that. Who was representing us, and how he got to represent us in Russia. What they thought - what they thought we ought to do - what our government did - and this is going to be quite a yarn when he gets it finished.

MURROW:

Well, what about the supply of scientists - the manpower pool - the training of the younger scientists?

ta

I don't know. You see - that - that always is a very - I am very bad about it, because I work in my own life with rather few people as student. I always have. They wouldn't let me at undergraduates because they are afraid I'd confuse them. I'd have a dozen or twenty men that I work with intimately. This Institute has a hundred people. I think in terms of the - of the rather good people rather than the number, but there's a lot of truth to that - a lot that's right about it, I mean. We - we are beginning to catch up on the mechanical side. We're just beginning to realize that if we want people to do very hard things, we've got and want them to do, irrespective of whether they are rich or poor, we've got to provide money in some way to see them through their training; if we want them to go into a profession which will never be very remunerative, we won't be successful unless we provide scholarships and that kind of thing. The training is "Operation Bootstrap." You -- one great teacher - one great man of learning changes a country. Denmark has one enormous figure in physics, and the whole country is altered by that. Japan has Yukawa who was the first Japanese to come to the Institute after the war and who was sent here with Mac Arthur's blessings - a sign of the new relations between the countries, and now all the bright young Japanese want to learn theoretical physics - not all -- but I mean - I exaggerate. So, I think that the statistics and the tables maybe don't tell the whole story.

fad

MURROW

Tell me, is there a very widespread reluctance on the part of scientists in this country to work for the government?

OPPENHEIMER

No, I don't think so. This also gets very much distorted when it's - when it's talked about in sloganistic terms. See, if you take a scientist who's excited by and interested in new discovery, he may have a problem as to whether he wants to do applied science - and for the government, that's what he would be doing -- and that's a - that's a legitimate doubt, and if all the scientists in the country did applied science, it would be terrible for us. I think that scientists like to be called in and asked to advise on how to make the Voice of America a better thing. They like to be called in and asked for their counsel. Everybody likes to be treated as though he knew something. I suppose that - that when the government behaves badly in a field you are working close to, and when decisions that look cowardly or vindictive or shortsighted or mean are made, and that's very close to your area, then you get discouraged and you may -- may -- you may recite George Herbert's poem - "I Will Abroad". But I think that's human rather than scientific.

MURROW

Dr. Oppenheimer, are you worried about - the - all the impediments placed in the way of free intercourse, travel and exchange among scientists? I think ...

fad

OPPENHEIMER

Very much.

MURROW

I've been coming down here, and I think this is true - that had the Mc Carran Act been enforced, neither Fermi nor Szilard would ever have been permitted to enter the country which would have been a rather expensive loss, I think.

OPPENHEIMER

Perhaps not even Einstein, I don't know. This is terrible. This is just terrible, and seems a wholly fantastic and grotesque way to - to meet the threat of espionage -- just an enormous apparatus, surely not well designed, for that, and terrible - for, for those of us who live with it. We are rightly ashamed by the contempt that the Europeans have for us, and we are rightly embarrassed that we can't hold congresses in this country -- that we can't - often don't let people go to congresses who are the most wanted. Year after year, we've met in Rochester to discuss the kind of basic physics that... There is one man who's the world's greatest in this, and very, very good. Oh, well, he sends his deputies and his representatives, but he doesn't come, and that's not one situation. It's over and over again. This is a scandal.

MURROW

Well, sir, apart from running the Institute, what do you do here?

OPPENHEIMER

I do two kinds of things. One is - is to write about what I think I know, hoping that it will be understandable to the public in general, and one is to try to understand physics and talk and work with the physicists and sometimes have a -- try to have an idea that may be helpful. And the part of physics that -- (I tried not to become too ignorant of any part) but the part that I -- I really get excited about is just what is called particle physics or atomic physics in its modern sense. About why are things the way they are -- What it is that's conserved, maintained, kept invariant in these fundamental particles and -- I've got a scheme here and the point is, that from this you can get some notion of whether it really fits with the particles that are found. Here, charge is - over this way. This is negative particles - neutral - doubly charged positive and positive. And this is the neutron and the proton, and up this way is mass. And we don't have this mass scale very well, but just - just as - as a schematic thing, and each of these lines then represents a - a particle that ought to live a long time according to - to - to this scheme. And this is the most famous. This was found - it's well defined energy and it's been observed very many times. It gives a kind of a V in the cloud chamber. The point of the V goes up to the - to the nuclear explosion that created it.

fad

MURROW

Tell me, Dr. Oppenheimer, do you ever become frightened at what you are finding out here in this area that can't be measured in either time or space?

OPPENHEIMER

I -- you see that's a real point. I only get frightened when - and it happens very rarely I think - I have an idea. That is what people find isn't frightening but the understanding of it sometimes has this quality. I remember a man who was my teacher in Goettingen, who is in Chicago now. James Franck. He said, "The only way I can tell whether my thoughts are -- really have some weight to them is the sense of terror when I think of something new." Well so, the kind of order that will come out of this -- that is frightening, but the fact that this is like walking through the woods and seeing that there are different kinds of flowers and it's all very amusing -- and so it is when you try to see why there is necessity to it -- why it is this way and no other way -- because all you can do here is guess in the night and correct in the daytime, and you have to try to find the mistakes.

MURROW

Is it true that humans have already discovered a method of destroying humanity?

OPPENHEIMER

Well, I suppose that really has always been true. You could always beat everybody to death. You mean to do it by inadvertence?

fad

MURROW

Yes.

OPPENHEIMER

Not quite. Not quite. You can certainly destroy enough of humanity so that only the greatest act of faith can persuade you that what's left will be human. This is -- this is a matter on which much, much, much, much more should be known. There's every reason for us to say what we -- what we know, and above all, to say what we don't know. The genetic problems -- the problems of what - what might happen in the future to the human species as a result of having radio-activity in the body or having radiation outside -- the geneticists don't know enough to be sure of that. We do know what happens if you're near an explosion -- a bomb explosion. We know it from experience and from common sense. The -- the things we know ought to be in the public domain so people are fearful only in the measure in which fear is justified and -- and rational. The things that aren't known should be talked about because one of the ways to get things found out is -- is to have it clear that we don't know the answers, and also one of the ways to -- to give people the kind of responsibility and humanity which we would like or think we have is that they recognize when they don't know something and take their very ignorance into account in their planning. This is too long an answer to your question but I feel very .....

fad

MURROW

It certainly is not and it brings me to another one that I wanted to ask very much and that is that in this era that is more frightened and dominated than any previous one by scientists, their decisions, their discoveries -- what about the poor uninformed civilian?

OPPENHEIMER

It isn't the layman that's ignorant. It's everybody that's ignorant. The scientist may know a little patch of something and if he's a humane and intelligent and curious guy, he'll know a few spots from other people's work. He may even be able to read a book. But - but his condition is a condition of everyone - which is that almost everything that's known to man he doesn't know anything about at all, or knows it only in a very sketchy way. And that's because it's - it's gotten a bit complicated. The problem of - of a coherent civilization is the problem of -- of living with ignorance and not - not being frustrated by it; so that you find occasionally a man who knows two things and that intersection may be a great event in the history of ideas. Occasionally, a man may think that something is relevant or exciting which no one before thought concerned him professionally. That may change the history of the world. And these are the connections, these virtual connections, these casual and occasional connections which make the only kind of coherence we have -- that and affection -- that and respect - that and I suppose a kind of humanity.

(MORE)

fad

OPPENHEIMER (CTD)

Now, if you look at the problem of science in government, then of course, it's possibly not really science. It's -- it's pretty much practical application because no one in the government of the United States needs to worry about whether the isotopic spin or the strangeness number are the real invariants of this sub-atomic work. They need to know, sometimes things that are very hard to answer. Can a ballistic rocket enter the - the earth's atmosphere if it's gone a few thousand miles and have a skin that isn't burned up? And so on. They need to know - is there any limit to the size of explosions you can make. They need to know all kinds of technical things. And these are not in the narrow sense the frontier of science but they are technical and complicated. Some will understand one another in one area -- some in another - and you get a kind of lacework of coherence. And that requires - I used the word "affection" before. For the government it might be better to say "trust". Though I think even for the government "affection" wouldn't be a hopeless word. Take in the government itself which consists of life. Take the President or his Secretary of State or his Secretary of Defense. All he can do is to be sure that in one way or another the advice he's being given is subject to criticism. If he gets a statement of how it is --this will cost so many dollars and take so many years -- this is impossible -- that anyone who has a different view and the professional qualifications that make it interesting, can get to him.

-more-

kk

OPPENHEIMER (CTD)

And I think that isn't too bad in the parts of the government I have been close to. That is the atom and military establishment and the State Department. I think -- I think people have been able to -- to tell their stories and that -- that the folly has been corrected. But if you mean the people outside the government ...

MURROW

Yes.

OPPENHEIMER

And I think that's more important. There - there - there is a - a really a point that I feel most deeply and I think I speak really now the voice of - of my profession -- and that is the integrity of communication. The trouble with secrecy isn't that it inhibits science - it could - but in this country it's hardly been used that way. Technical things are - are really quite widely known and those at the growing tip of - of any science are so far from practice that the - the people talk quite freely about them and should. The trouble with secrecy isn't that it doesn't give the public a sense of participation. The trouble with secrecy is that it denies to the government itself the wisdom and the resources of the whole community - of the whole country, and the only way you can do this is to let almost anyone say what he thinks - to try to give the best synopses, the best popularizations, the best mediations of technical things that you can, and to let men deny what they think is false -- argue what they think is false, you have to have a free and uncorrupted communication.

OPPENHEIMER (CTD)

And this is -- this is so the heart of living in a complicated technological world - it is so the heart of freedom that that is why we are all the time saying -- "Does this really have to be secret?" "Couldn't you say more about that?" "Are we really acting in a wise way?" Not because we enjoy chattering -- not because we are not aware of the dangers of the world we live in, but because these dangers cannot be met in any other way.

MURROW

Well, if I may say so, I think you were speaking there not only for your profession but for mine - if it is a profession.

OPPENHEIMER

I -- I'm sure of that.

MURROW

There aren't in fact very many secrets?

OPPENHEIMER

There aren't secrets about the world of nature. There are secrets about the thoughts and intentions of men. Sometimes they are secret because a man doesn't like to know what he's up to if he can avoid it.

MURROW

Dr. Oppenheimer, if enough nations get messing about with hydrogen bombs, is there any danger of contaminating the atmosphere -- of the fall-out being so heavy that we may damage ourselves without meaning to?

OPPENHEIMER

I'm not unworried about it. I tend still to worry about war rather than peace, I think -- I think the scale of things in these experimental undertakings is -- is so vastly smaller and their location so much more secure than -- than what you'd expect if -- if the battle were joined, that we do well to worry about the latter before the former.

MURROW

That was a brief glimpse of a two and a half hour conversation with Dr. J. Robert Oppenheimer, Director of the Institute for Advanced Study. One thing that impressed this reporter at that Institute was that he never heard so many people say -- I don't know. These men recognize mystery. They welcome it and they wrestle with it. Good night and good luck.

ANNOUNCER

A one hour version of this conversation with Dr. Oppenheimer will be made available to any college or university requesting it by the Fund for the Republic.

gj

Harper's MAGAZINE

We Accuse!

Joseph and Stewart Alsop

Many Americans—some of them private citizens, some in high office, and some whose official responsibility it was—have pronounced judgment on Dr. J. Robert Oppenheimer's fitness to have access to the scientific secrets on which the national security so largely depends. Many accusations were leveled against Dr. Oppenheimer in the course of this debate, and the formal findings of the Atomic Energy Commission against him were lengthily published. Among those who did not accept these findings, however, were the widely read and respected journalists, Joseph and Stewart Alsop, who here in turn accuse the accusers. Their conclusions are presented, not merely to answer charge with countercharge, but to round out the record on the critically important issues which the Oppenheimer Case has raised but by no means resolved.

—The Editors

I. Rise and Fall

THE title of this report is borrowed from Emile Zola, whose *J'Accuse* marked the turning point in the case of Captain Dreyfus. It is a proud title, for it is still the symbol of one of our era's rare triumphs of the liberal spirit over organized injustice. It is a title, indeed, that one must be presumptuous to borrow; and we only dare to do so because we too accuse.

We accuse the Atomic Energy Commission in particular, and the American government in general, of a shocking miscarriage of justice in the case of Dr. J. Robert Oppenheimer.

We accuse Oppenheimer's chief judge, the chairman of the Atomic Energy Commission, Admiral Lewis Strauss, and certain of Oppenheimer's accusers, of venting the bitterness of old disputes through the security system of this country.

And we accuse the security system itself, as being subject to this kind of ugliness, and as inherently repugnant in its present standards and procedures to every high tradition of the American past.

These are grave accusations, which must be factually supported. As to whether they are justified, the reader must decide. And so, without further ado, let us begin the re-trial by calling the defendant to the bar; for the first requirement for an understanding of the Oppenheimer case is an understanding of Oppenheimer himself.

J. Robert Oppenheimer was born in New York City in 1904, into a prosperous, cultivated, and liberal Jewish family. The father, an immigrant from Germany, was a successful businessman and a discriminating collector of modern pictures. The whole household was imbued with the rabbinical respect for the things of the mind, and with

the hope of progress made all the sweeter by the memory of dark things left behind, that so often distinguished Jews of their sort in that simpler and better time.

Characteristically, Oppenheimer's education began at the Ethical Culture School—where else would such a family send a promising son? It continued at Harvard, where Oppenheimer first showed promise as a physicist and graduated in 1925; and at Lord Rutherford's great laboratory in Cambridge and at Göttingen, where he took his doctorate in 1927. There were two more years of preparatory study, largely abroad. And then, in the spring of 1929, young Oppenheimer came back to America, to accept a double teaching assignment at the California Institute of Technology at Pasadena and the University of California at Berkeley.

It is curious, and in the light of subsequent events it is highly ironical, that this return of an utterly obscure twenty-five-year-old teacher should have proved a significant event in the American story. Yet such it was.

THE truth is that in a quite surprising degree, Oppenheimer was the bringer of a revelation. Long before his graduation from Harvard, he had descried from afar the revolution in thought that is the New Physics. But in this he was exceptional; even in 1929, when he came home for good, the American universities had only begun to grasp the cosmos-shaking advances of physical knowledge that had been achieved in Europe.

At Berkeley, Oppenheimer started with one graduate student; but around this slender, intense young man, all but hieratic in his dedication to his mystery, there rapidly grew up "the largest school in the country, of graduate and post-doctoral study in theoretical physics." He was the center, and each year, when his teaching term at Berkeley ended, many of his students followed him to Pasadena to be with him while he fulfilled his contract with Caltech. If the vast majority of American physicists today quite genuinely venerate Robert Oppenheimer, it is because he trained so many of them, and because the rest rightly regard him as the man who took the lead in naturalizing the New Physics in this country.

In those first years of great accomplishment, Oppenheimer was almost ludicrously—and perhaps somewhat self-consciously—remote from the mundane realities of the American scene. He took no newspaper. He owned no radio. The tumultuous political events of the early 1930s simply escaped his notice. For distraction he learned Sanskrit, because he wanted to read the *Bhagavad-Gita* in the original. For the rest, he lived his life in the rarefied upper air of abstract physical speculation.

THUS he was dangerously innocent—he had no better standards of practical political judgment than a visiting Martian—when the world around him at last began to attract his attention.

Partly he came down from his mountaintop because of his long and unhappy engagement to Jean Tatlock, the daughter of a fellow professor. Partly his interest in politics was stimulated by Hitler's persecution of the Jews in Germany, where he still had relatives whom he helped later to escape. Partly, too, he was caught up in the wave of emotion about the Spanish Civil War which then engulfed so many intellectuals. The Communist party was brilliantly exploiting both the Falange and the Nazis, to attract great numbers of men like Oppenheimer. Jean Tatlock was one of the generous but troubled spirits who were always joining and leaving the Communist party in those deceptive years of the united front. Not very surprisingly therefore, Oppenheimer became an active fellow-traveler.

He joined front organizations. He attended meetings. Since he had a private fortune, he contributed fairly regularly to the party's Spanish war and Spanish relief funds; and, since this was the fashion for the larger fellow-traveling contributors, he made these contributions through party officials. In short, he freely indulged in the brand of political folly that was then a common highbrow reaction to the menace of Nazism and Fascism. One thing, however, Robert Oppenheimer never did. Despite his fashionable folly, despite the easy emotionalism that was his first response to politics, Oppenheimer never took the final step of joining the Communist party.

By 1939, his fellow-traveling enthusiasm had cooled off markedly, although he continued to contribute through party channels to Spanish relief. In wartime, he

eschewed politics altogether. Finally, at the end of the war, the true meaning of the Soviet-Communist system came home to him with great force, and rather earlier than to many other leading Americans—Dwight D. Eisenhower, for example, was still talking eupeptically about how easy it was to "get on with Zhukov" as late as 1947.

After his awakening, the follies of Oppenheimer's past were utterly left behind. Former President Conant of Harvard, General Frederick Osborn, and many others who worked with him closely have testified as to the "hard-headedness" and strongly "anti-Soviet" character of Oppenheimer's political attitude throughout the postwar years. As time passed, indeed, Oppenheimer became the only truly eminent American outside the armed services—so far as these reporters are aware—who was willing to discuss dispassionately the idea of preventive war to save the world from Communist tyranny. On this point, too, there is rather shocked testimony, from the present chairman of the General Advisory Committee of the Atomic Commission, Dr. I. I. Rabi.

As is indicated by the gradual transition from modish breast-beating to cool, hard independence of thought, Oppenheimer's early follies were simply a disastrous phase in the difficult process of learning his way about in the everyday world. In this same process, his marriage in 1940 was also a stage, and a much happier one.

The story of Katherine Puening Oppenheimer is sad, but with a good ending. She was a pretty, serious, very young girl from a solid, prosperous, conservative family, when she rather improbably encountered Joe Dallet in 1935. Dallet was a romantic, born for a crusade, who had the half-pathetic, half-ironical misfortune to enlist under the Communist banner. She fell violently in love with Dallet; she left her family and joined the party to marry him. A little later, she proved her greater wisdom by rebelling against the party's dreary discipline, and this broke up the marriage. Yet she did not cease to love Dallet, and she had just gone to Paris to meet him again when the news reached her that he had been killed in action on the Spanish front. After that desperate moment, she half blindly blundered into another marriage. Yet she was still a very unhappy woman when she and Oppenheimer met in 1939.

Their feeling for one another was strong and irresistible. Yet he had to part with Jean Tatlock, who still loved him and made a tragic last request to see him before her death in 1943. She also had to part with her husband, and thus the Oppenheimers' marriage automatically stimulated much unkindness in the small Berkeley community. One of those who were not unkind was Haakon Chevalier, a clever, superficial teacher of romance languages, who also belonged to the West Coast group of Communist and fellow-traveling intellectuals. And this was to be important, because Oppenheimer, who is fiercely protective of his wife, was warmly grateful for Chevalier's kindness in a bad time.

Such are the main facts it is needful to know about Robert Oppenheimer, in the period before History chose him as the principal actor in a larger drama. He was not a member of the Einstein-Fermi-Szilard group of refugee physicists who first opened the eyes of the American government to the possibilities of the atom. But he joined the work soon after it began, and his value was recognized at once.

In the spring of 1942, only a few months after Pearl Harbor, Dr. Arthur Holly Compton asked Oppenheimer to recruit and lead a special scientific task force. Enrico Fermi had not yet achieved his great triumph, the famous first sustained nuclear chain reaction in the University of Chicago squash court. But Oppenheimer and his task force—which included Hans Bethe, Edward Teller, and others of like caliber—were nonetheless told to begin designing a workable atomic bomb.

Their astonishing progress led on, within a few months, to the decision to establish the great Los Alamos Laboratory. Oppenheimer had directed the work at Berkeley. He had even suggested the Los Alamos site to the new commander of the Manhattan District project, Lieutenant General Leslie R. Groves. But a question still remained, whether Oppenheimer should be chosen director of Los Alamos.

As soon as he joined the war effort, Robert Oppenheimer had filled out the usual personnel questionnaire, revealing the gen-

eral outlines of his political past. By now the memberships in front organizations, the fellow-traveling, the contributions, were all known to the appropriate authorities. So were his Communist personal associations, including the former party memberships of Katherine Oppenheimer and also of Frank and of his wife Jackie—for Frank Oppenheimer, an unhappy caricature of his brilliant elder brother, had joined the party in 1936. But General Groves had already come to know Oppenheimer rather well. He had no doubts whatever, and he still has none, about Oppenheimer's loyalty. He regarded Oppenheimer's appointment to head Los Alamos as a "calculated risk," which it was essential to take. Why this was essential has been simply explained by John J. McCloy, who represented Secretary of War Stimson in this decision. "Oppenheimer," McCloy has said, "was the only American physicist fully qualified for the job; there were plenty of refugees, of course, but everyone agreed Oppenheimer was the only American who was up to it in every way."

To complete this facet of the story, it must also be noted that Oppenheimer was by no means taken on trust after his appointment at Los Alamos. Throughout 1943, Colonel John Lansdale—a successful, conservative Cleveland lawyer who served as Los Alamos security officer—repeatedly questioned Oppenheimer, at great length and in very great detail, about all his fellow-traveling activities and Communist connections. In addition, Oppenheimer was closely watched at all times; and he was also interviewed by Colonel Boris Pash, the Manhattan District security officer at Berkeley. Colonel Pash, who saw Oppenheimer only once, always remained suspicious of him. But the able and tough-minded Colonel Lansdale had the primary responsibility. He did almost all the work on Oppenheimer and got to know Oppenheimer very intimately. And Lansdale gradually came to have an abiding faith in Oppenheimer's loyalty and discretion.

It is somehow sordid that this essentially insignificant aspect of the epic of Los Alamos should now have to be recalled. How Oppenheimer tirelessly gathered a great new scientific team, while the new laboratory buildings were rising on the sun-drenched mesa; how Los Alamos expanded until, at the end, Oppenheimer was the admired leader of 8,000 people, including 4,000 scientists and technicians; how the work proceeded relentlessly, past obstacle after obstacle, until the final blinding triumph that altered the whole shape of our world—these are the points it would be more fitting to dwell upon. The end came in the bareness of the desert at Alamogordo, when Trinity—the first of the absolute weapons—was tested with brilliant and terrible success.

From that moment, to Hiroshima, to the Medal of Merit and a high position in the councils of the American government, Robert Oppenheimer's journey was rapid and ineluctable. The bomb whose glare illuminated a new world also gave the once-obscure brotherhood of physicists a strange new standing in America. They acquired something of the position in our society of the Mathematician-Astronomer-Priests of the ancient Mayas, who were at once feared and revered as the knowers of the mystery of the seasons and the helpers of the sun and stars in their life-giving courses. Oppenheimer, the maker of the bomb, became the unofficial high priest.

In the next years, his primary public position was the chairmanship of the General Advisory Committee of the AEC; and in 1947 he also found his private niche as director of the Institute for Advanced Study at Princeton. But with all his other duties, he was constantly called upon to serve in the more general capacity of chief scientist to the American government, working on many Presidential assignments, always asked to give counsel on the big political-military-scientific problems, often consulted, indeed, as though his pronouncements had an oracular value. And although he left the General Advisory Committee in 1952, his work for the government continued, and his standing before the country remained undiminished.

Such was Oppenheimer, such were his remarkable record and great position, when President Eisenhower named a new chairman of the Atomic Energy Commission. Lewis Lichtenstein Strauss—a promoter, investment banker, and civilian-in-wartime Admiral, who had previously served as one of Truman's first AEC commissioners—moved into the AEC chairmanship on July 3,

1953. Just four days later came the first warning signal. On July 7, as Strauss proudly announced in his first press release on the Oppenheimer case, the new chairman "initiated the steps" that were to end with a heavy-handed squad of AEC security officers descending on Princeton to remove the classified documents which Oppenheimer had always been allowed to store in a specially guarded facility in his office.

Not long thereafter, events began to move with unwonted swiftness. At the beginning of November, a former member of the staff of the Joint Congressional Committee on Atomic Energy, William Liscum Borden, wrote the FBI what can only be called a *lettre de cachet* attacking Oppenheimer's loyalty.

The letter was a mishmash of the stale facts and unsupported conclusions. According to Borden, Strauss was not privy to the writing of this letter. In any case, under the established procedures, the *lettre de cachet* set the whole ponderous security machinery in motion; and Strauss leaped into the driver's seat to make the wheels turn faster.

IT WAS Strauss who went to the President without consulting his colleagues, and came back with the dramatically phrased order putting a "blank wall" between Oppenheimer and all classified data. It was Strauss who directed the preparation of the harshest possible statement of charges; Strauss who called the still unsuspecting Oppenheimer to Washington to notify him that his AEC clearance was suspended; Strauss who hastened on the trial of the case. It was Strauss or his underling, AEC General Manager K. D. Nichols, who forbade the hearings to be held in New York, thus effectively preventing the distinguished but no longer young John W. Davis from appearing as Oppenheimer's counsel. And it was Strauss who decided that the AEC counsel should be Roger Robb, a man best known as the lawyer for Senator Joseph R. McCarthy's chief journalistic incense-swinger, Fulton Lewis, Jr.

In April of this year, the long hearings began before a special board composed of the Chancellor of North Carolina University and former Secretary of the Army, Gordon Gray; the former head of the Sperry Gyroscope Company, Thomas A. Morgan; and the well-known chemist, Dr. Ward Evans, of Loyola University.

In late May came the Gray board findings. Gordon Gray and Thomas Morgan decided that Oppenheimer was a security risk; but almost in the same breath they pronounced him devotedly loyal, unusually discreet, and a public servant whose contribution could never be repaid. In his dissent, Dr. Ward Evans sternly remarked that the Gray-Morgan finding would be a "black mark on the escutcheon" of the country. The nation argued the issue, and the case then went to the AEC.

FINALLY, late in June, came the Atomic Energy Commission's majority opinion, again declaring Oppenheimer a security risk. It was written—in brutal language, contrasting sharply with the reflective, regretful tone of Gordon Gray—by Admiral Lewis Strauss. It represents a curious evolution. For the Gray Board had firmly dismissed the bulk of the AEC's original charges against Oppenheimer, which had to do with his prewar associations. Gray and Morgan had then found Oppenheimer guilty primarily on one issue, that his lack of enthusiasm delayed the hydrogen bomb project. But now Strauss, in his turn, firmly dismissed this Gray-Morgan finding, stating that Oppenheimer's views about the H-bomb had not even been considered by the AEC, because he had a right to take any view he chose. By this process of elimination, all the serious charges against Oppenheimer were successively refuted or dropped, until none remained except those contained in the final opinion by Lewis Strauss. Hence this Strauss opinion is the test—the sole test—of the Oppenheimer case.

What then was the purport of this historic opinion? Strauss conspicuously failed to challenge the favorable Gray-Morgan finding, that Oppenheimer was wholly loyal and wholly discreet. Strauss mentioned dangerous associations, but this was strictly subsidiary. In bitter words, Strauss took his stand squarely on the ground that Oppenheimer suffered from "substantial defects of character." Oppenheimer was guilty, said Strauss, of persistent "falsehood, evasion, and misrepresentation"; but as proof of these vices Strauss offered only six "examples."

And what were these six proofs, that were held sufficient to convict Oppenheimer of character defects so grave as to jeopardize national security? Three of the Strauss "examples" can be grouped together and discussed together, for they are all matters with a common background. They are as follows:

First, in the course of a long interrogation in 1943, Colonel Lansdale once asked Oppenheimer a single question: "Do you know Rudy Lambert?" Oppenheimer replied with a short counter-question: "Do you know what he looks like?" That was all; and, as will be seen, it is by no means sure there was even this much. But in the Gray board hearings, it developed that Oppenheimer had indeed known Lambert, a minor Communist official; had lunched with him once or twice; and thus knew what he looked like.

Second, again in 1943, Oppenheimer told Colonel Lansdale he had heard that Joseph Weinberg, a younger physicist at Berkeley whom he did not know well, was a member of the Communist party. Lansdale did not ask Oppenheimer about Weinberg. Oppenheimer volunteered the information. Then, seven years later, in 1950, an FBI agent questioned Oppenheimer about Weinberg. On this occasion Oppenheimer said that he thought he had first learned of Weinberg's Communist affiliations when they became public knowledge, which was after 1943.

Third, again in 1943, Oppenheimer told Colonel Lansdale he had also heard that another Berkeley physicist, Giovanni Rossi Lomanitz, was a Communist. Shortly after this, Lomanitz was drafted in order to remove him from Berkeley. The head of the Berkeley laboratory, Dr. Ernest O. Lawrence, raised a great row about losing Lomanitz. Partly at Lawrence's request, Oppenheimer spoke to Lansdale about getting Lomanitz re-assigned to work at Berkeley, as one of those special risks the Manhattan District made it a policy to take in special cases. Later he wrote Lansdale, renewing the same suggestion, but adding that he "was not in a position to endorse this request in an absolute way," since he did not know the full facts about Lomanitz. Finally, after eleven years had passed, Oppenheimer was asked a surprise question at the Gray board hearing: Would he have recommended Lomanitz's re-assignment to Berkeley if he had known Lomanitz was a Communist? And he answered this question in the negative.

The first thing to note about these matters, which are solemnly presented by Admiral Strauss as final proof of habitual untruth, is the simple immensity of their context. Three incorrect answers are torn, as it were, from a vast hodgepodge of innumerable questions put to Oppenheimer by many different people—Pash, Lansdale, Groves, several FBI agents, Congressional committees, the Gray Board—and innumerable questions put, moreover, in all sorts of different conditions and at different time intervals over a period of eleven years. Only a miracle witness could have avoided minor mistakes and contradictions in these circumstances; and Oppenheimer was far from being a miracle witness about small points.

And by any reasonable standard, the three mistakes about Lambert, Weinberg, and Lomanitz were all extremely minor. In the case of the Lomanitz letter, Oppenheimer was asked to recall the forgotten background of a letter written eleven years before, and asked in a way that invited a wrong reply. In the Weinberg case, he fell into what is surely the commonest of all human errors, which is confusing the time when you have learned a long-known fact in a past already remote. As for the Lambert case, there may be no case at all, for the transcript of the Lansdale-Oppenheimer interview in 1943 is badly garbled. And if the transcript is correct, it is surely not stretching things too far to suppose that just once in all these unending interrogations, Oppenheimer was tired or muddled or inattentive, and thus gave a misleading reply to just one short question, casually put and never asked again.

One might be unwilling to make this sympathetic stretch, of course, if the record showed that Oppenheimer had any important motive for being evasive about Lambert, or changing the date of his knowledge of Weinberg's Communism, or misrepresenting the background of his letter about Lomanitz. But the record shows no trace of an important motive, and no attempt to establish any motive. Lomanitz, Weinberg, and Lambert were all men who played no serious role in Oppenheimer's life. While Oppenheimer

made mistakes about these men who meant little to him, he was exceedingly accurate —and at sore cost to his own feelings—about other persons who meant a great deal to him. Surely an intelligent man does not tell the bleak, uncomfortable truth about what is important, and then, just for fun and games, tell lies about what is unimportant. With no showing of motive, in short, these things are trifles. Yet they are one-half of Admiral Strauss's proof of Oppenheimer's habitual untruth.

THE fourth of the Admiral's examples, the so-called Peters letter, is really too silly to be worth discussing in detail. Before a Congressional committee, Oppenheimer testified somewhat intemperately about the political past of a German refugee physicist, Bernard Peters; and then, when the news leaked and Peters' job was endangered, he wrote a letter that went rather far in truing up. Admiral Strauss also went rather far to true up, in his recent Congressional testimony about his faithfulness in consulting all his commission colleagues. The motive of one was disinterested; of the other, interested. The conduct of both was human and natural under the circumstances.

Example five is also a letter; it is also silly; and it is only worth discussing in detail because of the light it throws on the climate and procedures of the Oppenheimer case.

Very briefly, there was one member absent from the historic meeting of the General Advisory Committee of the AEC, in October 1949, that unanimously recommended against an all-out program to produce the hydrogen bomb. The University of California physicist, Dr. Glenn T. Seaborg, had gone to Sweden two weeks earlier. Before leaving, he wrote Oppenheimer a long, rambling, inconclusive letter that Dr. Seaborg himself described as "having more questions than answers." Yet it contained the sentence: "Although I deplore the prospect of our country putting a tremendous effort into [the H-bomb program], I must confess that I have been unable to come to the conclusion that we should not."

Dr. Seaborg added that he doubted his letter would be helpful, that he was ready to be shown he was wrong, but that the arguments would have to be convincing. He did not ask that his letter be shown to the other members of the General Advisory Committee; and Oppenheimer probably did not interrupt the GAC's tense deliberations with Dr. Seaborg's triplication of negatives, although the GAC members are not clear on this point.

The October meeting was a long, solemn, and heart-searching discussion of one of the truly terrible scientific-strategic Rubicons of our time. In the end, James B. Conant; Enrico Fermi; Cyril Smith; President Eisenhower's personal scientific adviser, Dr. Lee DuBridge; the present chief scientific adviser to Admiral Strauss, Dr. Rabi; and the GAC's two businessmen members, Hartley Rowe and Oliver Buckley, all joined Oppenheimer in opposing a great, immediate effort to make the H-bomb, on both moral and technical grounds. Rabi and Fermi went further than the others, declaring the H-bomb "should never be made" in this country under any circumstances.

Some time after this meeting, Dr. Seaborg returned from Sweden, and was of course told what had happened. He then attended the next GAC meeting in December, long before President Truman's final decision on the H-bomb. At this meeting, when the great issue was again discussed at length, Dr. Seaborg raised no objection to the decision of his colleagues. He offered no criticism or argument. Presumably because he was still of two minds about it, he simply said that he would prefer not to express his views. A couple of months later, before the Joint Congressional Committee on Atomic Energy, Oppenheimer testified that "there was surprising unanimity" in the GAC on the H-bomb issue, but added that Dr. Seaborg "had not expressed his views."

TO UNDERSTAND how a mountain was made of this molehill, you must understand the most curious feature of the Gray board hearings. The Gray board permitted the AEC counsel to act, and Roger Robb enthusiastically acted, as an ambitious prosecutor with none of the inconvenient restraints that the courts impose on the prosecution. The Seaborg letter was scooped up by the AEC security officers when they took over Oppenheimer's classified files. Robb had the letter. Since Oppenheimer was deprived of the usual protections of a defendant in an adversary proceeding, Oppenheimer did not

have the letter, and had long ago forgotten all about it.

So Robb brought out the Oppenheimer testimony as to the GAC's surprising unanimity and Dr. Seaborg's failure to express his views. He induced Oppenheimer to point out that Seaborg was in Sweden during the October GAC meeting, and led him into saying there had been no communication with Seaborg. And then he produced Seaborg's forgotten triplication of negatives like a rabbit out of a hat. Had not Seaborg in fact expressed his views? Was this not a communication? Was there not concealment? So the questioning went.

Of course the letter was indecisive and, indeed, quite meaningless in view of the position that Seaborg took later on. Of course it was natural for Oppenheimer to forget such a letter in the intense and complex debate on the H-Bomb. Of course it was natural for Oppenheimer to remember only the key point, that Dr. Seaborg had in fact refrained from expressing his views when he had the best possible opportunity to do so. All the same, the Seaborg letter was paraded among Admiral Strauss's examples.

II. The Oppenheimer Haters

In the Oppenheimer case layer after layer of false appearances, of chaff dressed up to look like corn, of petty matters artificially inflated into serious matters, must be painstakingly got rid of before what is really serious can be reached. And even what is really serious has usually, in one way or another, been given a false appearance. There is no better illustration of these rules than the sixth famous "example" which Admiral Lewis Strauss used to prove Robert Oppenheimer's "substantial defects of character."

Among the six, this is the only example that is worthy of serious consideration. Even so, the story can be briefly told.

Shortly before Oppenheimer's final move to Los Alamos to take over the great laboratory, he and his wife received a visit at their Berkeley house from the man who had been kind to Katherine Oppenheimer in the bad time, Haakon Chevalier. When he and Oppenheimer were alone together in the kitchen, Chevalier said that George Eltenton, a West coast Communist, had "spoken to him about the possibility of transmitting technical information to the Soviet scientists." Oppenheimer replied sharply that "this sounded very wrong to him," and the matter ended there for the time being.

There were two reasons for this temporary ending. First, the modern concept of "security" was still very strange and unfamiliar in America that early in the war; and Oppenheimer at first convinced himself that he had fulfilled his obligations to "security" when he so firmly rejected Chevalier's feeler. Second, Oppenheimer did not wish to implicate his friend, since he felt indebted to him and since he believed Chevalier was acting as a mere unwitting tool for Eltenton.

Maybe Chevalier was an active Communist. Oppenheimer did not think so. In the atmosphere of those days, after all, it was rather easy to persuade a woolly-minded teacher of romance languages that it was not only right and moral to communicate technical data to the hard-pressed scientists of our gallant Soviet ally, but also that this was a fine way of frustrating the "anti-Soviet" reactionaries everyone was warming against. 1943 was the year, remember, when *Time* Magazine was criticizing the choice of Charles E. Bohlen to accompany Cordell Hull on his mission to Moscow, on the ground that Bohlen was full of stuffy prejudices against the noble Russians.

That summer at Los Alamos, however, Colonel Lansdale happened to tell Oppenheimer that the security people were worried about the activities at Berkeley of the Federation of Architects, Engineers, Chemists, and Technicians. Oppenheimer recalled that Eltenton was an officer of this left-wing union. He remembered the Chevalier incident. Under Colonel Lansdale's tutelage, he had learned a good deal about the need for security precautions. He thought the whole problem over, and when he went to Berkeley a little later, he warned the security officers there that Eltenton would bear watching. He knew, he said, that Eltenton had tried to obtain secret information.

It is not clear whether Oppenheimer was taken unawares by the next move, or whether he had decided in advance to tell a lie to shield Chevalier. At any rate, when the chief security officer, Colonel

Boris Pash, immediately asked Oppenheimer for details, Oppenheimer answered with an idiotic "cock and bull story" about how three persons, all unnamed, had been approached by Eltenton, and about microfilm, the Soviet consulate, and God knows what else. There, once again, the matter ended for the time being; for when Oppenheimer was pressed for names, he refused to give them, merely saying that Eltenton's overtures had been rebuffed.

A couple of months later, General Groves at length told Oppenheimer that he would have to order him to name names; and at this point Oppenheimer told how the approach to him had been made by Chevalier. Neither Colonel Lansdale nor General Groves seems to have been particularly shocked by Oppenheimer's behavior in this matter, and both rather made light of it before the Gray board. They did not of course seek to excuse or palliate either the delay in giving the warning about Eltenton, or the subsequent cock and bull story to protect Chevalier. But Lansdale strongly emphasized that Oppenheimer had taken the initiative to give the warning about Eltenton, going to Pash of his own volition. This, he said, was the significant point.

Of the cock-and-bull story, Groves remarked that Oppenheimer merely showed the "typical American schoolboy attitude that there is something wicked about telling on a friend." He added that he "felt [he] had gotten what [he] needed out of" Oppenheimer's final confession. And he summed up pretty effectively: "I do know this: that [Oppenheimer] was doing what he thought was essential, which was to disclose to me the dangers of this particular attempt of a potential spy to enter the project."

This is the whole of the famous Chevalier incident, together with the opinions on it of the two men who had the best reasons to be upset about it and were closest to it at the time.

It had a minor sequel, in that Oppenheimer did not absolutely break off relations with Chevalier. He still believes that Chevalier ignorantly let himself be used by Eltenton; and there was still the old sense of gratitude. Last year, when the Oppenheimers were in Paris, the Chevaliers learned of their visit from Professor Niels Bohr. They wrote asking to see the Oppenheimers. Chevalier was then working for UNESCO, which had raised the question of his clearance. He did not know whether to resume his French citizenship to keep his job, or to brave the thing out as an American; and he wanted to talk to Oppenheimer about it. The two couples lunched together one day, and the next day paid a call together on Chevalier's friend, André Malraux, hardly a left-wing association.

The best comment on this encounter was made by George F. Kennan, when Gordon Gray sought an admission that it was improper for Oppenheimer to see a former friend with Chevalier's background. "I don't like to think," said Kennan, "that people in a senior capacity in government should not be permitted or conceded maturity of judgment to know when they can see such a person or when they can't. If they come to you, sometimes I think it is impossible for you to turn them away abruptly or in a cruel way, simply because you are afraid of association with them, so long as what they are asking of you is nothing that affects your governmental work. I myself say it is a personal view on the part of Christian charity to try to be at least as decent as you can to them."

Such are the facts. It remains to be explained how these rather simple facts have been blown up, before the American public, almost to the proportions of a nightmare. The explanation is that AEC counsel Robb used the old prosecutor's trick of forcing Oppenheimer to admit, over and over again, that he had lied in his original cock and bull story to Colonel Pash. What was really a single made-up story was worked, like a mine, to produce thirteen admissions of lying. Robb's trick evidently gave Admiral Strauss just what he wanted, as one can see from the account he gives of the Chevalier incident in his AEC opinion:

> Dr. Oppenheimer has now admitted under oath that while in charge of the Los Alamos Laboratory and working on the most secret weapons development for the government, he told Colonel Pash a fabrication of lies. Colonel Pash . . . was charged with the duty of protecting the atomic-weapons project against spies. Dr. Oppenheimer told Colonel Pash in circumstantial detail of an attempt by a Soviet agent to

> obtain from him information about the work on the atom bomb. This was the Haakon Chevalier incident. In the hearings recently concluded, Dr. Oppenheimer under oath swears that the story he told Colonel Pash was a "whole tissue and fabrication of lies."

There are several things to be said about that remarkable paragraph, of which the first is that it amounts to as big and ugly an untruth as Oppenheimer ever told Colonel Pash. "This" was emphatically not "the Haakon Chevalier incident." It was only a part of the Chevalier incident, and by no means the major part. The major part was Oppenheimer's voluntary decision to give the warning about Eltenton. That was the heart of the matter, according to both Lansdale and Groves. Strauss left out the heart of the matter. He omitted every other explanatory and extenuating fact. He rejected the testimony of the two real experts on this Chevalier incident, Groves and Lansdale. And so he achieved no mere caricature of the truth, but a gross and flagrant distortion.

THERE is an ancient rule of Roman law that *suppressio veri* and *suggestio falsi*, in combination, are tantamount to a conscious lie and may be so treated by the judge on the bench. There is no known rule that covers the judge himself indulging, wholesale, in the suppression of what is relevant and true, and the suggestion of what is irrelevant and false.

One would like to pause to analyze at some length the other instances of these practices, which are liberally studded throughout Admiral Strauss's opinion. His accounts of all the other five "examples" are also biased in language, and the central, explanatory facts—showing why Oppenheimer acted as he did and putting his actions in sensible proportion—are omitted without exception.

After giving his examples, furthermore, Admiral Strauss permitted himself a bold hint that the secret and unpublished part of the record contained many other facts damaging to Oppenheimer. "The catalog does not end with these six examples," he wrote. "The work of Military Intelligence, the Federal Bureau of Investigation, and the Atomic Energy Commission—all, at one time or another, have felt the effect of his falsehoods, evasions, and misrepresentations." This statement is nailed as just not true, in the powerful dissenting opinion of AEC Commissioner Henry C. Smyth, who saw and studied every document that Strauss saw and studied.

Here, then, was an American citizen of great eminence and public usefulness, who had been lengthily tried and found to be unquestionably discreet and unquestionably loyal. And by such methods and on such evidence as we have shown, this man was publicly disgraced before his country and the world.

YET even the peculiarities of the evidence and the curiosities of its presentation do not bring us to the end of the strange story of this Strauss opinion that condemned Robert Oppenheimer as a security risk. One must also remember that Oppenheimer's security clearance had come before the AEC once before. And here we find what Admiral Strauss would probably call a "pattern," made up of three interrelated sets of facts, and pointing to a decidedly unappetizing conclusion.

First, there is the story of the clearance itself. When the FBI summary came to the AEC in the winter of 1947, preliminary clearance of Oppenheimer was voted promptly, but the commission was sufficiently concerned to defer final clearance. J. Edgar Hoover was consulted and raised a special warning flag about the Chevalier incident, saying that it was the "only thing he didn't like." Besides the summary, the FBI's full investigative file on Oppenheimer was also sent to the AEC and made available to the commissioners. This file not only gave the essential facts of the Chevalier incident; it also included an explicit admission by Oppenheimer—made to the FBI in 1946 and comparable in all but wording to the admission he made to the Gray board—that the first story he told Colonel Pash was pure fabrication. Yet in August 1947, after considering the matter four months, the AEC unanimously voted to give Oppenheimer full and final clearance.

Second, the fullness and finality of this 1947 clearance was hidden from Oppenheimer and his lawyers for a period of several months after the case against Oppenheimer was started. The AEC, which means Strauss, at first made available a strikingly incomplete record, making it seem that the 1947 clear-

ance was casually voted without any opportunity to consider the derogatory data. Oppenheimer's counsel before the Gray board, Lloyd Garrison, had to press very hard to get the whole story from the AEC. The full record was only produced toward the end of the hearings. On the face of the evidence, in short, there was at least a strong reluctance to reveal the truth about the 1947 AEC clearance, if not a positive effort to conceal it.

Third, this reluctance to reveal or this effort to conceal, whichever it may have been, assumes a most disturbing significance in view of the central fact about the subsequent opinion handed down by Lewis Strauss. In the Strauss opinion, the Chevalier story is everything. It provides the only proof cited by Strauss of Oppenheimer's "persistent and willful disregard of the obligations of security." It provides the only proof cited by Strauss of Oppenheimer's "continuing associations with Communists" in the postwar period. Above all, if it had not been for this Chevalier incident, in which Oppenheimer undoubtedly acted very wrongly, Strauss's other five "examples" would have been laughed out of court. The prosecutor's trick that provided the invaluable phrase—"a whole tissue and fabrication of lies"—alone gave a persuasive color of sinister importance to the other small stuff.

Now one of the AEC Commissioners in 1947—and a most active commissioner, who was regarded, so the testimony shows, as the AEC's expert on security—was none other than Lewis L. Strauss. As we have seen, everything significant in the Chevalier story—except, of course, the sad little Paris luncheon last year—was included in the full FBI file that went to the AEC. In that file, there was even the same sort of flat admission of lying to Colonel Pash that Oppenheimer also made before the Gray board. Furthermore, Lewis Strauss studied that file in 1947; for at least one member of the AEC staff clearly remembers being called in by Strauss that spring, to discuss the file and its derogatory data. Hence there can be no doubt that in 1947 Strauss knew all the basic facts of the Chevalier incident, which was to become the be-all and end-all of his bitter 1954 opinion condemning Robert Oppenheimer as a security risk. But in August 1947, Lewis Strauss voted with the other Atomic Energy Commissioners to grant Robert Oppenheimer full and final security clearance for the most confidential scientific post in the American government, the chairmanship of the AEC's General Advisory Committee. And in October 1947, in his capacity as a member of the Institute board, Strauss also nominated Oppenheimer to the directorship of the Institute for Advanced Study.

There is a glaring contrast here between the Strauss of 1947 and the Strauss of 1954, which is made all the more glaring by the apparent attempt to prevent the contrast from becoming too obvious. There is a puzzle in this contrast, and not a very pretty puzzle either. The solution must be sought—it can only be sought—in the character of Admiral Strauss himself.

Lewis Strauss—he pronounces it "Straws"—is a short, natty, energetic, ambitious, and intelligent man. From rather poor beginnings, he has made a handsome fortune for himself as a Kuhn Loeb partner and as a financial adviser to the Rockefellers. But he is no mere money-getter. He genuinely cares about the public service. He usefully served the late James V. Forrestal in wartime. And again, in his first term at the AEC, he was sometimes petty and wrong-headed; but he was also a valuable official, right about the hydrogen bomb when many others were wrong, and right too in pressing for the adoption of the long-range detection system that warns us of Soviet atomic and thermonuclear explosions.

Yet there is in Strauss something which gives him a desperate need to condescend, to be always agreed with, to be endlessly approved and admired, to dominate and play the great man. With his chiefs, like Forrestal and Eisenhower, he is all pliability. But from equals and subordinates, he likes no argument. One of his fellow commissioners has said of him, "If you disagree with Lewis about anything, he assumes you're just a fool at first. But if you go on disagreeing with him, he concludes you must be a traitor."

With such a man as Strauss, Oppenheimer was fated from the first to get on badly. He is by no means a man without fault. He has impossibly high intellectual standards. He

insists on them, with more than a trace of intellectual snobbery and sometimes with cold scorn for those who fall short. He has a good deal of the arrogance of the brightest boy in class; he is not patient with obtuseness, and his tongue can be very cutting. All these faults of Oppenheimer's were bound to exaggerate and indeed to inflame the faults of Strauss. And the very sign and seal of their early good relations, Oppenheimer's election to the Princeton Institute directorship, was a natural source of friction. For Strauss thought he had placed Oppenheimer under an obligation. Oppenheimer thought he had been given a job because he was worthy of it. And thus there arose between the two men the difficulties between the sponsor and the sponsored that are sadly familiar in all academic communities, as well as in the larger world.

With fair certainty, one can identify the crystallizing incident in the trouble between Oppenheimer and Strauss. It was a disagreement over the export of radioactive isotopes to our allies. In his first term at the AEC, Strauss, who knows little of physics and has a mania for official secrecy, always opposed the export of isotopes except for medical purposes. The AEC voted Strauss down, but that did not stop him. And in 1949, Strauss charged before the Joint Congressional Committee on Atomic Energy that American atomic secrets were being endangered by the export of certain isotopes to Norway.

In the ensuing ruckus, Oppenheimer was called by the Joint Committee to give his opinion, which he did with far too devastating brilliance. He made mincemeat of Strauss's scientifically uninformed thesis. He pointed out that anything—the knowledge that two and two makes four—may play a part in atomic weaponry. "You can use a shovel for atomic energy," he said, "—in fact, you do. You can use a beer bottle for atomic energy—in fact you do." Then, not content with making Strauss look an ignoramus, Oppenheimer went on to make him seem small-minded. "The positive arguments for making [isotopes] available," he said, "lie in fostering science; they lie in making cordial relations with the scientists and technical people of Western Europe . . . They lie in doing the decent thing."

The AEC Counsel of that period, Joseph Volpe, recalls watching Strauss's face darken with fury during this testimony; and he remembers an exchange with Oppenheimer at the close. "Joe," said Oppenheimer, "how did I do?" Looking at Strauss still suffering from his humiliation, Volpe answered, "Robert, you did much too well for your own good." The memory of Volpe is confirmed by the behavior of Strauss, for whom the isotopes remain, to this day, a major King Charles's head. When he became AEC Chairman five years after the defeat in the hearing, Strauss solemnly exhumed this dead-as-mutton issue, and discoursed on it at great length and with tedious self-justification at no less than four commission meetings.

There were other, later disputes to deepen the trouble between Strauss and Oppenheimer—about the Hydrogen Bomb, about the closeness of our partnership with Britain and Canada, and about Senator Hickenlooper's famous and nonsensical charge of "incredible mismanagement" at the AEC, which Strauss had encouraged in his backstairs way. In the end, the trouble clearly became very deep and dark indeed.

As often happens, however, a good face was put upon this trouble for a long time. Not too long before the Oppenheimer case began, Strauss even put his name to a generally desired motion raising Oppenheimer's salary from the Princeton Institute. Quite possibly, the action against Oppenheimer that Strauss initiated as soon as he became AEC Chairman was not then really intended to lead to anything; for there was a delay of several months between the initiation and fruition. Very probably, the precipitating factor was a series of moves against Oppenheimer by Senator McCarthy, indicating an imminent investigation, which left Strauss the choice between forestalling McCarthy or appearing before the Grand Inquisitor as Oppenheimer's sponsor.

At any rate, what really matters is the central fact. It is impossible to avoid the conclusion that this petty, tangled, tragic business of the old friction and disagreement between Strauss and Oppenheimer contains one of the essential clues to the Oppenheimer case.

It is not surprising, then, that Commissioner Henry Smyth's dissent grimly emphasized the role of "powerful personal

enemies" in the attack on Robert Oppenheimer. Yet it would be *simpliste*, and it would leave vital questions unanswered, to close our inquiry at this point. The part of Strauss has been shown, and his opinion has been analyzed. But how about the other AEC commissioners, all of whom except Smyth voted not to clear Oppenheimer? And how about Gordon Gray and Thomas Morgan, who also voted against clearance, although for reasons quite different from those given by Strauss?

It is not good enough to say that Dr. Ward Evans and Commissioner Smyth devastatingly answer the majority opinions of the Gray board and the AEC. It is not good enough to say, either, that Strauss and Robb staged a prosecution in the guise of a fact-finding proceeding, and that this device was remarkably successful. It is not even good enough to blame the result on the *Zeitgeist*, as was done by the great physicist Leo Szilard in the best of all comments on the Gray-Morgan finding. Szilard, who is no friend of Oppenheimer's, said simply: "Unfortunately for all of us, [Gray and Morgan] are as good men as they come, and if they are affected by the general insanity which is more and more creeping up on us, who can be counted on to be immune?"

THE truth is that Strauss, Robb, and the *Zeitgeist* had important collaborators. No high, confidential official of his time was more careful than Robert Oppenheimer about discussing problems of policy outside the government councils; but in council, as his duty required, he freely spoke his mind and obstinately followed his conscience on many controversial matters over a long period of years. He spoke his mind, moreover, with no amiable willingness to suffer fools gladly. In several quarters, he thus built up a massive accumulation of enmity and suspicion, aroused institutional sensibilities and personal jealousy and dislike. The record of the Gray board hearings reeks like a compost heap with the emotions engendered by old policy disputes. And it shows, alas, that in modern America Lewis Strauss is by no means alone in equating disagreement with disloyalty.

In the somewhat bedragged parade of Oppenheimer-haters whom Prosecutor Robb led to the stand, the former chief of the Air War College, Major General Roscoe C. Wilson, will serve to typify—for he almost *is*—an aroused institution. General Wilson was called because he once "felt compelled to go to the Director of Intelligence to express my concern over what I felt was a pattern of action . . . not helpful to national defense." He solemnly testified that he was first alerted by Oppenheimer's "interest in what I call the internationalizing of atomic energy"—an interest that was shared, to be sure, by all the leaders of the American government and a few others too, such as Bernard M. Baruch. Then there were other things in this pattern that worried General Wilson. There was, for instance, Oppenheimer's insistence that it was technically premature to try to build nuclear-powered aircraft. "I don't challenge his technical judgment," remarked the General plaintively, "but at the same time he felt less strongly opposed to nuclear-powered ships."

THE Air Force General who saw a security risk in the suggestion that a ship can take a nuclear reactor more conveniently than an airplane has his perfect companion piece in the Air Force Chief Scientist, David Tressel Griggs, who decided Oppenheimer was either "confused or pro-Russian" because Oppenheimer actively urged a serious air defense of America's cities and industries against Soviet atomic attack.

The issues that Griggs and Oppenheimer quarreled over must be examined later. It is enough to say here that the Griggs testimony is a morass of the kind of inaccuracies that go with petty bureaucratic talebearing, and that Griggs unblushingly confessed the origin of his opinions. There had been "pretty strong controversies," he said, and he added complacently that "when you get involved in a hot controversy, it is awfully hard not to question the motives of the people who oppose you." This he appeared to consider as common Christian, or at least common bureaucratic, practice.

At the same time, Griggs seems to have some dim inkling that, just possibly, differences of view on highly arguable policy issues ought not always to lead to security proceedings. After repeatedly attacking Oppenheimer's loyalty because of past disagree-

ments, he finished with a grandiose flourish: "If it ever comes to the day when we can't disagree and disagree violently on public and on national policy, then of course I feel it will be a calamity for our democracy. I think perhaps I have said enough."

He had indeed, and so we may leave Griggs for the most interesting and complex, the most distinguished, and the most demanding of sympathetic understanding among all these Oppenheimer haters.

DR. EDWARD TELLER, author of the "brilliant invention" that made the hydrogen bomb possible, is one of the great scientists of our time. This strange genius (in himself the final argument for a security system that allows for the exceptional and the eccentric) is a man all light and dark, gentleness and anger, serene high thought and furious personal feeling. With Oppenheimer he has had a most curious relationship, official yet somehow intense and tragic, which can be traced through the pages of the Gray board record.

Its beginnings at Los Alamos are revealed in the testimony of the respected Dr. Hans Bethe. Bethe told the Gray board that "no enterprise quite as hard" as the job done at Los Alamos "had ever been attempted before"; and that the "success was due mostly to [Oppenheimer's] leadership." Oppenheimer, said Bethe, was the "man who really understood everything and was recognized [by the other scientists] as superior in judgment . . . and knowledge to us all." But as usually happens in any large community with an admired leader, there were a few, a very few, who sharply rejected Oppenheimer's leadership. One of these was Edward Teller, who served under Bethe in the important Los Alamos Theoretical Division.

Said Bethe: "I relied . . . I hoped to rely very heavily on [Teller] to help our work . . . . It turned out he did not want to co-operate. He did not want to work on . . . the line of research that everybody else had agreed to as the fruitful line. . . . So that in the end there was no choice but to relieve him of work in the general line of development of Los Alamos, and to permit him to pursue his own ideas entirely unrelated to World War II."

Teller's own testimony shows a great deal more. There is Teller in wartime, fixed in his "own ideas" (which already concerned thermonuclear weapons) and objecting sharply to Oppenheimer's wartime policies. There is Teller, just postwar, bitterly disappointed because thermonuclear development was not already being pushed on the scale of another Manhattan District. There is Teller blaming Oppenheimer for this decision, which was made by many people and on the highest level of government. And there is Teller again blaming Oppenheimer for the postwar slump at Los Alamos, at a time when Oppenheimer was infuriating the rest of the scientific community by backing the May-Johnson bill, with its prolongation of military control, because he thought this was the only way to hold Los Alamos together.

Then there is Teller hurrying to Washington after JOE I, the code name for the first Soviet atomic test, to press for an immediate H-bomb program on the largest scale. And there is Teller infuriated by the adverse recommendation of the AEC General Advisory Committee, and once more blaming Oppenheimer alone for this unanimous action of one of the most high-powered boards ever assembled.

FINALLY, there is the last and the somehow conclusive episode, for which one must return to the testimony of Bethe. President Truman had announced his decision to build the H-bomb at all costs. As the leading expert and grand advocate of the ultimate weapon, Teller immediately became the key man in the project at Los Alamos. But Teller regarded the great laboratory as Enemy Ground, no doubt because he thought of it as Oppenheimer Territory. He complained to the Air Force authorities—and the ears of David Griggs were eagerly receptive—that his work was being hampered and sabotaged. He demanded a second laboratory, a duplicate of Los Alamos, in which to do his job. Bethe, who was by now working under Teller, had to go to Washington to explain that Teller was talking nonsense. And nonsense it proved to be; for Teller's "brilliant invention" only indicated the right approach, while the Los Alamos staff triumphantly did the immense job of designing and building the H-bomb.

Great power of intellect, an obsessive concentration on a single object, above all an

obsessive conviction that one man and one man only stood in the way of attaining that object—these are the qualities that stand out in the Teller story. Before the Gray board, Teller pictured Oppenheimer as a sort of mass-Svengali, somehow commanding the sheeplike obedience of scores of equally distinguished, extremely opinionated, and incorrigibly individualistic leaders of American science, and always swaying the majority of American physicists to oppose and obstruct Teller. Yet Teller also told the Gray board that he believed Oppenheimer was "loyal to the United States." And when Gordon Gray asked him whether "it would endanger the common defense and security to grant clearance to Dr. Oppenheimer," Teller replied with a fine display of intellectual precision.

"I believe," he said, ". . . that Dr. Oppenheimer's character is such that he would not knowingly and willingly do anything that is designed to endanger the safety of this country. To the extent, therefore, that your question is directed towards intent, I would say I do not see any reason to deny clearance. If it is a question of wisdom and judgment, as demonstrated by actions since 1945, then I would say it would be wiser not to grant clearance. I must say that I am myself a little bit confused on this issue, particularly as it refers to a person of Oppenheimer's prestige and influence."

III. What Is Security?

EDWARD TELLER's final statement to the Gray board deserves to be closely analyzed. First, he said that Robert Oppenheimer would not "knowingly" take any action contrary to this country's interests. Second, however, he questioned Oppenheimer's "judgment," implying that Oppenheimer's advice on great issues of national policy had been injudicious and unhelpful. In other words, Dr. Teller said that Oppenheimer was not a security risk under any sane definition of the term. But Teller also told the Gray board that he would not grant security clearance to Oppenheimer, simply because Oppenheimer's judgment had differed from Teller's judgment.

Whether Oppenheimer was right, or Teller was right, in these matters on which they differed, does not affect the question that Teller raised. It is a very simple question When you do not like a man's advice on policy, do you simply strike him off your list of advisers, or do you drag him before a security board and hold him a security risk—which really means, if it any longer means anything at all, that his advice was evilly motivated?

This question is crucial, for Oppenheimer's loyalty and discretion were held proven and there was no hint of blackmailability, or anything of that sort. Instead, behind every accusation except that of the Berkeley intelligence officer, Colonel Pash, there was always the same background of what Griggs called "hot" controversy.

Speaking for the majority of the Atomic Energy Commission, Admiral Strauss formally declared that "Dr. Oppenheimer was, of course, entitled to his opinion." He thereby denied that Oppenheimer was being held a security risk because of the hot controversies of the past. But on the face of the record this Strauss declaration-denial was both misleading and hypocritical.

THE one important new item in the original AEC charges against Oppenheimer, drawn up under Strauss's own direction, related to Oppenheimer's opinions about the H-bomb. Oppenheimer was in fact tried for these and other policy opinions before the Gray board, at such length that at least half the record is an inquiry into his opinions. The Gray board, in its most important finding, held him guilty on his opinions. And it is abundantly clear that if it had not been for his opinions there would have been no Oppenheimer case. For Lewis Strauss, Roger Robb and the *Zeitgeist,* all working together, still needed the allies who had been recruited and the climate that had been engendered by Oppenheimer's forthrightness on great issues of national policy.

One is tempted to avoid looking into this matter of Oppenheimer's policy advice, since it has no relevance at all to the question of his loyalty or disloyalty, security or insecurity, unless a wrong motive can be shown. There was no such showing, as the Gray board acknowledged; yet the matter of Oppenheimer's policy advice cannot be avoided, because it is relevant to the Oppenheimer case as a demonstration of how

our American security system now works.

There were, then, three main pieces of advice that Oppenheimer gave his government, which ended by getting him into trouble. All three were subtly inter-related, since all three in part at least grew out of Oppenheimer's concept of the right American world strategy. And of these pieces of advice, the first was the one most people think was wrong, the advice about the hydrogen bomb.

THE stage for that advice, its backdrop as it were, was the Truman–Louis Johnson disarmament program of 1949-50. "Defense economy" had left the country with no serious defense except air-atomic striking power. The Strategic Air Command itself was in far from satisfactory shape at that time, and our world strategy squarely depended on the effects—to a quite large degree, on the psychological effects—of our atomic monopoly. And in September 1949 the Soviets broke that monopoly by successfully testing their first atomic bomb.

The news of JOE I caused natural and widespread consternation. Edward Teller, Dr. Ernest Lawrence, and Lawrence's sidekick, Dr. L. W. Alvarez, enplaned from the West coast to urge an immediate, all-out effort to top the Soviet A-bomb with an American H-bomb. Commissioner Strauss, the Air Force and the other services, the Joint Congressional Committee, were all rapidly mobilized. In this agitated climate, AEC Chairman David Lilienthal asked for the views of his General Advisory Committee. And toward the end of October the grandees of the GAC assembled, with Oppenheimer in the chair; and after the most prayerful discussion they recommended against the "crash" program Teller was urging.

It must have taken considerable moral courage to make that recommendation. And it was by no means so eccentric as most people suppose, for the objections to the H-bomb crash program were very strong indeed.

First, there were the moral objections. Anyone who thinks it was immoral to feel moral objections to the H-bomb must either know very little about the absolute weapons or be sadly in need of training as a human being. Beyond that, these need not be discussed.

Second, there were extremely important technical objections. At that period, our atomic stockpile was not yet adequate. As then conceived by Teller and everyone else, the H-bomb would have consumed an enormous quantity of fissionable raw stuff, with much less return in total explosive power than could be got from an equivalent investment in more A-bombs. It was not at all clear whether many A-bombs should be sacrificed to get one H-bomb. It was not at all clear, either, whether the kind of H-bomb that was being discussed could ever be built at all; and in the end it never *was* built. In 1950, Dr. Teller's "brilliant invention" changed the whole picture, opening the way to the large, economy-sized H-bomb with a lithium-hydride core. And we have Teller's own testimony that when he first announced his "invention," Oppenheimer warmly congratulated him and declared that he would have felt quite differently in the 1949 H-bomb debate if this altogether different weapon had been the subject.

THIRD, there was also a strategic objection to the H-bomb, felt particularly strongly by Oppenheimer and Conant, and clearly expressed in the unhappy letter that Oppenheimer wrote Conant before the fateful GAC meeting. Here is the relevant passage:

> What concerns me is really not the technical problem. I am not sure the miserable thing [*i.e.* the H-bomb] will work, nor that it can be gotten to a target except by oxcart. It seems likely to me even further to worsen the *unbalance of our present war plans. What does worry me is that this thing appears to have caught the imagination, both of the Congressional and the military people, as the answer to the problem posed by* [*the Soviet atomic test*]. It would be folly to oppose the exploration of this weapon. We have always known it had to be done; and it does have to be done, although it appears to be singularly proof against any form of experimental approach. *But that we become committed to it as the way to save the country and save the peace, appears to me full of dangers.*

Behind these passages we have italicized was Oppenheimer's conviction that an unthinking and unqualified dependence on a stock of absolute weapons, as a sole defense of this country, had now become infinitely perilous.

This magical theory of defense was already enthroned at the Pentagon, it must be remembered, in the obstreperous person of Secretary Johnson. The common reaction to JOE I—"Well, the Soviets have the A-bomb but we'll just get the H-bomb, and then everything will still be all right"—seemed so wrong to Oppenheimer that he perhaps overreacted against it. This over-reaction can also be observed in the testimony of Dr. Conant, who told the Gray board that he had feared an American H-bomb because he expected it to breed groundless complacency, and so to cause all the many needs of a balanced defense to be scamped or neglected.

EVENTS have proved the wisdom of Oppenheimer's and Conant's fear of the magical theory of defense by nothing but absolute weapons. Nonetheless, Oppenheimer now acknowledges that the GAC recommendation of 1949 was mistaken, both because the way to prevent groundless complacency is to fight it head on; and because the GAC did not answer the two simple questions asked by President Truman, when he was first told of the H-bomb debate by Admiral Sidney Souers. "Can the Russians make this thing?" Truman inquired. "And if so, how can we help making it?"

It should be noted, however, that in 1949-50 the anti-H-bomb recommendation of the General Advisory Committee caused hardly more than a temporary ripple. Conant, Fermi, Smith, DuBridge, Rabi, Rowe, Buckley, and Oppenheimer presented a solid front together. In those happier days, no one was foolish enough to suppose that the unanimous verdict of such a group could be unpatriotic in purpose. Except for Edward Teller, no one was foolish enough to suppose, either, that all the members of a group of this caliber could be swayed against their better judgments by the mesmeric influence of Svengali-Oppenheimer. President Truman rejected the General Advisory Committee recommendation. The H-bomb program was launched. And the position taken by the GAC was generally forgotten, until later and quite different advice given by Oppenheimer caused certain powerful persons to look for sticks to beat him with.

The next act of our drama of opinion occurred after an interval of two years, in 1951. This time the backdrop was the bloody ground fighting in Korea, the inadequacy of our tactical air effort over the Korean battlefields, and the vast convulsive Western struggle to rearm, centering around NATO, that Korea had set in motion. For all these reasons Project VISTA was started by the Pentagon. Under the leadership of Drs. DuBridge and Charles Lauritsen, VISTA was to study the tactical use of atomic bombs and related problems.

In the GAC, Oppenheimer had taken the lead in pressing tactical A-bomb development. When the VISTA scientists had assembled their data, he was called in as a consultant; and at the request of DuBridge and Lauritsen, he drafted the fifth chapter of the VISTA report, setting forth its conclusions and recommendations. In two important ways, this chapter Oppenheimer drafted was a significant turning point. It outlined what is now the approved American doctrine for tactical use of atomic weapons. And, while still in draft form, it was taken to Paris by Oppenheimer, to be shown to General Dwight D. Eisenhower, who then welcomed the VISTA conclusions with intense enthusiasm and made them the basis of a radical revision of his Western European defense plans.

UNFORTUNATELY, however, there was one thing in Oppenheimer's VISTA draft that made it as unwelcome to the Air Force Staff as it had been welcome to Eisenhower and his staff at SHAPE. Oppenheimer, who was well aware we were entering the era of atomic plenty, proposed that the Joint Chiefs of Staff make a new division of our atomic stockpile, allocating part to reserve, part for tactical use, and part to the Strategic Air Command.

The Air Generals, no great believers in atomic plenty, had been fighting tooth and claw for five years to keep the entire atomic stockpile as the Strategic Air Command's monopoly asset. Compared to SAC, the Air Generals cared very little indeed about tactical air, which was one of the reasons for the difficulties in Korea. Now Oppenheimer was suggesting that the Joint Chiefs change the rules, and allocate some of SAC's hard-won bombs to tactical uses. This automatically reduced most of the leaders of the Air

Staff to a condition of apoplectic fury.*

Then came the drama of opinion's third act, in 1952. This time the back-drop was the increasingly alarming intelligence about the growth of the Soviet atomic stockpile, about the rising power of the Soviet Strategic Air Army, about the first long-range reconnaissance flights over this hemisphere. And, besides this immediate backdrop, there was also some earlier background.

In 1950, Joe I had started a battle in the National Security Council. The majority had insisted that the new Soviet atomic bomb made continental air defense an urgent matter. The Air Generals, who cared even less about air defense than about tactical air, had pooh-poohed the whole idea. But over the angry opposition of the Air Force, the National Security Council had issued a directive giving the air defense of this continent the highest defense priority. In reluctant obedience to this directive, Project Lincoln had been established at the Massachusetts Institute of Technology, to study the air defense problem. Since then, as the intelligence indicated, the problem had grown urgent. And now, in the summer of 1952, Lincoln had collected its data; it had made certain brilliant technological break-throughs; and a large number of the country's leading scientists were gathering to act as consultants of a special Summer Study Group, which would organize the Lincoln results in a coherent plan.

Even before the Lincoln Summer Study Group began its work, the Air Staff was on the *qui vive*. We have the testimony of Dr. Jerrold Zacharias, a leading member of Project Lincoln, that Air Force Chief Scientist Griggs attempted to "sabotage" the effort at the very start. Griggs was repelled, however. Oppenheimer, Dr. Rabi, Dr. Lauritsen, and many others gathered as planned. And in the end, the Summer Study Group produced a powerful report which is now proving to have been another great turning point in national defense planning. This report made two main points:

(1) That the Soviets would soon have the air-atomic capability of destroying the United States.

(2) That owing to the recent technological break-throughs, an effective American air defense could now be constructed, although at very great cost.

You would have supposed the Air Force would have welcomed the report. Instead the Air Force authorities first sought to prevent the Lincoln results from being communicated to the rest of the government. Then, when the results were nonetheless communicated and air defense became a serious issue, the word went out from the Air Staff that the Lincoln program was nothing but a plan for "another Maginot line," and an impractical, long-hair plan at that. This crude propaganda was further combined with a mounting personal attack on Oppenheimer, portrayed as the devil of the Summer Study Group.

There was talk of a sinister cabal called ZORC (standing for Zacharias, Oppenheimer, Rabi and—illogically—Charles, from the first name of Lauritsen) that was darkly plotting against the security of the United States. A *Fortune* article full of snide hints about Oppenheimer's motives was directly inspired by the Office of the Chief of Air Staff. And the issue of Oppenheimer's loyalty was officially raised in government councils.

To the ordinary American citizen, who is not familiar with Big Bomber Generals and Battleship Admirals, these goings-on may appear downright fantastic. Yet they are described without exaggeration, as these reporters, who lived through all these episodes, can testify from first-hand knowledge. And, if you consider all the factors, the fantasy is not so extreme as it may appear.

In brief, the Big Bomber Generals, the champions of strategic air power, have always dominated the American Air Force. For human reasons, Big Bomber Generals are possessed by the same passionate feeling for their own special weapons, even at the expense of all other air weapons, that was also the mark of the Battleship Admirals who fought the carriers so long and so bitterly. Moreover, the Big Bomber Generals were and are more justified than the Battleship Admirals. Since

*David Griggs' testimony indicates that there were other things unfavorable to the Strategic Air Command in this Oppenheimer draft of the Vista report's fifth chapter. But, after the Gray Board hearings ended, the original draft was found by Oppenheimer; and it provided documentary proof that Griggs' memory was at fault in this matter.

the end of the last war, strategic air power has been our only form of offensive power against the major enemy; and it has been absolutely vital to have an adequate and efficient Strategic Air Command.

THE fight for an adequate Strategic Air Command has been long and hard, and it has been especially envenomed whenever appropriations were in question. Zealots who must endlessly fight for their beliefs humanly tend to lose their sense of proportion —to feel that the object of their zeal is all that matters, and that all else is nothing. Hence the leaders of the air staff saw one thing, and one thing only, in the LINCOLN Program for American Air Defense. They saw that it would make heavy demands for funds. They further feared, and perhaps reasonably feared, that the economizing politicians might partly subtract the funds for air defense from the appropriations of the Strategic Air Command. And that danger (which the LINCOLN scientists had not considered) was enough to persuade the air staff that the LINCOLN air defense plan was nothing less than a sinister, insidious, indirect attack on strategic air power.

All these points emerge very clearly in the Gray board hearings, if you read the testimony of David Griggs and then subtract the many errors of fact shown up in it by the testimony of Rabi, Zacharias, and others. Of course LINCOLN was not a plot against SAC, any more than VISTA was a plot against SAC. Of course the Summer Study Group's idea was the one Dr. Rabi neatly expressed when he was asked whether a belief in air defense necessarily proved hostility to strategic air power: "But there are the two arms," said the mild Rabi patiently. "There is the punching arm, and there is the guard. You have to have *both*."

Oppenheimer said the same thing even better, when he remarked that he had "never seen a first-class prize fighter with a complete glass jaw." The fact that this country dare not continue with a complete glass jaw is now being officially recognized—belatedly, and with insufficient urgency—by the Eisenhower Administration. The Summer Study Group's recommendations are now being acted upon, but after two precious years have been wasted.

But surely it is no longer necessary to labor the point, in the new era of the Soviet hydrogen bomb, that Oppenheimer and the scientists were right in urging a serious American air defense. The opposition to the air defense idea, which incidentally defied national policy as laid down in the Security Council directive of 1950, was the blind, angry reaction of a military bureaucracy both set in its ways and easily irritated by military proposals of civilian origin. The question remains why Oppenheimer was chosen, among so many others, as the particular target of this irritation. The answer comes in two parts.

ON THE one hand, the political follies of Oppenheimer's prewar years made him vulnerable, as he was well aware —for one of the things that stand out in this story is Oppenheimer's cool courage in challenging the greatest power groups of the government with this knowledge of his own vulnerability always in his mind. And because Oppenheimer was vulnerable, the temptation to try to smear his past politics over onto his present advice could hardly be resisted by the little men who were upset by that advice. On the other hand, the Air Force zealots quite rightly smelled, if they did not quite understand, the difference between Oppenheimer's strategic concept and their own. They still believed that America could be satisfactorily and uniquely defended by strategic air power and atomic weapons. They had a lot of support for that belief—and still have, for that matter; the present Secretary of Defense has clearly adopted this theory to suit his budgetary convenience.

After the Soviet atomic bomb, on the other hand, Oppenheimer had enough sense to realize that the "grand deterrent" or "massive retaliation" theory of American strategy had become pure nonsense. He did not oppose strategic air power. He certainly wanted to avoid a war of absolute destruction with the absolute weapons if that were possible, but he also worried about whether we had enough strategic air power and whether SAC was modern enough. Furthermore, he could foresee that mere "massive retaliation" would become very cold comfort, when the thing to be retaliated against was the total destruction of these United States. He could foresee the weakening of will, the paralysis of policy that total peril would inevitably beget, and indeed has already partly begotten

in Britain and Western Europe. And he could foresee that in time of total peril, there would be an increasing reluctance to respond to local challenges, such as those in Korea and Indochina; and his correctness on this point has been proven too.

For all these reasons, Oppenheimer pleaded for a more balanced defense system; and he made his plea before the march of events rendered his reasons comprehensible to most people. So the zealots' attack on him was organized, and the ground was prepared for the Oppenheimer case. On this aspect of the case the final word was said by Dr. Vannevar Bush, in a superb explosion of indignation to the Gray board.

The Grand Old Man of American science told the board, point blank, that the AEC's statement of charges ought to be sent back for re-drafting, because it included the charge that Oppenheimer had opposed the hydrogen bomb. This, he said, was "quite capable of being interpreted as placing a man on trial because he held opinions, and had the temerity to express them."

"If this country ever gets . . . that near to the Russian system," Bush continued, "we are certainly not in any condition to attempt to lead the free world. . . . We have been slipping backward in our maintenance of the Bill of Rights. . . . I think . . . no board should ever sit on a question in this country of whether a man [served] his country or not because he expressed strong opinions. If you want to try that case, you can try me."

In those brave words, Dr. Bush was indicting the whole American security system. His single indictment was enough to damn, yet the record of the Oppenheimer case contains half a dozen other points which Dr. Bush might have attacked with equal justice.

Consider, first, the organization of the case. As Gordon Gray repeatedly asserted, it was supposed to be a fact-finding proceeding. In procedures it was nonetheless a prosecution, and in organization it shows the very opposite of a serious desire to find out the facts.

The proof of that is simple. All the witnesses called by the AEC were hostile to Oppenheimer in one way or another. Prosecutor Robb was content to marshall his parade of Oppenheimer-haters. It was Lloyd Garrison who called to the stand Conant and Fermi, DuBridge, Bacher, Bush, Bethe, John J. McCloy, George F. Kennan, Zacharias, Rowe, Lauritsen, Lilienthal, John von Neumann, Gordon Dean, and even General Groves and Colonel Lansdale.

In the air defense case, for instance, did Robb really prefer Griggs' wildly distorted version of the facts to the solid and detailed evidence of Zacharias, Rabi, and Lauritsen? And if so, what kind of fact-finding was this?

Again, consider the way this case was managed. As has been shown already, one set of charges was originally specified by the AEC; Gray and Morgan convicted Oppenheimer on quite another set of charges; and Strauss in turn rejected the main Gray-Morgan finding and held Oppenheimer a security risk on still a third set of charges never mentioned until then. The peculiarity of this process is made all the more glaring by the obvious fact that sustaining Gray and Morgan would have been fatal to Strauss. For most of the other great American physicists had fully shared Oppenheimer's lack of enthusiasm for the H-Bomb, and thus the AEC could not dare to accept this lack of enthusiasm as proof of risk to security.

If these are the methods—if the guardians of our security may continuously make up new charges as old ones are refuted or found inexpedient—when or where can the defendant-citizen hope to find solid ground?

Then too, consider the presentation of the case, and particularly the strange episode of the last-minute publication of the transcript of the Gray board hearings. During the hearings, Chairman Gray strongly warned every witness that all that passed was strictly confidential, and would never see the light of day. But the public reception of the Gray-Morgan opinion was puzzled, cold, and unfriendly. As it came time for Strauss to hand down his own condemnation of Oppenheimer, the climate was decidedly unfavorable. At this juncture, despite all that Gray had said, the transcript was hurriedly printed by order of Admiral Strauss. It was handed to the press—all 992 pages of it—eighteen hours before the deadline set for publication. By a most singular coincidence, Prosecutor Robb's star client, the McCarthy incense-swinger, Fulton Lewis, Jr., broke the deadline immediately to tell

his radio audience about all the ugliest stuff that the transcript contained. No one has explained how Lewis so rapidly located these gamy morsels. Other reporters, at any rate, had the utmost difficulty in finding their way through the massive document. So most of them followed Lewis, headlining precisely the charges of disloyalty that Gray, Morgan, and even Strauss had held to be utterly unfounded. And thus the stage was admirably, if somewhat artificially, set for the Strauss opinion, which followed hard on the heels of these misleading headlines.

Nor is this all. A main buttress of Prosecutor Robb's case was the group of Air Force documents in which the zealots spewed out their suspicions of Oppenheimer. These documents were communicated to the Gray board before the hearings began. These documents were never communicated to Oppenheimer on the grounds that they were highly classified, and thus Oppenheimer and his counsel were required from start to finish to answer accusations which were never fully known or forthrightly made. But since the hearings ended, Admiral Strauss has made the attempt he so strangely did not make during the hearings to have at least one of these documents declassified, and it is a fair bet that as the inwardness of the Oppenheimer case begins to be more widely understood, these same documents that were always hidden from Oppenheimer will be spread before the general public to start a counter-fire.

If these things are permitted, why may not the American government blacken the name of any honest citizen it chooses? After all, we are officially encouraged, nowadays, to write poison-pen letters about one another in security's sacred name. The security files bulge with them. What could be easier than to daub the ugliest dirt on the most innocent man by fishing the appropriate nastiness out of the files and giving it solemn and official publication?

FINALLY, consider with the utmost care, consider as an American citizen who may some day be called to answer as Oppenheimer was called, the standards of security that this case establishes. Look, for these standards, to the opinion of Gordon Gray and Thomas Morgan. Look, and look well, at these things Gray and Morgan said about this man whom they then held a "security risk":

> We find no evidence of disloyalty. Indeed, we have before us much responsible and positive evidence of the loyalty and love of country of [Robert Oppenheimer]. . . .
>
> Dr. Oppenheimer served his country because it sought him. The impact of his influence was felt immediately and increased progressively as his services were used. The nation owes [such scientists], we believe, a great debt of gratitude for loyal and magnificent service. This is particularly true with respect to Dr. Oppenheimer. . . . the Board had before it eloquent and convincing testimony of Dr. Oppenheimer's deep devotion to his country. . . .
>
> We have given particular attention to the question of his loyalty, and we have come to the clear conclusion, which should be reassuring to the people of this country, that he is a loyal citizen. . . .
>
> It must [also] be said that Dr. Oppenheimer seems to have had a high degree of discretion reflecting an unusual ability to keep to himself vital secrets.

SO HERE is this man, passionately spoken for by the great men of his profession and his finest colleagues in the government, his "deep devotion" to his country acknowledged, his immense services admitted, his perfect discretion approved, with not an iota of evidence in the record that he ever, at any time since he became a mature man, failed to put this nation's interests first and the rest nowhere. And what has happened to him? On the evidence that has been shown, by the methods that have been described, for the reasons that have been suggested, and by such accusers as have been displayed, he has been held a "security risk," fit to serve his country no longer, pilloried before his fellow citizens and the world, debarred from continuing his immense contribution to the true security of the United States.

Earlier in this report, we have said that the ruling of the Atomic Energy Commission disgraced Robert Oppenheimer. But we were wrong. This act did not disgrace Robert Oppenheimer: it dishonored and disgraced the high traditions of American freedom.

PAUL, WEISS, RIFKIND, WHARTON & GARRISON

MEMORANDUM FOR THE PRESS

FOR RELEASE: 7 PM. Tuesday, June 1, 1954

"Dr. Oppenheimer has requested John W. Davis and myself to make public the attached documents of record in the proceedings concerning him.

Lloyd K. Garrison"

* * *

LIST OF ATTACHED DOCUMENTS:

1. "Findings and Recommendation of the Personnel Security Board in the case of Dr. J. Robert Oppenheimer," containing Majority and Minority Opinions of the Board (in form of letter to K. D. Nichols, General Manager, AEC, dated May 27, 1954).

2. Letter from General Nichols to Dr. Oppenheimer, dated May 28, 1954.

3. Reply to General Nichols by Lloyd K. Garrison, counsel for Dr. Oppenheimer, in letter dated June [illegible], 1954, in which John W. Davis joins.

* * *

PAGE INDEX TO ATTACHED DOCUMENTS:

Loyalty and Discretion: Personnel Security Board unanimously (3-0) finds Dr. Oppenheimer loyal (pp. 21, 27, 29, 30, 31, 33, Document #1) and discreet in keeping vital secrets (p. 31).

Reinstatement of Clearance: A majority of the Board (2-1) recommends that Dr. Oppenheimer's clearance not be reinstated (p. 33). Dr. Evans, the third member of the Board, dissents (p. 34).

Procedure on Appeal: K. D. Nichols, General Manager, AEC, advises Dr. Oppenheimer and counsel that final determination as to reinstatement of Dr. Oppenheimer's clearance will be made by the Atomic Energy Commission (Document #2).

Counsel for Dr. Oppenheimer waives review by Personnel Security Review Board and requests leave to file brief and make oral argument before the Commission (Document #3).

FOR RELEASE: 7 P.M. Tuesday, June 1, 1954

UNITED STATES
ATOMIC ENERGY COMMISSION
Washington 25, D. C.

May 28, 1954

Dr. J. Robert Oppenheimer
The Institute for Advanced Study
Princeton, New Jersey

Dear Dr. Oppenheimer:

I am enclosing herewith a copy of the Findings and Recommendation of the Personnel Security Board which has been considering your case. A majority of the Board recommends that your clearance not be reinstated.

You have the right, under Section 4.18 of the Atomic Energy Commission's Security Clearance Procedures, to request review of your case by the Personnel Security Review Board and to submit a brief in support of your contentions. If you wish such review, it is necessary that you submit your request to me within five days of your receipt of this letter or June 7, 1954, whichever is later, and that your brief be filed with me not later than twenty days after your receipt of this letter.

If you do not request review of your case by the Personnel Security Review Board within the prescribed time, a final determination will be made on the basis of the existing record.

Upon full consideration of the entire record in the case, including the recommendation of the Personnel Security Review Board in the event you request review by that Board, I shall submit to the Commission my recommendation as to whether or not your clearance should be reinstated. The final determination will be made by the Commission.

I am sending a copy of this letter to Mr. Garrison, along with additional copies of the Personnel Security Board's Findings and Recommendation.

Sincerely yours,

/s/ K. D. Nichols

K. D. Nichols
General Manager

cc: Lloyd K. Garrison, Esq.
Paul, Weiss, Rifkind, Wharton
& Garrison
575 Madison Avenue
New York 22, New York
(with 5 copies Enclosure)

UNITED STATES
ATOMIC ENERGY COMMISSION
Washington, D. C.

May 27, 1954

Mr. K. D. Nichols, General Manager,
U. S. Atomic Energy Commission
1901 Constitution Avenue, N. W.
Washington 25, D. C.

Subject: FINDINGS AND RECOMMENDATION OF THE PERSONNEL SECURITY BOARD IN THE CASE OF DR. J. ROBERT OPPENHEIMER

Dear Mr. Nichols:

On December 23, 1953, Dr. J. Robert Oppenheimer was notified by letter that his security clearance had been suspended. He was furnished a list of items of derogatory information and was advised of his rights to a hearing under AEC procedures. On March 4, 1954, Dr. Oppenheimer requested that he be afforded a hearing. A hearing has been conducted by the Board appointed by you for this purpose, and we submit our findings and recommendation.

Dr. Ward V. Evans dissents from the recommendation of the majority of the Board, and his minority report is attached. He specifically subscribes to the "Findings" of the majority of the Board, and to a portion of the material entitled "Significance of the Findings."

Introduction

It must be understood that in our world in which the survival of free institutions and of individual rights is at stake, every person must in his own way be a guardian of the national security. It also must be clear that, in the exercise of this stewardship, individuals and institutions must protect, preserve and defend those human values for which we exist as a nation, as a Government, and as a way of life.

The hard requirements of security, and the assertion of freedoms, together thrust upon us a dilemma, not easily resolved. In the present international situation, our security measures exist, in the ultimate analysis, to protect our free institutions and traditions against repressive totalitarianism and its inevitable denial of human values. Thoughtful Americans find themselves uneasy, however, about those policies which must be adopted and those actions which must be taken in the interests of national security, and which at the same time pose a threat to our ideals. This Board has been conscious of these conflicts, presenting as they do some of the grave problems of our times, and has sought to consider them in an atmosphere of decency and safety.

We share the hope that some day we may return to happier times when our free institutions are not threatened and a peaceful and just world order is not such a compelling principal preoccupation. Then security will cease to be a central issue; man's conduct as a

citizen will be measured only in the terms of the requirements of our national society; there will be no undue restraints upon freedom of mind and action; and loyalty and security as concepts will cease to have restrictive implications.

This state of affairs seems not to be a matter of early hope. As we meet the present peril, and seek to overcome it, we must realize that at no time can the interests of the protection of all our people be less than paramount to all other considerations. Indeed, action which in some cases may seem to be a denial of the freedoms which our security barriers are erected to protect, may rather be a fulfillment of these freedoms. For, if in our zeal to protect our institutions against our measures to secure them, we lay them open to destruction, we will have lost them all, and will have gained only the empty satisfaction of a meaningless exercise.

We are acutely aware that in a very real sense this case puts the security system of the United States on trial, both as to procedures and as to substance. This notion has been strongly urged upon us by those who recommend clearance for Dr. J. Robert Oppenheimer, and no doubt a similar view is taken by those who feel he should not be cleared.

If we understand the two points of view, they may be stated as follows: There are those who apprehend that our program for security at this point in history consists of an uneasy mixture of fear, prejudice, and arbitrary judgments. They feel that reason and fairness and justice have abdicated and their places have been taken by hysteria and repression. They, thus, believe that security procedures are necessarily without probity and that national sanity and balance can be served only by a finding in favor of the individual concerned. On the other hand, there is a strong belief that in recent times our Government has been less than unyielding towards the problem of Communism, and that loose and pliable attitudes regarding loyalty and security have prevailed to the danger of our society and its institutions. Thus, they feel that this proceeding presents the unrelinquishable opportunity for a demonstration against Communism, almost regardless of the facts developed about the conduct and sympathies of Dr. Oppenheimer.

We find ourselves in agreement with much that underlies both points of view. We believe that the people of our country can be reassured by this proceeding that it is possible to conduct an investigation in calmness, in fairness, in disregard of public clamor and private pressures, and with dignity. We believe that it has been demonstrated that the Government can search its own soul and the soul of an individual whose relationship to his Government is in question with full protection of the rights and interests of both. We believe that loyalty and security can be examined within the frameworks of the traditional and inviolable principles of American justice.

The Board approached its task in the spirity of inquiry, not that of a trial. The Board worked long and arduously. It has heard forty witnesses including Dr. J. Robert Oppenheimer and compiled over three thousand pages of testimony in addition to having read the same amount of file material.

Dr. Oppenheimer has been represented by counsel, usually four in number, at all times in the course of the proceedings. He has confronted every witness appearing before the Board, with the privilege of cross examination. He is familiar with the contents of every relevant document, which was made available to the Board, except those which under Governmental necessity cannot be disclosed, such as reports of the Federal Bureau of Investigation. He has, in his own words, received patient and courteous consideration at the hands of the Board. The Board has, in the words of his chief counsel, displayed fairness in the conduct of the hearings. And, finally, perhaps it should be said that the investigation has been conducted under the auspices of the responsible agency which has the obligation of decision.

As it considered substance, the Board has allowed sympathetic consideration for the individual to go hand in hand with an understanding of the necessities for a clear, realistic, and rugged attitude towards subversion, possible subversion, or indeed broader implications of security.

It was with all these considerations in mind that we approached our task.

Procedures Governing the Hearing

This proceeding is based upon the Atomic Energy Act of 1946; upon the Atomic Energy Commission's published Security Clearance Procedures, dated September 12, 1950; and Personnel Security Clearance Criteria for Determining Eligibility, dated November 17, 1950; and upon Executive Order No. 10450, dated April 27, 1953.

Subparagraphs (ii) and (iv) of Section 10(b)(5)(B) of the Atomic Energy Act provide that, except as authorized by the Commission in case of emergency, no individual shall be employed by the Commission until the Civil Service Commission or (in certain instances) the Federal Bureau of Investigation shall have made an investigation and report to the Commission on the "character, associations, and loyalty" of such individual.

The AEC published Procedures provide, among other things, for written notice to the individual (1) listing the items of derogatory information and (2) explaining his rights (a) to reply in writing to the information set forth in the Commission's letter, (b) to request a hearing before a Personnel Security Board, (c) to challenge the appointment of the members of the Board for cause, (d) to be present for the duration of the hearing, (e) to be represented by counsel of his own choosing, and (f) to present evidence in his own behalf through witnesses, or by documents, or by both. The Commission's Procedures further provide that in the event of a recommendation for a denial of security clearance, the individual shall be immediately notified of that fact and of his right to request a review of his case by the AEC Personnel Security Review Board, with the right to submit a brief to that Board before the case goes to the General Manager for final determination.

The AEC published Criteria establish the uniform standards to be applied in determining eligibility for clearance. These

Criteria, which, of course, are binding on this Board, provide that it is the responsibility of the Atomic Energy Commission to determine whether the common defense or security will be endangered by granting security clearance.

The Executive Order requires the head of each department and agency of the Government to establish and maintain within his department or agency an effective program to insure that "the employment and retention in employment of any civilian officer or employee within the department or agency is clearly consistent with the interests of the national security." The Executive Order further provides that information on this issue shall relate, but shall not be limited, to certain categories of information set forth in the Order.

Findings

In compliance with Section 4.16(c) of the Commission's Security Clearance Procedures, the Board makes the following specific findings as to the allegations contained in Mr. K. D. Nichols' letter of December 23, 1953 to Dr. J. Robert Oppenheimer:

1. It was reported that in 1940 you were listed as a sponsor of the Friends of the Chinese People, an organization which was characterized in 1944 by the House Committee on Un-American Activities as a Communist-front organization.

The Board concludes that this allegation is true.

Dr. Oppenheimer in his answer replied that he had no recollection of the Friends of the Chinese People, or of what, if any, his connection with this organization was.

The Board had before it a four-page pamphlet (undated) entitled, "American Friends of the Chinese People." The fourth page contains a list of sponsors which includes "Professor J. R. Oppenheimer."

2. It was further reported that in 1940 your name was included on a letterhead of the American Committee for Democracy and Intellectual Freedom as a member of its National Executive Committee. The American Committee for Democracy and Intellectual Freedom was characterized in 1942 by the House Committee on Un-American Activities as a Communist-front which defended Communist teachers, and in 1943 it was characterized as subversive and Un-American by a Special Subcommittee of the House Committee on Appropriations.

The Board concludes that this allegation is true.

Dr. Oppenheimer testified before the Board to having joined the American Committee for Democracy and Intellectual Freedom in 1937. He said that it then stood as a protest against what had happened to intellectuals and professionals in Germany. The Board had before it a letterhead of the "American Committee for Democracy

and Intellectual Freedom." The letterhead contains a printed list of the National Executive Committee, which includes "Professor J. R. Oppenheimer." Dr. Oppenheimer testified that he supposed he accepted membership on this Executive Committee although he did not meet with it.

Dr. Oppenheimer stated in his personnel security questionnaire, which he executed on April 28, 1942, for the purpose of obtaining a clearance for work on the atomic program, that he had joined the "American Committee for Democratic Intellectual Freedom" in 1937 and was still a member on the date the PSQ was executed. He testified that he did not know how long after that he continued to be a member; that, in any event, he was not active thereafter.

> 3. It was further reported that in 1938 you were a member of the Western Council of the Consumers Union. The Consumers Union was cited in 1944 by the House Committee on Un-American Activities as a Communist-front headed by the Communist Arthur Kallet.

The Board concludes that this allegation is true.

Dr. Oppenheimer in his answer stated that for perhaps a year he had been a member of the Western Council of the Consumers Union. In his Personnel Security Questionnaire, which he executed on April 28, 1942, Dr. Oppenheimer stated that he had been a member of the Consumers Union (Western) in 1938-1939.

The Board had before it a photostat of a four-page pamphlet (undated) entitled, "Western Consumers Union," containing a list of Western sponsors, which included the name "Dr. Robert J. Oppenheimer -- Internationally-known Physicist at the University of California."

> 4. It was further reported that you stated in 1943 that you were not a Communist, but had probably belonged to every Communist-front organization on the west coast and had signed many petitions in which Communists were interested.

The Board concludes that this statement was made by Dr. Oppenheimer, and the Board had before it considerable evidence indicating Dr. Oppenheimer's membership in, and association with, Communist-front organizations and activities on the West Coast. However, Dr. Oppenheimer, in his answer, claimed that the quotation was not true and that if he had said anything along the lines quoted, it was a half-jocular overstatement,

The Board had before it a memorandum, dated September 14, 1943, prepared by Lt. Col. John Lansdale, Jr., who was then head of Security and Intelligence for the Manhattan District, which reported "Oppenheimer categorically stated (to General Groves) that he himself was not a Communist and never had been, but stated that he had probably belonged to every Communist-front organization on the West Coast and signed many petitions concerning matters in which Communists were interested."

The Board also had before it a transcript of an interview between Colonel Lansdale and Dr. Oppenheimer on September 12, 1943, which reflected that Colonel Lansdale had asked Dr. Oppenheimer, "You've probably belonged to every front organization on the coast," to which Dr. Oppenheimer replied "Just about." The transcript further records that Dr. Oppenheimer also stated that he thought he would have been considered at one time a fellow-traveler and that "my association with these things was very brief and very intense."

Dr. Oppenheimer in his testimony defined "fellow-traveler" as "someone who accepted part of the public program of the Communist Party, who was willing to work with and associate with Communists, but who was not a member of the Party." He testified to having been a fellow-traveler from late 1936 or early 1937, with his interest beginning to taper off after 1939, and with very little interest after 1942. He further stated that within the framework of his definition of a fellow-traveler, he would not have considered himself as such after 1942.

He further stated that with respect to things that the Communists were doing, in which he still had an interest, it was not until 1946 that it was clear to him that he would not collaborate with Communists no matter how much he sympathized with what they pretended to represent.

5. It was reported that in 1943 and previously you were intimately associated with Dr. Jean Tatlock, a member of the Communist Party in San Francisco, and that Dr. Tatlock was partially responsible for your association with Communist-front groups.

The Board concludes that this allegation is true.

Dr. Oppenheimer in his testimony before this Board admitted having associated with Jean Tatlock from 1936 until 1943. He stated that he saw her only rarely between 1939 and 1943, but admitted that the association was intimate. He admitted having seen Jean Tatlock under most intimate circumstances in June or July of 1943, during the time when he was Director of the Los Alamos Laboratory, and admitted that he knew she had been a Communist and that there was not any reason for him to believe that she was not at that time still a Communist. He named several Communists, Communist functionaries or Communist sympathizers whom he had met through Jean Tatlock, or as a result of his association with her.

6. It was reported that your wife, Katherine Puening Oppenheimer, was formerly the wife of Joseph Dallet, a member of the Communist Party, who was killed in Spain in 1937 fighting for the Spanish Republican Army.

The Board concludes that this allegation is true.

Mrs. Oppenheimer testified that she was married to Joseph Dallet from 1934 until he was killed in Spain, fighting for the Spanish Republican Army in 1937.

Mrs. Oppenheimer admitted knowing that Dallet was a member of the Communist Party and was actively engaging in Communist Party activities.

7. It was further reported that during the period of her association with Joseph Dallet, your wife became a member of the Communist Party. The Communist Party has been designated by the Attorney General as a subversive organization which seeks to alter the form of government of the United States by unconstitutional means, within the purview of Executive Order 9835 and Executive Order 10450.

The Board concludes that this allegation is true.

Mrs. Oppenheimer testified to having been a member of the Communist Party from about 1934 to June 1936 and having engaged in Communist Party activities in the Youngstown, Ohio area.

8. It was reported that your brother Frank Friedman Oppenheimer became a member of the Communist Party in 1936 and has served as a Party organizer and as Educational Director of the Professional Section of the Communist Party in Los Angeles County,

The Board concludes that this allegation is true.

Dr. Frank Friedman Oppenheimer admitted in testimony before the Committee on Un-American Activities of the House of Representative, on June 14, 1949, that he had been a member of the Communist Party from about 1937 until the early spring of 1941. He testified that he joined under the name of "Frank Folsom."

From information before it, the Board concludes that Dr. Frank Oppenheimer had served as a Party organizer and as Educational Director of the Professional Section of the Communist Party in Los Angeles County.

9. It was further reported that your brother's wife Jackie Oppenheimer, was a member of the Communist Party in 1938;

The Board concludes that this allegation is true.

Mrs. Jacquenette Oppenheimer in testimony before the Committee on Un-American Activities, House of Representatives, on June 14, 1949, admitted having been a member of the Communist Party from 1937 until the spring of 1941.

10. and that in August, 1944, Jackie Oppenheimer assisted in the organization of the East Bay branch of the California Labor School.

On the basis of information before it, the Board concludes that this allegation is true.

11. It was further reported that in 1945 Frank and Jackie Oppenheimer were invited to an informal reception at the Russian Consulate, that this invitation was extended by the American-Russian Institute of San Francisco and was for the purpose of introducing famous American scientists to Russian scientists who were delegates to the United Nations Conference on International Organization being held at San Francisco at that time, and that Frank Oppenheimer accepted this invitation.

On the basis of information before it, the Board concludes that this allegation is true.

12. It was further reported that Frank Oppenheimer agreed to give a six-week course on "The Social Implications of Modern Scientific Development" at the California Labor School, beginning May 9, 1946. The American-Russian Institute of San Francisco and the California Labor School have been cited by the Attorney General as Communist organizations within the purview of Executive Order 9835 and Executive Order 10450.

On the basis of information before it, the Board concludes that this allegation is true.

13. It was reported that you have associated with members and officials of the Communist Party, including Isaac Folkoff, Steve Nelson, Rudy Lambert, Kenneth May, Jack Manley, and Thomas Addis.

The Board concludes that this allegation is substantially true.

Dr. Oppenheimer in his answer and in his testimony admitted having associated with Isaac Folkoff, Steve Nelson, Rudy Lambert, Kenneth May and Thomas Addis. He testified that he knew at the time of his association with them that Folkoff, Nelson, Lambert and May were Communist Party functionaries, and that Addis was either a Communist or close to one. He admitted that his associations with these persons continued until 1942. There was no evidence before the Board with respect to an association with Jack Manley.

Dr. Oppenheimer testified that he made contributions to the Spanish War and Spanish Relief through Isaac Folkoff and Thomas Addis. He testified that he had seen Lambert on half a dozen occasions, and that he discussed such contributions once or twice at luncheon with Lambert and Folkoff.

Dr. Oppenheimer testified that Steve Nelson and his family visited his home on several occasions, the last being probably in 1942; that such visits lasted "a few hours;" that he had met Steve Nelson through his (Oppenheimer's) wife since Nelson had befriended her in Paris at the time of Dallet's death; that he had nothing in common with Nelson "except an affection for my wife."

14. It was reported that you were a subscriber to the Daily People's World, a west coast Communist newspaper, in 1941 and 1942.

The Board concludes that this allegation is true.

Dr. Oppenheimer testified that he had subscribed to the People's World "for several years." He could not recall when the subscription expired and stated that he did not believe he had cancelled the subscription. He testified that he knew the Daily People's World was the West Cost Communist newspaper.

15. It was reported in 1950 that you stated to an agent of the Federal Bureau of Investigation that you had in the past made contributions to Communist-front organizations, although at the time you did not know of Communist Party control or extent of infiltration of these groups. You further stated to an agent of the Federal Bureau of Investigation that some of these contributions were made through Isaac Folkoff, whom you knew to be a leading Communist Party functionary, because you had been told that this was the most effective and direct way of helping these groups.

The Board finds that Dr. Oppenheimer made the statements attributed to him by the Federal Bureau of Investigation.

The Board concludes that Dr. Oppenheimer in the past made contributions to Communist-front organizations and that some of these contributions were made through Isaac Folkoff, a leading Communist Party functionary.

Dr. Oppenheimer testified that he contributed to Spanish causes through Communist Party channels from the winter of 1937-38 until early in 1942. He said that he had contributed more than $500 and less than $1,000 each year during this period. He testified that he made the contributions in cash and, in explaining how these contributions came to an end, he said (in referring to Pearl Harbor) that he "didn't like to continue a clandestine operation of any kind at a time when I saw myself with the possibility or prospect of getting more deeply involved in the war."

Dr. Oppenheimer in his answer admitted making the contributions through Thomas Addis and Isaac Folkoff. He testified that he knew Addis was a Communist or very close to a Communist. He knew that Folkoff was connected with the Communist Party. In addition, Dr. Oppenheimer admitted having contributed about $100 in cash to the Strike Fund of one of the major strikes of "Bridges' Union" about 1937 or 1938.

16. It was reported that you attended a housewarming party at the home of Kenneth and Ruth May on September 20, 1941, for which there was an admission charge for the benefit of The People's World, and that at this party you were in the company of Joseph W. Weinberg and Clarance Hiskey, who were alleged to be members of the Communist Party and to have engaged

in espionage on behalf of the Soviet Union. It was further reported that you informed officials of the United States Department of Justice in 1942 that you had no recollection that you had attended such a party, but that since it would have been in character for you to have attended such a party, you would not deny that you were there.

The Board concludes on the basis of information before it, that it was probable that Dr. Oppenheimer attended "the housewarming party" at the home of Kenneth and Ruth May. The Board concludes that Dr. Oppenheimer made the statements to the U. S. Department of Justice officials attributed to him.

Dr. Oppenheimer did not deny having attended such a party and testified that he knew Kenneth May. He denied knowing Hiskey but testified that he, Oppenheimer, was at parties at which Weinberg was present.

17. It was reported that you attended a closed meeting of the professional section of the Communist Party of Alameda County, California, which was held in the latter part of July or early August, 1941, at your residence, 19 Kenilworth Court, Berkeley, California, for the purpose of hearing an explanation of a change in Communist Party policy. It was further reported that you denied that you attended such a meeting and that such a meeting was held in your home.

The Board is of the opinion that the evidence with respect to this meeting is inconclusive. The Board finds that Dr. Oppenheimer did deny that he attended such a meeting and that such a meeting was held in his home.

18. It was reported that you stated to an agent of the Federal Bureau of Investigation in 1950 that you attended a meeting in 1940 or 1941, which may have taken place at the home of Haakon Chevalier, which was addressed by William Schneiderman, whom you knew to be a leading functionary of the Communist Party. In testimony in 1950 before the California State Senate Committee on Un-American Activities, Haakon Chevalier was identified as a member of the Communist Party in the San Francisco area in the early 1940's.

The Board finds that Dr. Oppenheimer made the statements attributed to him by the Federal Bureau of Investigation.

Dr. Oppenheimer testified that on December 1, 1940, he attended an evening meeting at the home of Haakon Chevalier at which perhaps 20 people were present and at which William Schneiderman, Secretary of the Communist Party in California, gave a talk about the Communist Party line. He testified that he thought that possibly Isaac Folkoff, Dr. Addis and Rudy Lambert were there.

He also testified that "after the end of 1940" he attended a similar meeting at the home of Louise Bransten, "a Communist sympathizer," at which some of the same people were present and at which Schneiderman also spoke and expounded the Communist Party line.

Dr. Oppenheimer testified that sometime between 1937 and 1939 as a "guest" he attended a Communist Party meeting at the home of his brother, Frank.

19. It was reported that you have consistently denied that you have ever been a member of the Communist Party. It was further reported that you stated to a representative of the Federal Bureau of Investigation in 1946 that you had a change of mind regarding the policies and politics of the Soviet Union about the time of the signing of the Soviet-German Pact in 1939. It was further reported that during 1950 you stated to a representative of the Federal Bureau of Investigation that you had never attended a closed meeting of the Communist Party; and that at the time of the Russo-Finnish War and the subsequent break between Germany and Russia in 1941, you realized the Communist Party infiltration tactics into the alleged anti-Fascist groups and became fed up with the whole thing and lost what little interest you had.

Dr. Oppenheimer testified that he had never been a member of the Communist Party. The Board finds that Dr. Oppenheimer made the statements attributed to him by the Federal Bureau of Investigation.

It was further reported, however, that:

19(a). Prior to April, 1942, you had contributed $150 per month to the Communist Party in the San Francisco Area, and that the last such payment was apparently made in April, 1942, immediately before your entry into the atomic bomb Project.

The Board concludes on the basis of testimony and other information before it that Dr. Oppenheimer made periodic contributions through Communist Party functionaries to the Communist Party in the San Francisco area in amounts aggregating not less than $500 nor more than $1,000 a year during a period of approxima ely four years ending in April 1942. As of April 1942, Dr. Oppenheimer had been for several months participating in Government atomic energy research activities. He executed a questionnaire for Government clearance on 28 April 1942, and subsequently assumed full-time duties with the atomic energy project.

19(b). During the period 1942-1945 various officials of the Communist Party, including Dr. Hannah Peters, organizer of the Professional Section of the Communist Party, Alameda County, California, Bernadette Doyle, secretary of the Alameda County Communist Party, Steve Nelson, David Adelson, Paul Pinsky, Jack Manley, and Katrina Sandow, are reported to have made statements indicating that you were then a member of the Communist Party; that you could not be active in the Party at that time; that your name should be removed from the Party mailing list and not mentioned in any

way; that you had talked the atomic bomb question over with Party members during this period; and that several years prior to 1945 you had told Steve Nelson that the Army was working on an atomic bomb.

The Board finds that during the period 1942-45, Dr. Hannah Peters, Bernadette Doyle, Steve Nelson, Jack Manley and Katrina Sandow made statements indicating that Dr. Oppenheimer was then a member of the Communist Party; and that the other statements attributed to officials of the Communist Party in this allegation were made by one or more of them. The Board does not find on the basis of information available to it that such statements were made by David Adelson and Paul Pinsky.

19(c). You stated in August of 1943 that you did not want anybody working for you on the Project who was a member of the Communist Party, since "one always had a question of divided loyalty" and the discipline of the Communist Party was very severe and not compatible with complete loyalty to the Project. You further stated at that time that you were referring only to present membership in the Communist Party and not to people who had been members of the Party. You stated further that you knew several individuals then at Los Alamos who had been members of the Communist Party. You did not, however, identify such former members of the Communist Party to the appropriate authorities. It was also reported that during the period 1942-1945 you were responsible for the employment on the atomic bomb Project of individuals who were members of the Communist Party or closely associated with activities of the Communist Party, including Giovanni Rossi Lomanitz, Joseph W. Weinberg, David Bohm, Max Bernard Friedman, and David Hawkins. In the case of Giovanni Rossi Lomanitz, you urged him to work on the Project, although you stated that you knew he had been very much of a "Red" when he first come to the University of California and that you emphasized to him that he must forego all political activity if he came on to the Project. In August, 1943, you protested against the termination of his deferment and requested that he be returned to the Project after his entry into the military service.

The Board concludes that Dr. Oppenheimer did state in 1943 that he did not want anybody working for him on the Project who was a member of the Communist Party since "one always had a question of divided loyalty" and the discipline of the Communist Party was very severe and not compatible with complete loyalty to the Project. He further stated at that time he was referring only to present membership in the Communist Party and not to people who had been members of the Party. He stated further that he knew several individuals then at Los Alamos who had been members of the Communist Party. He did not, however, identify such former members of the Communist Party to the appropriate authorities.

The Board concludes that Dr. Oppenheimer was responsible for the employment on the atom bomb Project of Giovanni Rossi Lomanitz at Berkeley and David Hawkins at Los Alamos.

The Board concludes that Dr. Oppenheimer asked for the transfer of David Bohm to Los Alamos, although Bohm was closely associated with the Communist Party. In his answer, Dr. Oppenheimer admitted that while at Berkeley he had assigned David Bohm to a problem of basic science having a bearing on atomic research.

Dr. Oppenheimer testified that he understood that Hawkins had left-wing associations; and that Hawkins "talked about philosophy in a way that indicated an interest and understanding and limited approval anyway of Engels."

The Board does not conclude that Dr. Oppenheimer was responsible for the employment of Friedmann or Weinberg on the Atomic Energy Program.

Dr. Oppenheimer testified that Joseph W. Weinberg was a graduate student of his; that he had heard that Weinberg had been a member of the Young Communist League before coming to Berkeley and the Board had before it a transcript of a conversation with Dr. Oppenheimer indicating that at least by August 1943, he knew Weinberg to be a member of the Communist Party and that he "suspected that before but was not sure." Weinberg gave Oppenheimer as a reference at the time he (Weinberg) obtained employment at the Radiation Laboratory on April 22, 1943.

Dr. Oppenheimer testified that he asked General Groves for the transfer of David Bohm to Los Alamos in 1943, but was told by General Groves that he could not be transferred since he had relatives in Nazi Germany. In March, 1944, after a conversation with Bohm at Berkeley, (a surveillance report indicated that the talk took place at a sidewalk meeting) he checked with the Security Officer at Los Alamos to see whether the objections to Bohm still obtained.

Dr. Oppenheimer testified that he thought that in 1946 or 1947 he helped Bohm get a job as Assistant Professor of Physics at Princeton. He testified that he happened to meet Bohm and Lomanitz on the street in Princeton in 1949 just prior to their testifying before the House Committee on Un-American Activities; that he said that "they should tell the truth"; that he later saw Bohm at Princeton and attended a farewell party for him in Princeton; that he would, if asked, have written a letter of recommendation for Bohm as a competent physicist in connection with a job in Brazil, although he knew and was worried about Bohm having pleaded the Fifth Amendment when he testified.

The Board finds that Dr. Oppenheimer did urge Lomanitz to work on the project although he knew he had been very much a "Red" when he first came to the University of California and, in fact, during his attendance at the University, and that Dr. Oppenheimer later stated to a Manhattan District official that

he had warned Lomanitz that he must forego all political activity if he came to the project. The Board finds further that in August, 1943, Dr. Oppenheimer protested against the termination of Lomanitz' deferment and urgently requested that he be returned to the Project after his entry into the military service. It appears from the testimony that Dr. Oppenheimer first learned of the impending induction of Lomanitz in a letter from Dr. E. U. Condon who wrote to him "About it in a great sense of outrage."

20. It was reported that you stated to representatives of the Federal Bureau of Investigation on September 5, 1946, that you had attended a meeting in the East Bay and a meeting in San Francisco at which there were present persons definitely identified with the Communist Party. When asked the purpose of the East Bay meeting and the identity of those in attendance, you declined to answer on the ground that this had no bearing on the matter of interest being discussed.

The Board concludes that this allegation is true. The Board finds that Dr. Oppenheimer did attend a meeting in the East Bay and a meeting in San Francisco (see Item 18 above) at which there were present persons definitely identified with the Communist Party and that when he was asked about this meeting by representatives of the Federal Bureau of Investigation on September 5, 1946, he declined to answer on the ground that this had no bearing on the matter of interest being discussed.

The Board finds that Dr. Oppenheimer advised representatives of the F.B.I. of this meeting in a subsequent interview in 1950.

21. It was reported that you attended a meeting at the home of Frank Oppenheimer on January 1, 1946, with David Adelson and Paul Pinsky, both of whom were members of the Communist Party. It was further reported that you analyzed some material which Pinsky hoped to take up with the Legislative Convention in Sacramento, California.

The Board concludes that this allegation is true.

22. It was reported in 1946 that you were listed as Vice Chairman on the letterhead of the Independent Citizens Committee of the Arts, Sciences, and Professions, Inc., which has been cited as a Communist-front by the House Committee on Un-American Activities.

The Board concludes that this allegation is true, although the Board finds that Dr. Oppenheimer advised the organization in a letter on October 11, 1946, that he was not in accord with its policy and wished to resign. He wrote again on December 2, 1946, insisting upon resignation. The resignation was accepted on December 10, 1946.

23. It was reported that prior to March 1, 1943, possibly three months prior, Peter Ivanov, Secretary at the Soviet Consulate, San Francisco, approached George Charles Eltenton for the purpose of obtaining information regarding work being done at the Radiation Laboratory for the use of Soviet scientists; that George Charles Eltenton subsequently requested Haakon Chevalier to approach you concerning this matter; that Haakon Chevalier thereupon approached you, either directly or through your brother, Frank Friedman Oppenheimer, in connection with this matter; and that Haakon Chevalier finally advised George Charles Eltenton that there was no chance whatsoever of obtaining the information. It was further reported that you did not report this episode to the appropriate authorities until several months after its occurrence; that when you initially discussed this matter with the appropriate authorities on August 26, 1943, you did not identify yourself as the person who had been approached, and you refused to identify Haakon Chevalier as the individual who had made the approach on behalf of George Charles Eltenton; and that it was not until several months later, when you were ordered by a superior to do so, that you so identified Haakon Chevalier. It was further reported that upon your return to Berkeley following your separation from the Los Alamos Project, you were visited by the Chevaliers on several occasions; and that your wife was in contact with Haakon and Barbara Chevalier in 1946 and 1947.

The Board concludes that this allegation is substantially true.

The Board had before it a recording of a conversation between Dr. Oppenheimer and Lt. Col. Boris T. Pash, War Department Intelligence Officer, who had the responsibility for investigating subversive activities at the Radiation Laboratory, University of California at Berkeley. This conversation took place on August 26, 1943, at the Radiation Laboratory.

It was on this occasion that Dr. Oppenheimer reported the incident to Government authorities. He named Eltenton but refused to identify Chevalier. He also stated that the unnamed contact (Chevalier) had approached three persons on the atomic project and in the course of the interview mentioned other factors, such as the use of microfilm or other means and the involvement of the Russian Consulate.

The Board also had before it a transcript of a conversation between Dr. Oppenheimer and Lt. Col. Lansdale which records that on September 12, 1943, Dr. Oppenheimer again refused to name Chevalier but reported the involvement of three others.

It was not until December, 1943, that Dr. Oppenheimer, after being told by General Groves that he would be ordered to divulge the identity of the contact, reported the name of Chevalier.

However, the record shows that having been told of the identity of Chevalier by Dr. Oppenheimer, the Manhattan District officials were still of the opinion that Chevalier had contacted three employees on the atomic project.

Dr. Oppenheimer, in his answer, stated that his friend, Haakon Chevalier, with his wife, visited him at his home on Eagle Hill probably in early 1943. He stated further that during the visit Chevalier came into the kitchen and told him that George Eltenton had spoken to him of the possibility of transmitting technical information to Soviet scientists. Dr. Oppenheimer said that he made some strong remark to the effect that this sounded terribly wrong to him, and the discussion ended there.

Dr. Oppenheimer's answer further states that nothing in his long-standing friendship would have lead him to believe that Chevalier was actually seeking information, and he was certain that Chevalier had no idea of the work on which Dr. Oppenheimer was engaged.

Dr. Oppenheimer testified that the detailed story of the Chevalier incident which he told to Colonel Pash on August 26, 1943, and affirmed to Colonel Lansdale on September 12, 1943, was false in certain material respects. Dr. Oppenheimer testified that this story was "a cock-and-bull story;" that "the whole thing was a pure fabrication except for the one name, Eltenton." He said that his only explanation for lying was that he "was an idiot" and he "was reluctant to mention Chevalier" and "no doubt somewhat reluctant to mention myself." He admitted on cross examination, however, that if the story he told Colonel Pash had been true, it would have shown that Chevalier "was deeply involved;" that it was not just a casual conversation; that Chevalier was not an innocent contact, and that it was a criminal conspiracy.

Dr. Oppenheimer admitted that if this story to Colonel Pash had been true, it made things look very bad for both Chevalier and himself. He acknowledged that he thought the request for information by Eltenton was "treasonable." He admitted that he knew when he talked to Colonel Pash that his falsification impeded Colonel Pash's investigation.

Dr. Oppenheimer testified that in June or July of 1946 shortly after Chevalier was interviewed by the F.B.I. about the Eltenton-Chevalier Incident, Chevalier came to Oppenheimer's home in Berkeley and told Oppenheimer about the interview; that Chevalier said the F.B.I. had pressed him about whether he talked to anyone besides Oppenheimer; that quite awhile later Dr. Oppenheimer was interviewed by the F.B.I. about the same matter, and at this time he knew from Chevalier substantially what Chevalier had said to the F.B.I. about the incident.

Dr. Oppenheimer testified that he recalled getting a letter from Chevalier in 1950 asking him about Dr. Oppenheimer's testimony before the House Un-American Activities Committee concerning the Chevalier-Eltenton incident. He responded, giving

Chevalier a summary of what he, Dr. Oppenheimer, had testified. This letter was later used by Chevalier in support of his application for a passport. Dr. Oppenheimer further testified that at about that time, Chevalier came to Princeton and spent two days with Dr. Oppenheimer, discussing Chevalier's personal affairs and that he also then mentioned the matter of his passport. Dr. Oppenheimer said that on this occasion he recommended to Chevalier a lawyer named Joseph Fanelli, who, cross examination disclosed, was the attorney who represented Joseph Weinberg at his trial for perjury. Dr. Oppenheimer testified that he did not know Mr. Fanelli at this time but he had represented Frank Oppenheimer at his appearance before the House Committee on Un-American Activities.

Dr. Oppenheimer testified further that in December of 1953, when he and Mrs. Oppenheimer were in Paris, they had dinner with Dr. and Mrs. Chevalier and, on the following day, went with the Chevaliers to visit a Dr. Malraux. According to Dr. Oppenheimer, Dr. Malraux had given a speech at a "Spanish Relief" meeting in California at which Chevalier presided in about 1938. Dr. Oppenheimer said that since that time, Malraux had undergone "rather major political changes;" that "Malraux became a violent supporter of DeGaulle and his great brainman and deserted politics and went into purely philosophic and literary work." It appears also that subsequent to his meeting with Dr. Oppenheimer in Paris in December 1953, Chevalier wrote a letter to an official of the United States Embassy in Paris, reading as follows:

> "My friend -- and yours -- Robert Oppenheimer, gave me your name when he was up for dinner here in our apartment early last December, and urged me to get in touch with you if a personal problem of mine which I discussed with him became pressing. He gave me to understand that I could speak to you with the same frankness and fullness as I have with him, and he with me, during the fifteen years of our friendship.
>
> "I should not have presumed to follow up such a suggestion if it had come from anyone else. But, as you know, Opje never tosses off such a suggestion lightly.
>
> "If you are in Paris, or will be in the near future, I should, then, like to see you informally and discuss the problem.
>
> "On rereading what I have written, I have a feeling that I have made the thing sound more formidable than it really is. It's just a decision that I have to make, which is fairly important to me, and which Opje in his grandfatherly way suggested that I shouldn't make before consulting you.
>
> "Very sincerely, Haakon Chevalier."

Dr. Oppenheimer testified that the problem which was bothering Chevalier and his wife was that Chevalier was employed as a translator for UNESCO, and he understood that if he continued this work as an American citizen, he would have to be cleared after investigation, and he was doubtful as to whether he would be cleared. He did not wish to renounce his American citizenship but did wish to keep his job, and he was in a conflict about it. Dr. Oppenheimer in his testimony denied going to the American Embassy to assist Dr. Chevalier in getting a passport to return to the United States although he admitted having had lunch with the official in question.

Dr. Oppenheimer also denied discussing with the official in question or anyone else the matter of Chevalier's passport.

Dr. Oppenheimer in his testimony has stated that his association with Chevalier has continued and that he still considers him to be his friend.

24. It was reported that in 1945 you expressed the view that "there is a reasonable possibility that it (the hydrogen bomb) can be made," but that the feasibility of the hydrogen bomb did not appear, on theoretical grounds, as certain as the fission bomb appeared certain, on theoretical grounds, when the Los Alamos Laboratory was started; and that in the Autumn of 1949 the General Advisory Committee expressed the view that "an imaginative and concerted attack on the problem has a better than even chance of producing the weapon within five years." It was further reported that in the Autumn 1949, and subsequently, you strongly opposed the development of the hydrogen bomb: (1) on moral grounds, (2) by claiming that it was not feasible, (3) by claiming that there were insufficient facilities and scientific personnel to carry on the development, and (4) that it was not politically desirable. It was further reported that even after it was determined, as a matter of national policy, to proceed with development of a hydrogen bomb, you continued to oppose the project and declined to cooperate fully in the project. It was further reported you departed from your proper role as an advisor to the Commission by causing the distribution, separately and in private, to top personnel at Los Alamos of the majority and minority reports of the General Advisory Committee on development of the hydrogen bomb for the purpose of trying to turn such top personnel against the development of the hydrogen bomb. It was further reported that you were instrumental in persuading other outstanding scientists not to work on the hydrogen bomb project, and that the opposition to the hydrogen bomb, of which you are most experienced, most powerful, and most effective member, has definitely slowed down its development.

In order to assess the influences of Dr. Oppenheimer on the thermonuclear program, it has been necessary for the Board not only to consider the testimony but also to examine many documents

and records, most of which are classified. Without disclosing the contents of classified documents, the Board makes the following findings, which it believes to be a sufficient reference to this allegation.

The Board confirms that in 1945 Dr. Oppenheimer expressed the view that "'there is reasonable possibility that it (the hydrogen bomb) can be made,' but that the feasibility of the hydrogen bomb did not appear, on theoretical grounds, as certain as the fission bomb appeared certain, on theoretical grounds, when the Los Alamos Laboratory was started; and that in August of 1949, the General Advisory Committee expressed the view that 'an imaginative and concerted attack on the problem has a better than even chance of producing the weapon within five years.'"

With respect to Dr. Oppenheimer's attitude and activities in relation to the hydrogen bomb in World War II, the evidence shows that Dr. Oppenheimer during this period had no misgivings about a program looking to thermonuclear development and, indeed, during the latter part of the war, he recorded his support of prompt and vigorous action in this connection. When asked under cross examination whether he would have opposed dropping an H-Bomb on Hiroshima, he replied that "It would make no sense," and when asked "Why?" replied, "The target is too small." He testified further under cross examination that he believed he would have opposed the dropping of an H-Bomb on Japan because of moral scruples although he did not oppose the dropping of an A-Bomb on the same grounds. During the post-war period, Dr. Oppenheimer favored, and in fact urged, continued research in the thermonuclear field and seemed to express considerable interest in results that were from time to time discussed with him. However, he was aware that the efforts being put forth in this endeavor were relatively meager and he knew that if research were continued at the same pace, there would be little likelihood of success for many years. Testimony in this connection indicated that there was a feeling on his part that it was more important to go forward with a program for the production of a wider range of atomic bombs.

The Board finds further that in the Autumn of 1949, and subsequently, Dr. Oppenheimer strongly opposed the development of the hydrogen bomb on moral grounds; on grounds that it was not politically desirable; he expressed the view that there were insufficient facilities and scientific personnel to carry on the development without seriously interfering with the orderly development of the program for fission bombs; and until the late spring of 1951, he questioned the feasibility of the hydrogen bomb efforts then in progress.

Dr. Oppenheimer testified that what he was opposing in the fall of 1949 was only a "crash program" in the development and production of thermonuclear weapons. In this connection, Dr. Oppenheimer contended that the main question relating to thermonuclear weapons presented to the GAC at its meeting of October 29, 1949, was whether or not the United States should

undertake such a crash program. The Board does not believe that Dr. Oppenheimer was entirely candid with the Board in attempting to establish this impression. The record reflects that Dr. Oppenheimer expressed the opinion in writing that the "super bomb should never be produced," and that the commitment to this effect should be unqualified. Moreover, the alternatives available to the GAC were not a choice between an "all-out effort" and no effort at all; there was a middle course which might have been considered.

The Board further concludes that after it was determined, as a matter of national policy (January 31, 1950) to proceed with development of a hydrogen bomb, Dr. Oppenheimer did not oppose the project in a positive or open manner, nor did he decline to cooperate in the project. However, Dr. Oppenheimer is recognized in scientific circles as one of the foremost leaders in the atomic energy field and he has had considerable influence on the "policy direction" of the atomic program. The Board finds that his views in opposition to the development of the H-Bomb as expressed in 1949 became widely known among scientists, and since he did not make it known that he had abandoned these views, his attitude undoubtedly had an adverse effect on recruitment of scientists and the progress of the scientific effort in this field. In other words, the Board finds, that if Dr. Oppenheimer had enthusiastically supported the thermonuclear program either before or after the determination of national policy, the H-bomb project would have been pursued with considerably more vigor, thus increasing the possibility of earlier success in this field.

The Board finds that Dr. Oppenheimer was not responsible for the distribution, separately and in private, to top personnel at Los Alamos of the majority and minority reports of the General Advisory Committee on development of the hydrogen bomb, but that such distribution was made on the direction of the then General Manager of the Atomic Energy Commission, Carroll L. Wilson, apparently in order to prepare the personnel at Los Alamos to discuss the matter with the Chairman of the Joint Committee on Atomic Energy of the Congress.

The Board does not find that Dr. Oppenheimer urged other scientists not to work on the program. However, enthusiastic support on his part would perhaps have encouraged other leading scientists to work on the program.

Because of technical questions involved, the Board is unable to make a categorical finding as to whether the opposition of the hydrogen bomb "has definitely slowed down its development." The Board concludes that the opposition to the H-bomb by many persons connected with the atomic energy program, of which Dr. Oppenheimer was the "most experienced, most powerful and most effective member" did delay the initiation of concerted effort which led to the development of a thermonuclear weapon.

General Considerations

We do not believe that our findings with respect to the letter of notification provide a full and automatic answer to the categorical question posed to us in these proceedings. Only the dimensions of the problem have perhaps been defined. On the one hand, we find no evidence of disloyalty. Indeed, we have before us much responsible and positive evidence of the loyalty and love of country of the individual concerned. On the other hand, we do not believe that it has been demonstrated that Dr. Oppenheimer has been blameless in the matter of conduct, character and association.

We could in good conscience, we believe, conclude our difficult undertaking by a brief, clear and conclusive recommendation to the General Manager of the Commission in the following terms: There can be no tampering with the national security, which in times of peril must be absolute, and without concessions for reasons of admiration, gratitude, reward, sympathy, or charity. Any doubts whatsoever must be resolved in favor of the the national security. The material and evidence presented to this Board leave reasonable doubts with respect to the individual concerned. We, therefore, do not recommend reinstatement of clearance.

It seemed to us that an alternative recommendation would be possible, if we were allowed to exercise mature practical judgment without the rigid circumscription of regulations and criteria established for us.

In good sense, it could be recommended that Dr. Oppenheimer simply not be used as a consultant, and that therefore there exists no need for a categorical answer to the difficult question posed by the regulations, since there would be no need for access to classified material.

The Board would prefer to report a finding of this nature. We have had a desire to reconcile the hard requirements of security with the compelling urge to avoid harm to a talented citizen.

The Board questioned why the Commission chose to revoke Dr. Oppenheimer's clearance and did not follow the alternative course of declining to make use of his services, assuming it had serious questions in the area of security. To many, this would seem the preferable line of action. We think that the answer of the Commission to this question is pertinent to this recital. It seemed clear that other agencies of Government were extending clearance to Dr. Oppenheimer on the strength of AEC clearance, which in many quarters is supposed to be an approval of the highest order. Furthermore, it was explained that without the positive act of withdrawal of access, he would continue to receive classified reports on Atomic Energy activities as a consultant, even though his services were not specifically and currently engaged. Finally it is said that were his clearance continued, his services would be available to, and probably would be used by, AEC contractors. It is noted that most AEC work is carried on by contractors.

Withdrawal of clearance and Dr. Oppenheimer's request for a hearing precipitated this proceeding.

In view of the fact that we must address ourselves to security, we feel constrained to examine some of the great issues and problems brought into focus by the case. Many of these are perhaps more important than the outcome of this inquiry. We believe their examination is a necessary precondition to its disposition on security grounds.

What, within the framework of this case, is meant by loyalty?

Because of widespread confusions and misapprehensions about the security system of the United States, the Board feels that it must state some considerations with respect to loyalty. If a person is considered a security risk in terms of loyalty, the fact or possibility of active disloyalty is assumed, which would involve conduct giving some sort of aid and comfort to a foreign power. The Communist Party is an international conspiracy organized in support of the Soviet Union. It should then be clear that (1) a member of the Communist Party is automatically barred from a position of trust with the United States Government; (2) a fellow-traveler must be declared ineligible for such a position of trust -- such a person being described as one who perhaps may not be subject to party discipline, but who is sufficiently close to the Party, or sympathetic with its aims, purposes and methods that danger inheres in the situation; (3) any person whose absolute loyalty to the United States is in question, aside from present or former Communist affiliations or associations, should be rejected for Government service; (4) a person whose former status would be encompassed in one, two or three above, has the burden of proof of change in position and attitude which must be so clearly borne by him as to leave no reasonable doubt in the minds of those who are called upon to make a Governmental decision in the case. If he fails in this demonstration, he must be considered a security risk and denied access to classified information.

One of the important issues presented in cases of this sort is that of rehabilitation

Stated in the context of this proceeding, must we accept the principle that once a Communist, always a Communist, once a fellow-traveler, always a fellow-traveler? Can an individual who has been a member of the Communist Party, or closely enough associated with it to make the difference unimportant at a later time, so comport himself personally, so clearly have demonstrated a renunciation of interest and sympathy, so unequivocally have displayed a zeal for his country and its security as to overcome the necessary presumptions of security risk? We, as a Board, firmly believe that this can be the case, and, if we may be permitted something in the nature of a dictum, we believe that this principle should be a part of the security policy of the United States Government. The necessary but harsh requirements of security should not deny a man the right to have made a mistake if its recurrence is so remote a possibility as to permit a comfortable prediction as to the sanity and correctness of future conduct.

This Board has been conscious of the atmosphere of the time in which Dr. Oppenheimer's clear-cut Communist affiliations occurred. We have considered his activities against the background of the pervasive disillusionment among many of our people arising out of the effects of the great depression and the perhaps normal tendency of a humanitarian to turn to an organization which seemed to him to be espousing primarily humanitarian causes. We recognize what may have seemed to be at the time a beckoning towards a better social order. We know that many academic people and other intellectuals, honest and moral though they were, misinterpreted the talk, aims and purposes of the Communist Party and its affiliated organizations. We are aware that the fact that the Soviet Union was an ally during some of those years cannot be overlooked. This intellectual exercise has, we think, not been inappropriate because we recognize that 1943 conduct cannot be judged solely in the light of 1954 conditions. At the same time, it must be remembered that standards and procedures of 1943 should not be controlling today.

Another vital question is, can an individual be loyal to the United States and, nevertheless, be considered a security risk?

Because the security interests of this country may be endangered by involuntary act, as well as by positive conduct of a disloyal nature, personal weaknesses of an individual may constitute him a security risk. These would include inordinate use of alcohol or drugs, personal indiscretion (in the sense of careless talk), homosexuality, emotional instability, tendency to yield to pressures of others, unusual attachment for foreign systems. The presence of any of these items would support a finding of security risk, even though in every case accompanied by a deep love of country.

There remains also an aspect of the security system which perhaps has had insufficient public attention. This is the protection and support of the entire system itself. It must include an understanding and an acceptance of security measures adopted by responsible Government agencies. It must include an active cooperation with all agencies of Government properly and reasonably concerned with the security of our country. It must involve a subordination of personal judgment as to the security status of an individual as against a professional judgment in the light of standards and procedures when they have been clearly established by appropriate process. It must entail a wholehearted commitment to the preservation of the security system and the avoidance of conduct tending to confuse or obstruct.

The Board would assert the right of any citizen to be in disagreement with security measures and any other expressed policies of Government. This is all a part of the right of dissent which must be preserved for our people. But the question arises whether an individual who does not accept and abide by the security system should be a part of it.

In this connection, we should acknowledge that in the early war years very few people were aware of the full implications of security or security measures which needed to be undertaken. Even many of those in the military services found themselves for a time in a new field. This was a new concept under strange and alien pressures. We believe that no person should now be held accountable for lack of full knowledgeability in the early years of the war. However, those who have been associated with it during the war years and subsequently and who have been exposed repeatedly to security measures, should not fail to understand the need for their full support of the system.

Another major question posed by these proceedings is whether we should take calculated risks where the national security is involved

It has been urged upon us that where there is lingering doubt about the security status of an individual in the absence of a finding of disloyalty or a tendency towards indiscretion, we should take a calculated risk in granting clearance to such an individual if he is a man of great attainments and capacity and has rendered outstanding services.

Within the framework of our national philosophy which rests in large part upon the declaration that all men are equal before the bar of justice, can we apply one test to an individual, however brilliant his capacities and however magnificent his contributions, and another test to an individual with more mundane capabilities and lesser contributions? In other words, can a different test for security purposes be justified in the case of the brilliant technical consultant than in the case of the stenographer or clerk? It seems to us that such a distinction can be justified only on the ground of critical national need and that otherwise there can be but one standard for all.

We acknowledge that the national necessity may at times require the taking of a calculated risk. Such a calculated risk was taken in the employment and retention of Dr. Oppenheimer as Director of the Los Alamos Laboratory during the war years, on the ground of the overriding need for his services. The officer-in-charge has said that, had he found the risk becoming a danger, he would have felt impelled to open up the whole project and throw security to the winds rather than lose the talents of the individual. Again, wartime exigencies demanded the use of Nazi scientists before the issues with Germany were settled.

What we have learned in this inquiry makes the present application of this principle inappropriate in the instant case. Notwithstanding the undoubted and unparalleled contributions of Dr. Oppenheimer to the atomic energy program, it appears that his services as a consultant were used by the Atomic Energy Commission during the entire year of 1953 for a period approximating only two and one-half days' time. We conclude, therefore, that our recommendation should not be based upon such principle, overriding all other considerations.

Another major issue which has been highlighted by this inquiry is whether a moral principle akin to double jeopardy in the traditional legal sense should have a place in the jurisprudence of security.

We properly ask ourselves the question: How many times may the same circumstances of a man's life be examined with a view toward determination of his security status? Once a responsible agency of the Government has made an evaluation, should this not be a bar to later and similar consideration by the same or another agency in the absence of newly discovered evidence or developments. This is an important consideration and the Board has undertaken to examine it with care.

It must be made clear to the public by the Government that its employees and consultants are not to be subject to repeated and capricious reviews of their loyalty or security status. In General, this Board believes that responsible prior clearance should be given great weight and should be virtually considered a settled matter in cases where there is manifestly no new material or developments of consequence. We would not urge this as an absolute principle, however, for the reason that the criminal law concept referred to is for the protection of the individual whereas security measures are for the protection of the country, whose interests should never be foreclosed.

There seems to be a widespread view that such a principle should apply in the case at hand. It has been suggested that the clearance by the Manhattan Engineering District and the subsequent action of the AEC in 1947 should be controlling. We believe this not to be sound.

In the first place, we must acknowledge the important difference between an administrative review of files not involving the personal appearance of the employee and of which he is probably not aware, and a hearing before the Board at which the employee appears and at which testimony is taken. This is the first occasion of review of this case by a Personnel Security Board. Indeed, this is the only time that all of the available evidence regarding Dr. Oppenheimer has been correlated and presented in a package. This latter fact suggests the second reason why Dr. Oppenheimer is not being placed in double jeopardy in a moral sense by this proceeding. It was necessary to the national security that material information not considered in previous clearances be studied.

Third, new developments have occurred since the granting of previous clearances. Among these are changed national and international circumstances and new security standards and criteria which have been published in the interim. We refer specifically to the AEC criteria published since 1947 and the Executive Order of the President of April 27, 1953.

It must be recalled that the Manhattan District criteria were primarily loyalty and discretion. Such records as are available with respect to the AEC clearance in 1947 indicate that in general it was based in large part upon the earlier clearance by the Manhattan Engineering District, upon a finding of loyal service to the country, and the risk to the program in the loss of services of the individual.

Fourth, viewed against the background of earlier history, the conduct of the individual subsequently to 1947 has been such as to raise questions of security risk.

Another major issue prompted by these proceedings concerns itself with the extent of the right of a citizen to continued employment by his Government because of loyal and distinguished accomplishment in Government service.

There are those who seem deeply convinced that Dr. Oppenheimer has a right to continued employment, in view of his previous contributions and in the light of his brilliant capabilities. Citizens of this country have many inalienable rights, but it is clear that Government service is a privilege and not a right. This principle was simply, but effectively, stated by Oliver Wendell Holmes:

> ". . . The petitioner may have a constitutional right to talk politics, but he has no constitutional right to be a policeman . . ."

We deem it, therefore, to be within the power of Government in the absence of Civil Service requirement or contractual relationships to terminate employment of a consultant at any time.

A major question which has repeatedly emerged in our deliberations is whether in determining the security status of an individual who is a scientist, the Government must take into account the reactions of, and the possible impact upon, all other scientists.

The Board takes cognizance of the serious alarm expressed to it by witnesses and frequently adverted to in the public press that denial of clearance to Dr. Oppenheimer would do serious harm in the scientific community. This is a matter of vital concern to the Government and the people.

We should express our considered view that, because the loyalty or security risk status of a scientist or any other intellectual may be brought into question, scientists and intellectuals are ill-advised to assert that a reasonable and sane inquiry constitutes an attack upon scientists and intellectuals generally. This Board would deplore deeply any notion that scientists are under attack in this country and that prudent study of any individual's conduct and character within the necessary demands of the national security could be either in fact or in appearance a reflection of anti-intellectualism.

The Board has taken note of the fact that in some cases of this sort groups of scientists have tended towards an almost professional opposition to any inquiry about a member of the group. They thus, by moving in a body to the defense of one of their number, give currency, credence, and support to a notion that they as a group are under attack. A decision of a Board of this sort, whether favorable or unfavorable to the individual whose case is before it, should be considered neither as an exoneration of all

scientists from imputations of security risk nor a determination that all scientists are suspect.

We know that scientists, with their unusual talents, are loyal citizens, and, for every pertinent purpose, normal human beings. We must believe that they, the young and the old and all between, will understand that a responsible Government must make responsible decisions. If scientists should believe that such a decision in Government, however distasteful with respect to an individual, must be applicable to his whole profession, they misapprehend their own duties and obligations as citizens.

In this connection, the Board has been impressed, and in many ways heartened by the manner in which many scientists have sprung to the defense of one whom many felt was under unfair attack. This is important and encouraging when one is concerned with the vitality of our society. However, the Board feels constrained to express its concern that in this solidarity there have been attitudes so uncompromising in support of science in general, and Dr. Oppenheimer in particular, that some witnesses have, in our judgment, allowed their convictions to supersede what might reasonably have been their recollections.

<u>One important consideration brought into focus by this case is the role of scientists as advisors in the formulation of Government policy</u>.

We must address ourselves to the natural constraints and the particular difficulties inherent in the AEC program itself. As a nation we find it necessary to delegate temporary authority with respect to the conduct of the program and the policies to be followed to duly elected representatives and appointive officials as provided for by our constitution and laws. For the most part, these representatives and officials are not capable of passing judgment on technical matters and, therefore, appropriately look to specialists for advice. We must take notice of the current and inevitable amplification of influence which attaches to those giving advice under these circumstances. These specialists have an exponential amplification of influence which is vastly greater than that of the individual citizen.

It must be understood that such specialists did not, as scientists, deliberately create this condition. For example, Dr. Oppenheimer served his Government because it sought him. The impact of his influence was felt immediately and increased progressively as his services were used. The nation owes these scientists, we believe, a great debt of gratitude for loyal and magnificent service. This is particularly true with respect to Dr. Oppenheimer.

A question can properly be raised about advice of specialists relating to moral, military and political issues, under circumstances which lend such advice an undue and in some cases decisive weight. Caution must be expressed with respect to judgments which go beyond areas of special and particular competence.

Any man, whether specialist or layman, of course, must have the right to express his deep moral convictions; must have the privilege of voicing his deepest doubts. We can understand the

emotional involvement of any scientist who contributed to the development of atomic energy and thus helped to unleash upon the world a force which could be destructive of civilization. Perhaps no American can be entirely guilt-free, and, yet, these weapons did not bring peace nor lessen the threats to the survival of our free institutions. Emotional involvement in the current crisis, like all other things, must yield to the security of the nation.

Dr. Oppenheimer himself testified, "I felt perhaps quite wrongly that having played an active part in promoting a revolution in warfare I needed to be as responsible as I could with regard to what came of this revolution".

We have no doubt that other distinguished and devoted scientists have found themselves beset by a similar conflict.

It is vitally important that Government and scientists alike understand the need for and value of the advice of competent technicians. This need is a present and a continuing one. Yet, those officials in Government who are responsible for the security of the country must be certain that the advice which they seriously seek appropriately reflects special competence on the one hand, and soundly based conviction on the other, uncolored and uninfluenced by considerations of an emotional character.

In evaluating advice from a specialist which departs from the area of his speciality, Government officials charged with the military posture of our country must also be certain that underlying any advice is a genuine conviction that this country cannot in the interest of security have less than the strongest possible offensive capabilities in a time of national danger.

Significance of the Findings of The Board

The facts referred to in General Nichols' letter fall clearly into two major areas of concern. The first of these, which is represented by Items 1 through 23, involves primarily Dr. Oppenheimer's Communist connections in the earlier years and continued associations arising out of those connections.

The second major area of concern is related to Dr. Oppenheimer's attitudes and activities with respect to the development of the hydrogen bomb.

The Board has found the allegations in the first part of the Commission letter to be substantially true, and attaches the following significance to the findings: There remains little doubt that, from late 1936 or early 1937 to probably April, 1942, Dr. Oppenheimer was deeply involved with many people who were active Communists. The record would suggest that the involvement was something more than an intellectual and sympathetic interest in the professed aims of the Communist Party. Although Communist functionaries during this period considered Dr. Oppenheimer to be a Communist, there is no evidence that he was a member of the Party in the strict sense of the word.

Using Dr. Oppenheimer's own characterization of his status during that period, he seems to have been an active fellow-traveler.

emotional involvement of any scientist who contributed to the development of atomic energy and thus helped to unleash upon the world a force which could be destructive of civilization. Perhaps no American can be entirely guilt-free, and, yet, these weapons did not bring peace nor lessen the threats to the survival of our free institutions. Emotional involvement in the current crisis, like all other things, must yield to the security of the nation.

Dr. Oppenheimer himself testified, "I felt perhaps quite wrongly that having played an active part in promoting a revolution in warfare I needed to be as responsible as I could with regard to what came of this revolution".

We have no doubt that other distinguished and devoted scientists have found themselves beset by a similar conflict.

It is vitally important that Government and scientists alike understand the need for and value of the advice of competent technicians. This need is a present and a continuing one. Yet, those officials in Government who are responsible for the security of the country must be certain that the advice which they seriously seek appropriately reflects special competence on the one hand, and soundly based conviction on the other, uncolored and uninfluenced by considerations of an emotional character.

In evaluating advice from a specialist which departs from the area of his speciality, Government officials charged with the military posture of our country must also be certain that underlying any advice is a genuine conviction that this country cannot in the interest of security have less than the strongest possible offensive capabilities in a time of national danger.

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Using Dr. Oppenheimer's own characterization of his status during that period, he seems to have been an active fellow-traveler.

According to him, his sympathies with the Communists seem to have begun to taper off somewhat after 1939, and very much more so after 1942. However, it is not unreasonable to conclude from material presented to this Board that Dr. Oppenheimer's activities ceased as of about the time he executed his Personnel Security Questionnaire in April, 1942. He seems to have had the view at that time and subsequently that current involvement with Communist activities was incompatible with service to the Government. However, it also would appear that he felt that former Communist Party membership was of little consequence if the individual concerned was personally trustworthy.

Dr. Oppenheimer's sympathetic interests seemed to have continued beyond 1942 in a diluted and diminishing state until 1946, at which time we find the first affirmative action on his part which would indicate complete rejection. In October, 1946, he tendered his resignation from the Independent Citizens Committee of the Arts, Sciences, and Professions, Inc., and he now says it was at this time that he finally realized that he could not collaborate with the Communists, whatever their aims and professed interests. We would prefer to have found an affirmative action at an earlier date.

The Board takes a most serious view of these earlier involvements. Had they occurred in very recent years, we would have found them to be controlling and, in any event, they must be taken into account in evaluating subsequent conduct and attitudes.

The facts before us establish a pattern of conduct falling within the following Personnel Security Clearance criteria: Category A, including instances in which there are grounds sufficient to establish a reasonable belief that an individual or his spouse has (1) Committed or attempted to commit or aided or abetted another who committed or attempted to commit any act of sabotage, espionage, treason, or sedition. (2) Establish an association with espionage agents of a foreign nation . . . (3) Held membership or joined any organization which had been declared by The Attorney Geneal to be . . . Communist, subversive . . . . These criteria under the AEC procedures establish a presumption of security risk.

The Board believes, however, that there is no indication of disloyalty on the part of Dr. Oppenheimer by reason of any present Communist affiliation, despite Dr. Oppenheimer's poor judgment in continuing some of his past associations into the present. Furthermore, the Board had before it eloquent and convincing testimony of Dr. Oppenheimer's deep devotion to his country in recent years and a multitude of evidence with respect to active service in all sorts of Governmental undertakings to which he was repeatedly called as a participant and as a consultant.

We feel that Dr. Oppenheimer is convinced that the earlier involvements were serious errors and today would consider them an indication of disloyalty. The conclusion of this Board is that Dr. Oppenheimer is a loyal citizen.

With respect to the second portion of General Nichols' letter, the Board believes that Dr. Oppenheimer's opposition to the

hydrogen bomb and his related conduct in the post-war period until April, 1951, involved no lack of loyalty to the United States or attachment to the Soviet Union. The Board was impressed by the fact that even those who were critical of Dr. Oppenheimer's judgment and activities or lack of activities, without exception, testified to their belief in his loyalty.

The Board concludes that any possible implications to the contrary which might have been read into the second part of General Nichols' letter are not supported by any material which the Board has seen.

The Board wishes to make clear that in attempting to arrive at its findings and their significance with respect to the hydrogen bomb, it has in no way sought to appraise the technical judgments of those who were concerned with the program.*

We cannot dismiss the matter of Dr. Oppenheimer's relationship to the development of the hydrogen bomb simply with the finding that his conduct was not motivated by disloyalty, because it is our conclusion that, whatever the motivation, the security interests of the United States were affected.

We believe that, had Dr. Oppenheimer given his enthusiastic support to the program, a concerted effort would have been initiated at an earlier date.

Following the President's decision, he did not show the enthusiastic support for the program which might have been expected of the chief atomic adviser to the Government under the circumstances. Indeed, a failure to communicate an abandonment of his earlier position undoubtedly had an effect upon other scientists. It is our feeling that Dr. Oppenheimer's influence in the atomic scientific circles with respect to the hydrogen bomb was far greater than he would have led this Board to believe in his testimony before the Board. The Board has reluctantly concluded that Dr. Oppenheimer's candor left much to be desired in his discussions with the Board of his attitude and position in the entire chronology of the hydrogen bomb problem.

We must make it clear that we do not question Dr. Oppenheimer's right to the opinions he held with respect to the development of this weapon. They were shared by other competent and devoted individuals, both in and out of Government. We are willing to assume that they were motivated by deep moral conviction. We are concerned, however, that he may have departed his role as scientific adviser to exercise highly persuasive influence in matters in which his convictions were not necessarily a reflection of technical judgment, and also not necessarily related to the protection of the strongest offensive military interests of the country.

In the course of the proceedings, there developed other facts which raised questions of such serious import as to give us concern about whether the retention of Dr. Oppenheimer's services would be clearly consistent with the security interests of the United States.

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*This is the end of page 32 referred to in the minority report of Dr. Ward V. Evans.

It must be said that Dr. Oppenheimer seems to have had a high degree of discretion reflecting an unusual ability to keep to himself vital secrets. However, we do find suggestions of a tendency to be coerced, or at least influenced in conduct over a period of years.

By his own testimony, Dr. Oppenheimer was led to protest the induction into military service of Giovanni Rossi Lomanitz in 1943 by the outraged intercession of Dr. Condon. It is to be remembered that, at this time, Dr. Oppenheimer knew of Lomanitz's connections and of his indiscretions. In 1949, Dr. Oppenheimer appeared in executive session before the House Un-American Activities Committee, and at that time was asked about his friend, Dr. Bernard Peters. Dr. Oppenheimer confirmed the substance of an interview with the security officer which took place during the war years and in which he had characterized Dr. Peters as a dangerous Red and former Communist. This testimony soon appeared in the Rochester, New York, newspapers. At this time, Dr. Peters was on the staff of the University of Rochester. Dr. Oppenheimer, as a result of protestations by Dr. Condon, by Dr. Peters himself, and by other scientists, then wrote a letter for publication to the Rochester newspaper, which, in effect, repudiated his testimony given in secret session. His testimony before this Board indicated that he failed to appreciate the great impropriety of making statements of one character in a secret session and of a different character for publication, and that he believed that the important thing was to protect Dr. Peters' professional status. In that episode, Dr. Condon's letter, which has appeared in the press, contained a severe attack on Dr. Oppenheimer. Nevertheless, he now testifies that he is prepared to support Dr. Condon in the loyalty investigation of the latter.

Executive Order 10450 in listing criteria to be taken into account in cases of this sort indicates in part the following:

> "Section 8(a)(1)(i) any behavior, activities, or associations which tend to show that the individual is not reliable or trustworthy.
>
> (v) Any facts which furnish reason to believe that the individual may be subjected to coercion, influence, or pressure which may cause him to act contrary to the best interest of the national security."

Whether the incidents referred to clearly indicate a susceptibility to influence or coercion within the meaning of the criteria or whether they simply reflect very bad judgment, they clearly raise the question of Dr. Oppenheimer's understanding, acceptance, and enthusiastic support of the security system. Beginning with the Chevalier invident, he has repeatedly exercised an arrogance of his own judgment with respect to the loyalty and reliability of other citizens to an extent which has frustrated and at times impeded the workings of the system. In an interview with agents of the F.B.I. in 1946, which in good part concerned itself with questions about Chevalier, when asked about a meeting which Dr. Oppenheimer had attended, at which Communists and Communist sympathizers were in attendance, he declined to discuss it on the ground that it was irrelevant, although the meeting itself

was held in Chevalier's home. In a subsequent interview, he declined to discuss people he had known to be Communists.

Indeed, in the course of this proceeding, Dr. Oppenheimer recalled pertinent details with respect to Communist meetings and with respect to individuals with Communist connections, which he had never previously disclosed in the many interviews with Government authorities, in spite of the fact that he had been interviewed regarding such matters.

In 1946 or 1947, he assisted David Bohm in getting a position at Princeton and, at least on a casual basis, continued his associations with Bohm after he had reason to know of Bohm's security status. He testified that today he would give Bohm a letter of recommendation as a physicist, and, although not asked whether he would also raise questions about Bohm's security status, he in no way indicated that this was a matter of serious import to him.

While his meeting with Lomanitz and Bohm immediately prior to their appearance before the House Un-American Activities Committee in 1949, at which time both plead the Fifth Amendment, may have been a casual one as he testified, he nevertheless discussed with them their testimony before that Committee.

Moreover, his current associations with Dr. Chevalier, as discussed in detail in item No. 23, are, we believe, of a high degree of significance. It is not important to determine that Dr. Oppenheimer discussed with Chevalier matters of concern to the security of the United States. What is important is that Chevalier's Communist background and activities were known to Dr. Oppenheimer. While he says he believes Chevalier is not now a Communist, his association with him, on what could not be considered a casual basis, is not the kind of thing that our security system permits on the part of one who customarily has access to information of the highest classification.

Loyalty to one's friends is one of the noblest of qualities. Being loyal to one's friends above reasonable obligations to the country and to the security system, however, is not clearly consistent with the interests of security.

We are aware that in these instances Dr. Oppenheimer may have been sincere in his interpretation that the security interests of the country were not disserved; we must, however, take a most serious view of this kind of continuing judgment.

We are constrained to make a final comment about General Nichols' letter. Unfortunately, in the press accounts in which the letter was printed in full, Item number 24, which consisted of one paragraph, was broken down into four paragraphs. Many thoughtful people, as a result, felt that the implication of one or more of these paragraphs as they appeared in the press standing alone was that the letter sought to initiate proceedings which would impugn a man on the ground of his holding and forcefully expressing strong opinions. It is regrettable that the language of the letter or the way in which it publicly appeared, might have

given any credence to such an interpretation. In any event, the Board wishes strongly to record its profound and positive view that no man should be tried for the expression of his opinions.

Recommendation

In arriving at our recommendation we have sought to address ourselves to the whole question before us and not to consider the problem as a fragmented one either in terms of specific criteria or in terms of any period in Dr. Oppenheimer's life, or to consider loyalty, character and associations separately.

However, of course, the most serious finding which this Board could make as a result of these proceedings would be that of disloyalty on the part of Dr. Oppenheimer to his country. For that reason, we have given particular attention to the question of his loyalty, and we have come to a clear conclusion, which should be reassuring to the people of this country, that he is a loyal citizen. If this were the only consideration, therefore, we would recommend that the reinstatement of his clearance would not be a danger to the common defense and security.

We have, however, been unable to arrive at the conclusion that it would be clearly consistent with the security interests of the United States to reinstate Dr. Oppenheimer's clearance and, therefore, do not so recommend.

The following considerations have been controlling in leading us to our conclusion:

1. We find that Dr. Oppenheimer's continuing conduct and associations have reflected a serious disregard for the requirements of the security system.

2. We have found a susceptibility to influence which could have serious implications for the security interests of the country.

3. We find his conduct in the hydrogen bomb program sufficiently distrubing as to raise a doubt as to whether his future participation, if characterized by the same attitudes in a Government program relating to the national defense, would be clearly consistent with the best interests of security.

4. We have regretfully concluded that Dr. Oppenheimer has been less than candid in several instances in his testimony before this Board.

Respectfully submitted.

/s/ Gordon Gray
Gordon Gray, Chairman

/s/ Thomas A. Morgan
Thomas A. Morgan

MINORITY REPORT OF DR. WARD V. EVANS

I have reached the conclusion that Dr. J. Robert Oppenheimer's clearance should be reinstated and am submitting a minority report in accordance with AEC procedure.

The Board, appointed by the Commission, has worked long and arduously on the Oppenheimer case. We have heard 40 witnesses and have taken some 3,000 pages of testimony in addition to having read a similar number of pages of file material. We have examined carefully the notification letter to Dr. Oppenheimer from Mr. Nichols of December 23, 1953, and all other relevant material.

I am in perfect agreement with the majority report of its "findings" with respect to the allegations in Mr. Nichols' letter and I am in agreement with the statement of the Board concerning the significance of its "findings" to the end of page 32.* I also agree with the last paragraph of this section in which the Board makes a final comment on Mr. Nichols' letter. I do not, however, think it necessary to go into any philosophical discussion to prove points not found in Mr. Nichols' letter.

The derogatory information in this letter consisting of twenty-four items has all been substantiated except for one item. This refers to a Communist meeting held in Dr. Oppenheimer's home, which he is supposed to have attended.

On the basis of this finding, the Board would have to say that Dr. Oppenheimer should not be cleared.

But this is not all.

Most of this derogatory information was in the hands of the Commission when Dr. Oppenheimer was cleared in 1947. They apparently were aware of his associations and his left-wing policies; yet they cleared him. They took a chance on him because of his special talents and he continued to do a good job. Now when the job is done, we are asked to investigate him for practically the same derogatory information. He did his job in a thorough and painstaking manner. There is not the slightest vestige of information before this Board that would indicate that Dr. Oppenheimer is not a loyal citizen of his country. He hates Russia. He had Communistic friends, it is true. He still has some. However, the evidence indicates that he has fewer of them than he had in 1947. He is not as naive as he was then. He has more judgment; no one on the Board doubts his loyalty - even the witnesses adverse to him admit that - and he is certainly less of a security risk than he was in 1947, when he was cleared. To deny him clearance now for what he was cleared for in 1947, when we must know he is less of a security risk now than he was then, seems to be hardly the procedure to be adopted in a free country.

We don't have to go out of our way and invent something to prove that the principle of "double jeopardy" does not apply here. This is not our function, and it is not our function to rewrite any clearance rules. The fact remains he is being investigated twice for the same things. Furthermore, we don't have to dig

* The reference is to page 32 of the typewritten document. In this reproduction the material referred to is to the end of the second full paragraph at the top of page 30.

deeply to find other ways that he may be a security risk outside of loyalty, character and association. He is loyal, we agree on that. There is, in my estimation, nothing wrong with his character. During the early years of his life, Dr. Oppenheimer devoted himself to study and did not vote or become interested in political matters until he was almost thirty. Then, in his ignorance, he embraced many subversive organizations.

His judgment was bad in some cases, and most excellent in others but, in my estimation, it is better now than it was in 1947 and to damn him now and ruin his career and his service, I cannot do it.

His statements in cross examination show him to be still naive, but extremely honest and such statements work to his benefit in my estimation. All people are somewhat of a security risk. I don't think we have to go out of our way to point out how this man might be a security risk.

Dr. Oppenheimer in one place in his testimony said that he had told "a tissue of lies." What he had said was not a tissue of lies; there was one lie. He said on one occasion that he had not heard from Dr. Seaborg, when in fact he had a letter from Dr. Seaborg. In my opinion he had forgotten about the letter or he would never have made this statement for he would have known that the Government had the letter. I do not consider that he lied in this case. He stated that he would have recommended David Bohm as a physicist to Brazil, if asked. I think I would have recommended Bohm as a physicist. Dr. Oppenheimer was not asked if he would have added that Bohm was a Communist. In recent years he went to see Chevalier in Paris. I don't like this, but I cannot condemn him on this ground. I don't like his about face in the matter of Dr. Peters, but I don't think it subversive or disloyal.

He did not hinder the development of the H-bomb and there is absolutely nothing in the testimony to show that he did.

First he was in favor of it in 1944. There is no indication that this opinion changed until 1945. After 1945 he did not favor it for some years perhaps on moral, political or technical grounds. Only time will prove whether he was wrong on the moral and political grounds. After the Presidential Directive of January 31, 1950, he worked on this project. If his opposition to the H-bomb caused any people not to work on it, it was because of his intellectual prominence and influence over scientific people and not because of any subversive tendencies.

I personally think that our failure to clear Dr.Oppenheimer will be a black mark on the escutcheon of our country. His witnesses are a considerable segment of the scientific backbone of our nation and they endorse him. I am worried about the effect an improper decision may have on the scientific development in our country. Nuclear physics is new in our country. Most of our authorities in this field came from overseas. They are with us now. Dr. Oppenheimer got most of his education abroad. We have taken hold of this new development in a very great way. There is no predicting where and how far it may go and what its future

potentialities may be. I would very much regret any action to retard or hinder this new scientific development.

I would like to add that this opinion was written before the Bulletin of the Atomic Scientists came out with its statement concerning the Oppenheimer case.

This is my opinion as a citizen of a free country.

I suggest that Dr. Oppenheimer's clearance be restored.

/s/ W. V. Evans
Ward V. Evans

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June 1, 1954

General K.D. Nichols
General Manager
U.S. Atomic Energy Commission
Washington 25, D.C.

Dear General Nichols:

Dr. Oppenheimer has received your letter of May 28, 1954, in which you enclosed a copy of the "Findings and Recommendation of the Personnel Security Board" dated May 27. In this document the Board unanimously found that Dr. Oppenheimer was a loyal citizen, but by a 2 to 1 vote, Dr. Ward V. Evans dissenting, recommended that Dr. Oppenheimer's clearance should not be reinstated. Dr. Oppenheimer has asked me to send you this reply on his behalf.

You informed Dr. Oppenheimer that he might have until June 7 to notify the Commission whether he would request a review of the case by the Commission's Personnel Security Review Board. You also informed him that, after considering the record (including the recommendation of the Review Board if a review were taken), you would make your recommendation to the Commission and the Commission would finally determine the matter.

Since the Commission is in any event to decide the case, it seems to us that no useful purpose would be served by our requesting the Review Board to go over the matter afresh and to make

a recommendation that would have no more finality to it than that which the Personnel Security Board has already made.

Such a review would entail further delay, which Dr. Oppenheimer is naturally anxious to avoid; moreover, Dr. Oppenheimer's annual contract as a Consultant to the Atomic Energy Commission expires on June 30, the end of the fiscal year, and with it his clearance (now suspended) would automatically expire, so that if this case is not finally determined by June 30, the possibility exists that the question of reinstating Dr. Oppenheimer's clearance might be regarded as moot and might be left in a state of confusion and uncertainty. We do not believe that such an outcome would be in the public interest. Accordingly, Dr. Oppenheimer waives his privilege of review by the Personnel Security Review Board and requests immediate consideration of the case by the Atomic Energy Commission.

In order to assist the Commission in its deliberations, and because of the great public importance of some of the issues raised by the majority and minority opinions of the Personnel Security Board, we request the Commission's permission to file a brief and to make oral argument. A brief is already in the course of preparation and can be delivered to the Commission by June 7 (the latest date specified in your letter to request review). Argument of counsel can, we are sure, be arranged to meet the Commission's convenience.

Meanwhile we think it fitting to identify for the Commission what we conceive to be certain issues of basic importance which are presented by the majority and minority opinions. We believe it essential, however, that the Commission have the

benefit of critical analysis and illumination of these issues, which can only be supplied adequately by the brief and oral argument herein requested.

To begin with, the majority's conclusion not to recommend the reinstatement of Dr. Oppenheimer's clearance stands in such stark contrast with the Board's findings regarding Dr. Oppenheimer's loyalty and discretion as to raise doubts about the process of reasoning by which the conclusion was arrived at. All members of the Board agreed:

(1) That the nation owed scientists "a great debt of gratitude for loyal and magnificent service" and that "This is particularly true with respect to Dr. Oppenheimer." (p.27)

(2) That "we have before us much responsible and positive evidence of the loyalty and love of country of the individual concerned" (p. 21), and "eloquent and convincing testimony of Dr. Oppenheimer's deep devotion to his country in recent years and a multitude of evidence with respect to active service in all sorts of Governmental undertakings to which he was repeatedly called as a participant and as a consultant." (p.29)

(3) That "even those who were critical of Dr. Oppenheimer's judgment and activities or lack of activities, without exception, testified to their belief in his loyalty." (p.30)

(4) That "we have given particular attention to the question of his loyalty, and we have come to a clear conclusion, which should be reassuring to the people of this country, that he is a loyal citizen. If this were the only consideration, therefore, we would recommend that the reinstatement of his clearance

would not be a danger to the common defense and security." (p.33)

(5) That "It must be said that Dr. Oppenheimer seems to have had a high degree of discretion reflecting an unusual ability to keep to himself vital secrets." (p.31)

In spite of these findings of loyalty and of discretion in the handling of classified data, the majority of the Board reached the conclusion that Dr. Oppenheimer's clearance should not be reinstated. How can this be? The majority advanced four considerations as controlling in leading them to their conclusion (p.33).

The first two -- an alleged "serious disregard for the requirements of the security system", and an alleged "susceptibility to influence" -- rest upon an appraisal of the evidence which we do not think is justified by the record. Taking sharp issue, as we do, with the majority's treatment of the incidents cited in support of these two considerations, we cannot undertake here to review the detailed evidence, but propose to do so in the brief.

We would like, however, to draw attention to two of the incidents referred to by the majority in support of these considerations, merely to indicate the care with which we think the record needs to be reviewed by the Commission. The majority held it against Dr. Oppenheimer, apparently as an example of his supposed susceptibility to influence, that despite a severe attack on him by Dr. Edward Condon in 1949, in a letter which appeared in the press, Dr. Oppenheimer is now prepared to support Dr. Condon in

the latter's pending loyalty investigation (p.31). It seems to us strange that a man should be criticized for refusing to let his personal feelings stand in the way of his giving evidence on behalf of a man he believes to be loyal. The majority further criticized Dr. Oppenheimer for his continuing associations and supposed disregard of security requirements in that "In 1946 or 1947 he assisted David Bohm [a former student] in getting a position at Princeton and, at least on a casual basis, continued his associations with Bohm after he had reason to know of Bohm's security status. He testified that today he would give Bohm a letter of recommendation as a physicist, and, although not asked whether he would also raise questions about Bohm's security status, he in no way indicated that this was a matter of serious import to him." (p.32) Dr. Evans' comment on this incident was: "I think I would have recommended Bohm as a physicist. Dr. Oppenheimer was not asked if he would have added that Bohm was a Communist." (p.35)

We propose to analyze in detail, in brief and argument, these and other incidents referred to by the majority as bearing on Dr. Oppenheimer's supposed "disregard" for the "security system" and "susceptibility to influence".

The third and fourth considerations advanced by the majority for concluding that Dr. Oppenheimer was a "security risk" warrant more extended comment here.

The third item -- Dr. Oppenheimer's "conduct in the hydrogen bomb program", characterized as "disturbing," -- and the fourth -- alleged "lack of candor" in several instances in his

testimony -- require discussion, because they involve questions of policy and procedure which we wish particularly to draw to the Commission's attention in a preliminary way.

In the case of the third consideration -- Dr. Oppenheimer's so-called disturbing conduct in the hydrogen bomb program -- the Board's unanimous findings of fact again stand in stark contrast with the conclusion of the majority. Thus the Board unanimously found:

(1) That Dr. Oppenheimer's opposition to the H-bomb program "involved no lack of loyalty to the United States or attachment to the Soviet Union." (p.30)

(2) That his opinions regarding the development of the H-bomb "were shared by other competent and devoted individuals, both in and out of Government." (p. 30)

(3) That it could be assumed that these opinions "were motivated by deep moral conviction." (p.30)

(4) That after the national policy to proceed with the development of the H-bomb had been determined in January 1950, he "did not oppose the project in a positive or open manner, nor did he decline to cooperate in the project." (p.20)

(5) That the allegation that he urged other scientists not to work on the hydrogen bomb program was unfounded. (p.20)

(6) That he did not, as alleged, distribute copies of the General Advisory Report to key personnel with a view to

turning them against the project, but that on the contrary this distribution was made at the Commission's own direction. (p.20)

In short, all the basic allegations set forth in General Nichols' letter to Dr. Oppenheimer of December 23, 1953, regarding any improper action by him in the H-bomb problem were disproved.

In the face of these unanimous findings, the majority then somehow conclude that "the security interests of the United States were affected" by Dr. Oppenheimer's attitude toward the hydrogen bomb program. (p.30). Why? Because, according to the majority of the Board:

> "We believe that, had Dr. Oppenheimer given his enthusiastic support to the program, a concerted effort would have been initiated at an earlier date.
>
> "Following the President's decision, he did not show the enthusiastic support of the program which might have been expected of the chief atomic adviser to the Government under the circumstances. Indeed, a failure to communicate an abandonment of his earlier position undoubtedly had an effect upon other scientists." (p.30)

Without taking into account the factual evidence, which in our opinion should have led the Board to an opposite conclusions, we submit that the injection into a security case of a scientist's alleged lack of enthusiasm for a particular program is fraught with grave consequences to this country. How can a scientist risk advising the government if he is told that at some later day a security board may weigh in the balance the degree of his enthusiasm for some official program? Or that he may be held

accountable for a failure to communicate to the scientific community his full acceptance of such a program?

In addition to Dr. Oppenheimer's alleged lack of "enthusiasm", there are indications that the majority of the Board may also have been influenced in recommending against the reinstatement of Dr. Oppenheimer's clearance by judgments they had formed as to the nature and quality of the advice he gave to the AEC. While the majority of the Board stated -- with sincerity, we are sure -- that "no man should be tried for the expression of his opinions" (p.33), it seems to us that portions of the majority opinion do just that.

For example, the opinion says that while the Board can understand "the emotional involvement of any scientist who contributed to the development of atomic energy and thus helped to unleash upon the world a force which could be destructive of civilization", nevertheless "emotional involvement" of this sort in the current crisis "must yield to the security of the nation"; and government officials "who are responsible for the security of the country must be certain that the advice which they seriously seek appropriately reflects special competence on the one hand, and soundly based conviction on the other, uncolored and uninfluenced by considerations of an emotional character." (p.28) Does this mean that a loyal scientist called to advise his government does so at his peril unless, contrary to all experience, he can guarantee that his views are unaffected by his heart and his spirit?

The opinion further stated that defense officials "must also be certain that underlying any advice is a genuine conviction

that this country cannot in the interest of security have less than the strongest possible offensive capabilities in a time of national danger." (p.28) Does this mean that a loyal scientist called to advise his government does so at his peril if he happens to believe in the wisdom of maintaining a proper balance between offensive and defensive weapons?

It would appear from the following passage about Dr. Oppenheimer's advice that the majority of the Board assumed affirmative answers to both of the foregoing questions:

> "We are concerned, however, that he may have departed his role as scientific adviser to exercise highly persuasive influence in matters in which his convictions were not necessarily a reflection of technical judgment, and also not necessarily related to the protection of the strongest offensive military interests of the country." (p.30)

This poses a serious issue. If a scientist whose loyalty is unquestioned may nevertheless be considered a security risk because in the judgment of a board he may have given advice which did not necessarily reflect a bare technical judgment, or which did not accord with strategical considerations of a particular kind, then he is being condemned for his opinions. Surely our security requires that expert views, so long as they are honest, be weighed and debated and not that they be barred.

We quite agree with the Board's view that, "because the loyalty or security risk status of a scientist or any other intellectual may be brought into question, scientists and intellectuals are ill-advised to assert that a reasonable and sane inquiry consti-

tutes an attack upon scientists and intellectuals generally." (p.26) This statement, however, begs the fundamental question as to what are the appropriate limits of a security inquiry under existing statutes and regulations, and under a government of laws and not of men -- a question of concern not merely to scientists and intellectuals but to all our people.

* * * * *

As to the majority's comments about Dr. Oppenheimer's alleged lack of "candor" in "several instances in his testimony", we shall ask the Commission to take special note of the observations in Dr. Evans' minority opinion that while Dr. Oppenheimer's "statements in cross-examination show him to be still naive", they also show him to be "extremely honest and such statements work to his benefit in my estimation" and that while "his judgment was bad in some cases" it was "most excellent in others but it is better now than it was in 1947", when the Atomic Energy Commission unanimously cleared him. We shall also direct the Commission's attention to the fact that the text of the Board's report contains only three specific references to alleged lack of candor, all having to do with the hydrogen bomb program (pp.20 and 30). [We should point out that as to two of these references (p.30) Dr. Evans specifically dissociated himself from the majority (p. 34) and as to the other (p. 20) he did so by clear implication (p.35).] As to all these matters there was extensive testimony not only by Dr. Oppenheimer but also by others who served with him on the General Advisory Committee, including Dr. James B. Conant, Dr. I. I. Rabi (now Chairman of the GAC), Dr. Enrico Fermi, and Mr. Hartley Rowe of

the United Fruit Company; and also by Dr. Norris Bradbury (Dr. Oppenheimer's successor as director of Los Alamos); by Mr. Gordon Dean (former Chairman of the Atomic Energy Commission); by Dr. Hans Bethe; by Dr. Robert Bacher (a former member of the AEC); and by a number of other distinguished men.

In brief and argument we expect to analyze for the Commission's assistance the evidence they gave, which in our judgment bears out the truth and sincerity of Dr. Oppenheimer's account of the H-bomb controversy.

* * * * *

We wish to make two more observations of a general character.

First, we trust that the Commission in weighing the evidence, including the instances of alleged lack of candor, will take into account certain procedural difficulties which beset the presentation of Dr. Oppenheimer's case. Weeks before the hearing commenced we asked you and the Commission's general counsel for much information which we thought relevant to our case but which was denied us -- documents and minutes concerning Dr. Oppenheimer's 1947 clearance and a variety of other material. Much of this information did come out in the hearings but usually only in the course of cross-examination when calculated to cause the maximum surprise and confusion and too late to assist us in the orderly presentation of our case. Some of the information which was denied to us before the hearing was de-classified at the moment of cross-examination or shortly before and was made available to us only during cross-examination or after.

It is true that Dr. Oppenheimer was accorded the privilege of re-examining, prior to the hearings, reports and other material in the preparation of which he had participated. But he was not given access to the broad range of material actually used and disclosed for the first time at the hearings by the Commission's special counsel who had been retained for the case. And of course Dr. Oppenheimer was not given access to the various documents which, according to the Board's report "under Governmental necessity cannot be disclosed, such as reports of the Federal Bureau of Investigation." (p.31)

The voluminous nature of this undisclosed material appears from the Board's report. It notes that in our hearings the Board heard 40 witnesses and compiled over 3,000 pages of testimony; and we then learn from the report that "in addition" the Board has "read the same amount of file material." (p.2) We can only speculate as to the contents of this "file material." We cannot avoid the further speculation as to how much of this material might have been disclosed to Dr. Oppenheimer in the interests of justice without any real injury to the security interests of the government if established rules of exclusion, which the Board felt bound to apply and we to accept, had not stood in the way.

Having in mind the difficulties and handicaps which have been recounted above, we urge upon the Commission as strongly as possible the following:

(1) That in weighing the testimony, and particularly those portions where documents were produced on cross-examination in

the manner described above, the Commission should constantly bear in mind how, under such circumstances, the natural fallibility of memory may easily be mistaken for disingenuousness;

(2) That in the consideration of documentary material not disclosed to Dr. Oppenheimer, the Commission should be ever conscious of the unreliability of ex parte reports which have never been seen by Dr. Oppenheimer or his counsel or tested by cross-examination; and

(3) That if in the course of the Commission's deliberations the Commission should conclude that any hitherto undisclosed documents upon which it intends to rely may be disclosed to us without injury to what may be thought to be over-riding interests of the national government, they should be so disclosed before any final decision is made.

* * * * *

Our final observation has to do with the general structure of the Board's report, and with what has been omitted from it which we feel the Commission should put in the forefront of its consideration if it is to view this case in anything like the true perspective of history -- a history through which Dr. Oppenheimer has lived and which in part he has helped to create.

The Board's opinion, as required by the AEC Procedures, makes specific findings on each allegation of "derogatory information" contained in your letter of December 23, 1953. These findings, which are placed at the beginning of the report, are not thereafter,

except in Dr. Evans' dissenting opinion, considered in the context of Dr. Oppenheimer's life as a whole. Dr. Oppenheimer's letter to you of March 4, 1954, in answer to yours of December 23, 1953, stated at the outset that "the items of so-called 'derogatory information' set forth in your letter cannot fairly be understood except in the context of my life and work."

In his letter Dr. Oppenheimer tried to describe the derogatory information about him in that context. There is, in fact, little in the Board's findings that did not appear from what Dr. Oppenheimer volunteered about himself in his original letter to you. Over and above that, he gave a picture of his life and times without which the items of derogatory information cannot fairly be understood -- a picture to which many witnesses added who had known him intimately and had worked side by side with him in the positions of high responsibility which the government, first in war and then in peace and then in the cold war, successively devolved upon him. This picture, which is glimpsed in Dr. Evans' vivid opinion, does not appear at all in the main body of the report, nor is any mention made of the many witnesses who testified at his behest. Some of these we have already named above. The others were Mervin J. Kelly, president of the Bell Telephone Laboratories; General Leslie R. Groves; T. Keith Glennan, president of Case Institute of Technology; Karl T. Compton, retired president of Massachusetts Institute of Technology; Col. John Lansdale, Jr., wartime senior security officer for the atomic bomb project; James B. Fisk, vice president of the Bell Telephone Laboratories; Prof. Jerrold B. Zacharias, of M.I.T.; Oliver E. Buckley, of the GAC,

retired chairman of the Bell Telephone Laboratories; General Frederick H. Osborn; Ambassador George F. Kennan; Prof. Walter G. Whitman, of M.I.T.; Harry A. Winne, former vice president of General Electric, chairman of the Defense Department's Panel on Atomic Energy; Dr. Vannevar Bush, president of the Carnegie Institution; Sumner T. Pike, former Atomic Energy Commissioner; David E. Lilienthal, former chairman of the A.E.C.; Lee A. DuBridge, president of California Institute of Technology; James R. Killian, Jr., president of M.I.T.; Prof. Norman F. Ramsey, Jr., of Harvard; Maj. Gen. James McCormack, Jr., vice-commander of the Air Research and Development Command; John J. McCloy, chairman of the Chase National Bank; Prof. John von Neumann of the Institute for Advanced Study; Prof. John H. Manley of the University of Washington, former secretary of the G.A.C.; and Prof. Charles C. Lauritsen of the California Institute of Technology. The witnesses included ten former and present members of the General Advisory Committee, and five former Atomic Energy Commissioners.

Since all these witnesses testified to Dr. Oppenheimer's loyalty and since the Board unanimously found him to be loyal, the omission of their names from the report was understandable; but we mention them here and direct the Commission's attention particularly to their testimony, to which we hope to refer in brief and argument, because they did much more than vouch for Dr. Oppenheimer's loyalty. These, and the other men previously mentioned, were not ordinary character witnesses who tell about a man's reputation in the community. Every one of them had served with Dr. Oppenheimer, either at Los Alamos or on the many governmental boards and committees to

which he was later appointed. They saw him on the job and off the job, and in their varied testimony about their contacts with him over many years they helped to fill in the picture of the "man himself" which the Atomic Energy Commission, in its 1948 opinion in Dr. Frank Graham's case, said should be considered in determining whether an individual is a good or bad security risk.

Because we believe that the "man himself" can only be understood, and therefore fairly judged, by the closest attention to the testimony of those who have known him and worked intimately with him, as well as to his own testimony, we are particularly hopeful that the Commission will permit us to file a brief and to be heard.

In closing this letter we wish to record our appreciation of the patience and consideration accorded to Dr. Oppenheimer and his counsel by Mr. Gray, Mr. Morgan and Dr. Evans throughout the nearly four weeks of hearings, and our recognition of the sacrifices which they made in the public interest in assuming the long and arduous task assigned to them.

Mr. John W. Davis has authorized me to say that he joins in this letter and will join in the brief.

Very truly yours,

Lloyd K. Garrison

cc: Admiral Lewis L. Strauss, Chairman
Mr. Joseph Campbell
Mr. Thomas E. Murray
Dr. Henry D. Smyth
Mr. Eugene M. Zuckert

The New York Times
Thursday, June 17, 1954

TELLER HAS WON WIDE RECOGNITION

Physicist Was One of Einstein Colleagues Who Urged Start on Atom Bomb

Special to The New York Times.

SAN FRANCISCO, June 16—Dr. Edward Teller, Professor of Physics at the University of California, has won wide recognition in astrophysics, geophysics and physical chemistry as well as in nuclear physics.

He was appointed to his present position in July, 1953, while on leave from a similar position at the University of Chicago to conduct work in the Radiation Laboratory of the University of California in Berkeley and in Livermore, Calif.

He previously had been a member of a team of theoretical physicists that had worked on the Manhattan atomic bomb project. He was one of the six colleagues of Dr. Albert Einstein who collaborated in getting President Roosevelt to initiate the project in 1939.

Dr. Teller was born in Budapest, Hungary. He is 46 years old.

He attended the Technical Institute at Karlsruhe, Germany, and obtained a degree of Doctor of Philosophy at Leipzig, Germany, in 1930. For two years thereafter he was at the University of Goettingen and in 1933 went to England.

He thereafter worked for some time with Dr. Nils H. Bohr in Copenhagen. He came to the United States in 1935 to take a position as Professor of Physics at George Washington University in Washington, D. C. He remained there until 1941, when he transferred to Columbia University.

Named Professor in 1946

In 1942 Dr. Teller went to the University of Chicago to work in the metallurgical laboratory. He was named Professor of Physics in 1946.

By September, 1949, when the Russians accomplished an atomic explosion, some American scientists, including Dr. Teller, believed the more powerful hydrogen bomb could be made.

When Rear Admiral Lewis L. Strauss, now chairman of the Atomic Energy Commission, urged prompt beginning of work on the hydrogen bomb against the opposition of Dr. J. Robert Oppenheimer and others, Dr. Teller supported Admiral Strauss. When President Truman issued his go-ahead order in January, 1950, Dr. Teller became director of the program, and thus became known as "father of the H-bomb."

He was credited with finding short cuts through technical obstacles cited by other scientists and consequently with speeding the work. Preliminary tests were held at Eniwetok in early 1951.

Airplane Weapon Sought

When a new $11,500,000 hydrogen-bomb laboratory was established in the fall of 1952 at Livermore, Dr. Teller was placed in charge. He was at work there when the full-scale test of his complete thermonuclear device was held in the Marshall Islands. This was also a success, but the device was large and cumbersome, whereas something that an airplane could carry was sought.

Dr. Teller's quest for the hydrogen-bomb as an airplane weapon was spurred by the news of August, 1953, concerning a Russian thermonuclear explosion, apparently from a device far less complicated, more economical and more portable than anything then available to this country.

In March, 1954, Dr. Teller's work was crowned with still greater success when two more explosions in the Pacific indicated that the United States had forged far ahead in the hydrogen bomb competition.

DR. OPPENHEIMER DOUBTED 'THEORY'

H-Bomb That He Discounted Disclosed as One That Never Has Been Made

By ROBERT K. PLUMB

Testimony released yesterday disclosed that the "weapon" about which Dr. J. Robert Oppenheimer had doubts in 1949 was a hydrogen bomb in theory only.

Dr. Oppenheimer's successor as chairman of the General Advisory Committee of the Atomic Energy Commission, Dr. I. I. Rabi of Columbia University, testified that the "thing" discussed in 1949 "never has been made and probably never will be made, and we don't know to this day whether something like that will function."

The committee members decided to oppose a "crash" program to develop this 1949 weapon because they believed it unworkable, according to the testimony. A "very brilliant discovery" made in 1951 by Dr. Edward Teller, then at Los Alamos, N. M., made possible the actual hydrogen weapons tested in 1952 and again last spring, according to testimony by Dr. Hans Bethe of Cornell University.

The 1949 theoretical hydrogen weapon was to use hydrogen of atomic weight three, or tritium, a material that does not occur in nature in usable quantities.

To produce tritium for the hydrogen weapon, which itself was held probably unworkable, would require the utilization of vital nuclear reactors—atomic furnaces in which the fission-bomb element plutonium is also made.

Production of one pound of tritium requires the sacrifice of as much as eighty pounds of plutonium—a quantity probably sufficent for several large fission bombs of a type much improved over those used in Japan.

So some scientists feared that the nation would have to pay a price too high in terms of fission bombs for the opportunity to find out whether the theoretical 1949 bomb would work. Others believed that a hydrogen bomb, at best merely a larger bomb, would not confer significant advantage to the United States. Others believed the 1949 theoretical bomb would in any case prove to be too big to deliver to an enemy.

The New Approach

After President Truman had ordered the commission to proceed with the development of the hydrogen bomb on Jan. 31, 1951, Dr. Teller and a group of other scientists at Los Alamos had a "stroke of genius."

Apparently, this discovery proved that the hydrogen bomb would turn out to be much cheaper than the fission weapon. Early fears about its unworkability and its cost in priceless plutonium were quieted. The testimony shows that Dr. Oppenheimer, Dr. Bethe, Dr. Rabi and others realized at once the possibilities opened by the discovery of the Teller group.

The "discovery" has never been publicly reported. However, it appears that the Teller group found that only ounces of tritium would be needed to convert a fission bomb to a much more powerful fusion weapon. The new improved bomb could contain hundreds of pounds of deuterium, heavy hydrogen of weight two. Deuterium can be produced without sacrificing tritium, the triple-heavy hydrogen isotope.

Also, a new compound of light lithium 6, it was found, could be used to convert deuterium to tritium, the essential kindling ingredient, inside the bomb as it explodes, thus eliminating entirely the necessity the sacrifice of plutonium.

The More the Better

In the case of the old-style fission weapon the size of the explosion is somewhat limited by the amount of material that can be forced to fission before the weapon blows itself apart. The new hydrogen bomb apparently has no such inherent limitation: the explosive hydrogen is like gasoline—the more you pour on the fission-bomb kindling the bigger the blaze.

In his testimony, Dr. Teller estimated that the United States in 1947 could have achieved the hydrogen weapon that it now has if full resources had been thrown into research at the end of World War II. At that time, many physicists and other scientists were eager to return to academic work. Recruitment of competent men, according to the testimony, was difficult.

Dr. Teller testified that the "very brilliant discovery" was a piece of scientific reasoning that would have inevitably been made if a number of competent men had been assigned to ponder the hydrogen bomb problem. In his testimony, he modestly declined to take great credit for the invention, although credit was given to him by his colleagues.

Associated Press

LOST SUMMARY: Eugene M. Zuckert, A. E. C. commissioner, who had semi-secret files on Oppenheimer case but missed them when he left train at Stamford, Conn., Friday night.

SCIENTIST ALSO ATE 'CHICKEN LUNCHEON'

Special to The New York Times.

WASHINGTON, June 16—Robert T. Stevens, Secretary of the Army, is not the only victim of the "chicken-luncheon" technique, according to a University of California scientist.

Dr. Luis Walter Alvarez, testifying in the Oppenheimer investigation, explained that he had signed a 1950 report by a committee of scientists under circumstances similar to the McCarthy-Stevens luncheon last Feb. 24.

To his surprise, he said, the report was interpreted as an argument against giving top priority to development of the hydrogen bomb.

Dr. J. Robert Oppenheimer wrote the report as head of the special "Long Range Planning Committee," which met at the Pentagon, Dr. Alvarez told the investigating board.

"We have," he said, "a recent example of a man more skilled than I in the political field who thought after having a meeting with another gentleman that he had his points across. He signed the document and went out of the room saying, 'I have won my point,' and he took a terrible beating in the press.

"I find that I was in the same position. * * * I found out later that I had been had, if you don't mind my using that expression."

At the Feb. 24 luncheon, Secretary Stevens, Senator Joseph R. McCarthy and others signed a "memorandum of agreement." In it, the Secretary consented to lift an order prohibiting Army officers from testifying before the subcommittee. This was interpreted as capitulation to Senator McCarthy.

DATA ON HEARING LOST, THEN FOUND

Digest of Oppenheimer Case Was Mislaid by or Stolen From Zuckert on Train

WASHINGTON, June 16 (AP)—Representative Carl Hinshaw, Republican of California, said today a semi-secret summary of the proceedings in the security case of Dr. J. Robert Oppenheimer, and some secret Federal Bureau of Investigation documents, were lost or stolen last Friday night.

They were recovered again Sunday, in a lost-and-found railway ofifce, but Mr. Hinshaw said the summary's temporary disappearance was one reason the Atomic Energy Commission voted to release a transcript of the proceedings.

Mr. Hinshaw said the Senate-House Atomic Energy Commission, of which he is a member, received a letter from the A. E. C. telling this story:

Eugene M. Zuckert, A. E. C. commissioner, had the papers with him but missed them when he left a train Friday night at Stamford, Conn.

The commission got in touch with all the witnesses in this country and obtained their consent to publication.

The commission has said it would rule on whether Dr. Oppenheimer should continue to be barred from atomic data as a security risk by the end of this month.

Mr. Zuckert was not available for his version of how he came to lose the papers.

Statement by A. E. C.

There had been some question raised about publishing the transcript after witnesses had been told the whole proceeding was confidential. However, the A. E. C. issued the following explanation before Mr. Hinshaw told the Zuckert story:

"The wide national interest and concern in the matter make inevitable and desirable close public examination of the final determination.

"A. E. C. security clearance proceedings are conducted in privacy. The commission's personnel security regulations provide for closed hearings. The commission protects the privacy of the individuals concerned in such proceedings if they desire.

"In this instance, privacy was maintained for the hearings by the board. However, Dr. Oppenheimer's attorneys, as was their privilege, have issued texts of some documents. In the present circumstances, release of the transcript, within the limits of security, will in the opinion of the commission, best serve the public interest."

Excerpts From Testimony of Leading Witnesses in Security Hearings on Oppenheimer

Witnesses Who Testified in Oppenheimer's Behalf

The New York Times

Dr. Norris E. Bradbury
Director of Los Alamos Laboratory

Dr. James B. Conant
U. S. High Commissioner for Germany

The New York Times Studio

Dr. Vannevar Bush
President of Carnegie Institution

Dr. Vannevar Bush

[Dr. Bush, questioned by Mr. Garrison, said he had known Dr. Oppenheimer "since the early days of the war." He testified that at Los Alamos Dr. Oppenheimer "did a magnificent piece of work. More than any other scientist that I know of he was responsible for our having an atomic bomb on time."

[Mr. Gray asked Dr. Bush about the hydrogen bomb.]

Q.—Would it have been your guess that the Soviets would have attempted to develop this kind of weapon?

A.—Why, certainly, because it is very valuable indeed to them. To us, with 500 KT fission bombs I have very little need for a 10 megaton hydrogen bomb. The Russians, on the other hand, have the great targets of New York and Chicago, and what have you, of enormous advantage to them.

Q.—So they probably would have sought to develop this in the event unless some international control machinery had been in effect.

A.—That is right.

Q.—And our not proceeding, as some people thought we should, probably didn't have any relation to what the Russians would do about it.

A.—I think it has relation to what the Russians might do because it because whether we proceed or not determined to some extent the speed with which they would proceed. Let me interpose a word there, Mr. Chairman.

* * *

It was not a question, as I understand it, of whether we should proceed or not. It was a question of whether we should proceed in a certain manner and on a certain program. I have never expressed opinions on that. But certainly there was a great deal of opinion which seemed to me sound that the program as then presented was a somewhat fantastic one. So it was not a question of do we proceed or do we not. I think there was no disagreement of opinion as to whether we ought to be energetic in our research, whether we should be assiduously looking for ways in which such a thing could be done without unduly interfering with our regular program. The question of whether we proceeded along a certain path—may I say one more word on that, Mr. Chairman, quite frankly, and I hope you won't misunderstand me, because I have the greatest respect for this board. Yet I think it is only right that I should give you my opinion.

I feel that this board has made a mistake and that it is a serious one. I feel that the letter of General Nichols which I read, this bill of particulars, is quite capable of being interpreted as placing a man on trial because he held opinions, which is quite contrary to the American system, which is a terrible thing. And as I move about I find that discussed today very energetically, that here is a man who is being pilloried because he had strong opinions, and had the temerity to express them. If this country ever gets to the point where we come that near to the Russian system, we are certainly not in any condition to attempt to lead the free world toward the benefits of democracy.

Now, if I had been on this board, I most certainly would have refused to entertain a set of charges that could possibly be thus interpreted. As things now stand, I am just simply glad I am not in the position of the board.

Q.—What is the mistake the board has made?

A.—I think you should have immediately said before we will enter into this matter, we want a bill of particulars which makes it very clear that this man is not being tried because he expressed opinions.

* * *

DR. WARD V. EVANS [member of the Board].—Dr. Bush, then your idea is that suppose I was asked to serve on this board, and I didn't know anything about it—I had not seen any of this material—after I had agreed to serve, and saw this material, I should have resigned?

DR. BUSH.—No, I think you simply should have asked for a revision of the bill of particulars.

* * *

MR. GARRISON.—Dr. Bush, have you had some experience in handling security questions in the past?

A. Throughout the war, I was responsible for security in the Office of Scientific Research and Development. * * *

I might say in passing that there were a good many appointments, and I know of no case in which an appointment on O. S. R. D. was made in which disloyalty has since been proved. I am proud of that record. I think our procedure in clearance at that time was a sane and reasonable one and effective one.

Dr. James B. Conant

[Dr. Conant was asked by Mr. Garrison if he was appearing "at our request" and Dr. Conant agreed that he was. He said he had known Dr. Oppenheimer since the "early discussions of the atomic bomb affair."]

* * *

If it were true that Dr. Oppenheimer's opposition to the development of the hydrogen bomb were in any way connected with a sympathy which he might have had with the Soviet Union, or communism, then surely many other actions and decisions which he was involved in over the period of years in which I was associated with him would have likewise been influenced by any such point of view.

The record is quite the contrary.

One incident is a matter on which I think I can take some credit of calling to the attention of the Advisory Committee of getting ahead rapidly on methods of detecting any explosion that might occur in the atomic field by the Russians. I remember Dr. Oppenheimer may have picked that up before I did; he may have had the suggestion before I did, although I don't think so, and taking steps in the committee to see that something would be done in that regard.

Clearly anybody that was influenced by any point of view in favor of the Soviet Union could hardly have done that.

Another matter—the development of smaller atomic bombs which could be used for tactical purposes; support of the ground troops which in my judgment of military strategy seemed to me of great importance. He made strong statements about it. I think he was very active.

'There again it seems to me is an illustration of a definite action taken by this man which contradicts what seems to me the implied thesis in this part of the indictment.

There is a final matter which is not connected with the General Advisory Committee but which is of rather a personal nature. I spoke to the Committee on the Present Danger. That was a group of men that came together informally to make a public committee, started in the fall of 1950. The Korean War was going in a bad way. We believed that the United States Government was not taking proper steps to put itself in a strong military position, particularly with respect to the defense of Europe on the ground.

Late that year or early in 1951 we put out some statements urging Military Service and urging that we send more troops to Europe, generally the policy which has become the policy of the United States. Dr. Oppenheimer was asked to join that committee. He joined it. He subscribed to all those doctrines which were most vigorously anti-Communist. He spoke to at least one, I think, informal gathering where we were trying to raise some money to get ahead with a little of our propaganda work. Perhaps it is unnecessary to put on the record that I must admit that we had no success with our doctrine of Universal Military Service, but that is another story.

As far as the defense of Europe on the ground is concerned, things have followed the way we at least advocated.

* * *

Q.—Would you state very briefly for the board the reasons which lead you to make the recommendation which you did make on the subject of the hydrogen bomb?

A.—It is a very complicated thing. * * *

On the general strategic and political grounds there were some of the same reasons which we subsequently brought to a head on the Committee on the Present Danger, namely, this was supposed to be an answer to the fact that the Russians had exploded an atomic bomb.

Some of us felt then, and I felt more strongly as time went on, that the real answer was to do a job and revamp our whole defense establishment, put in something like Universal Military Service, get Europe strong on the ground, so that Churchill's view about the atomic bomb would not be canceled out.

One of the considerations was that this was sort of a Maginot Line psychology being pushed on us. On the technical ground the question was the investment in preparing certain materials which I am not going into, which are restricted, which seemed at that time necessary; the use of materials which I don't want to mention, which would be used up.

The question was when you expended a certain amount of manpower and energy and material, would you actually from the point of view of delivering blows against a potential enemy be very much better off even if this line worked?

* * *

[Gordon Gray, chairman of the Security Board, questioned Dr. Conant.]

MR. GRAY.—A summary of your testimony might be that so far as you have any knowledge about anything and on the basis of your best judgment you consider that Dr. Oppenheimer's character, loyalty and associations are such that he should have access to restricted data.

DR. CONANT.—Quite so. And I would give the specific items in which his judgment was such that if he had been influenced by pro-Communist views, or pro-Soviet views, he would not have taken those actions or decisions, and they were quite serious. In other words, this is not a general expression of belief based on casual conversations, but participating in a great many, I would say, fairly powerful anti-Soviet actions.

Prominent Scientists and Officials Relate Physicist's Role in Nation's Nuclear Programs

George F. Kennan

[Mr. Kennan was questioned by Herbert S. Marks, co-counsel for Dr. Oppenheimer.]

* * *

Q.—These problems that you are talking about, then, concerned the raw materials or at least in part concerned the raw materials problem?

A.—That is correct.

Q.—Raw materials for atomic energy.

A.—That is correct. They did. A collaboration was required between the two governments, and at the time that I came into these matters in 1947, it seemed evident to me that that collaboration was very seriously threatened by the way that events had developed to date, and it was time that both our Government and the British Government gave them the most serious thought.

We did that. I think it fair to say that we were successful in tiding these relationships over a very crucial and difficult period, primarily the period of the years of 1948 and 1949.

Q.—You are confident that the Russians would have profited greatly if the result had been opposite?

A.—Yes. * * *

Q.—Did Dr. Oppenheimer have a role of any importance in these deliberations that you have described?

A.—He was one of a number of officials, people in our governmental establishment, who were concerned with these matters. I say in our governmental establishment; I do not recall exactly what his position was at that time, but he was in councils of the Government about such matters, sat in on a number of these discussions, at least two or three that I recall specifically, I think.

* * *

Q.—As a result of your experience with Dr. Oppenheimer in the cases that you have reference to, what convictions, if any, did you form about him?

A.—I formed the conviction that he was an immensely useful person in the councils of our Government, and I felt a great sense of gratitude that we had his help. I am able to say that in the course of all these contacts and deliberations within the Government I never observed anything in his conduct or his words that could possibly, it seemed to me, have indicated that he was animated by any other motives than a devotion to the interests of this country.

Q.—Did you ever observe anything that would possibly have suggested to you that he was taking positions that the Russians would have liked?

A.—No. I cannot say that I did in any way. After all, the whole purpose of these exercises was to do things which were in the interest of this country, not in the interests of the Soviet Union, at least not in the interests of the Soviet Union as their leaders saw it at that time. Anyone who collaborated sincerely and enthusiastically in the attempt to reach our objectives, which Dr. Oppenheimer did, obviously was not serving Soviet purposes in any way.

Q.—Have you said that he contributed significantly to the results? A.—I have, sir.

Q.—Mr. Kennan, is there any possibility in your mind that he was dissembling? A.—There is in my mind no possibility that Dr. Oppenheimer was dissembling.

Q.—How do you know that? How can anybody know that?

A.—I realize that is not an assertion that one could make with confidence about everyone. If I make it with regard to Dr. Oppenheimer it is because I feel and believe that after years of seeing him in various ways, not only there in Government, but later as an associate and a neighbor, and a friend at Princeton, I know his intellectual makeup and something of his personal makeup and I consider it really out of the question that any man could have participated as he did in these discussions, could have based his thoughts to us time after time in the way that he did, could have thought those thoughts, so to speak, in our presence, and have been at the same time dissembling.

I realize that is still not wholly the answer. The reason I feel it is out of the question that could have happened is that I believed him to have an intellect of such a nature that it would be impossible for him to speak dishonestly about any subject to which he had given his deliberate and careful and professional attention.

That is the view I hold of him. I have the greatest respect for Dr. Oppenheimer's mind. I think it is one of the great minds of this generation of Americans. A mind like that is not without its implications.

Q.—Without its what?

A.—Implications for a man's general personality. I think it would be actually the one thing probably in life that Dr. Oppenheimer could never do, that is to speak dishonestly about a subject which had really engaged the responsible attention of his intellect. My whole impression of him is that he is a man who when he turns his mind to something in an orderly and responsible way, examines it with the most extraordinary scrupulousness and fastidiousness of intellectual process.

Witnesses Who Testified Against Oppenheimer

The New York Times

Dr. Luis W. Alvarez
University of California Professor

William L. Borden
Researcher for Westinghouse Electric

The New York Times

Dr. Edward Teller
"Father of the Hydrogen Bomb"

Identifications of Witnesses in Hearing

Following are brief identifications of witnesses who testified before the United States Atomic Energy Commission's Personnel Security Board hearing on Dr. J. Robert Oppenheimer:

ALVAREZ, LUIS WALTER—Physicist; helped develop radar research in World War II, Professor of Physics since at University of California; designed atom smasher known as lineal accelerator.

BACHER, ROBERT F.—Professor of Physics at California Institute of Technology; member of Atomic Energy Commission from 1946 to 1949.

BETHE, HANS A.—Professor of Physics at Cornell University; one of developers of atomic bomb.

BORDEN, WILLIAM LISCUM—Former executive director of Joint Committee on Atomic Energy, now doing atomic energy research with Westinghouse Electric Company; author of "There Will Be No Time," book on atomic warfare.

BRADBURY, NORRIS E.—Professor of Physics at Stanford University, then at University of California; succeeded Dr. Oppenheimer as director of Los Alamos Laboratory.

BUCKLEY, DR. OLIVER E.—Former chairman of board of Bell Telephone Laboratories; chairman of Science Advisory Commission of Office of Defense Mobilization in 1951-52.

BUSH, VANNEVAR—Scientist who figured largely in development of atomic fission as wartime director of Office of Scientific Research and Development; now director of Carnegie Institution of Washington.

COMPTON, DR. KARL T.—Member of Atomic Energy Commission panel and former president of Massachusetts Institute of Technology.

CONANT, DR. JAMES B.—United States Commissioner at Bonn, Germany, since 1953; president of Harvard University 1933-53; member of General Advisory Committee to Atomic Energy Committee 1947-52.

DEAN, GORDON—Member in 1949 and chairman, 1950 to June 30, 1953, Atomic Energy Commission; now member of banking firm of Lehman Bros. Energy Commission.

DUBRIDGE, DR. LEE A.—President of California Institute of Technology; member of General Advisory Committee of scientists which opposed "crash" development of hydrogen bomb.

FERMI, ENRICO—Nuclear physicist at University of Chicago. Refugee Italian who did pioneer work on atomic research while at Columbia University.

FISK, DR. JAMES B.—Vice president of Bell Telephone Laboratories; former director of research for Atomic Energy Commision, January, 1947, to August, 1948.

GLENNAN, DR. THOMAS KEITH—President of Case Institute of Technology at Cleveland; member of Atomic Energy Commission, August, 1950, to October, 1952.

GRIGGS, DAVID T.—Teacher of geophysics at University of California at Los Angeles; research associate in radiation laboratory at Massachusetts Institute of Technology, 1941-42; consultant to War Department, 1942-46.

GROVES, LIEUT. GEN. LESLIE R.—Retired Army officer; headed Manhattan (Atomic) Project in World War II; now vice president in charge of scientific research of Remington Rand.

HILL, ALBERT GORDON—Professor of Physics at Massachusetts Institute of Technology; former consultant for Brookhaven National Laboratory and Research and Development Board.

KELLY, MERVIN J.—Physicist, president of Bell Telephone Laboratories; former member of civilian committee to study national air defense; chairman of evaluation committee for National Bureau of Standards.

KENNAN, GEORGE F.—Former Ambassador to Russia, former chief of State Department Policy Planning Staff; now engaged in research at Institute for Advanced Study at Princeton, N. J.

LANSDALE, COL. JOHN JR.—Formerly in charge of security at atomic bomb laboratory at Los Alamos, N. M.; Government witness at spy trials of Julius and Ethel Rosenberg and Morton Sobell.

LATIMER, DR. WENDELL M.—Former dean of College of Chemistry at University of California; a director for Manhattan Engineering District program 1943-46 and atomic energy contract 1947-48.

LAURITSEN CHARLES CHRISTIAN—Professor of Physics at California Institute of Technology and member of committee to advise on defense of North America against atomic attack.

LILIENTHAL, DAVID E.—Former chairman of Tennessee Valley Authority and chairman of Atomic Energy Commission from 1946 to 1950; originally was opposed to going ahead on development of hydrogen bomb.

MAJ. GEN. JAMES MCCORMACK JR.—Now assigned to Air Research and Development Command, Baltimore; former director of Military Application of Atomic Energy Commission.

MCCLOY, JOHN J.—Chairman of board of Chase National Bank of New York; Assistant Secretary of War, 1941-45; president of International Bank for Reconstruction and Development, 1947-49; United States High Commissioner for Germany, 1949-52.

OSBORN, FREDERICK H.—Industrial management expert and scientist; chief United States negotiator on United Nations atomic controls 1947-50; major general in Army Special services in World War II.

PASH, COL. BORIS T.—Chief of the Sixth Army Counter Intelligence Division at San Francisco; speaks Russian fluently; has done counter-intelligence work in Europe and Japan.

PIKE, SUMNER T.—Business man; financier, Government official; Securities and Exchange Commissioner 1940-46; member of Atomic Energy Commission from 1946-50.

PITZER, DR. KENNETH—Head of Chemistry Department of University of California; former research for Atomic Energy Commission.

RAMSEY, NORMAN FOSTER JR.—Professor of Physics at Harvard; group leader at Los Alamos Laboratory in World War II; former head of physics department at Brookhaven National Laboratory for Atomic Energy Commission.

RABI, ISADORE ISAAC—Nobel Prize physicist and chairman of Atomic Energy Commission's General Advisory Committee as successor to Dr. Oppenheimer; professor of physics at Columbia.

ROWE, HARTLEY—Chief engineer and vice president of United Fruit Company; in World War II consultant to Manhattan District; former member of General Advisory Committee to Atomic

VON NEUMANN, PROF. JOHN—Mathematician, member of Institute for Advanced Study; appointed to General Advisory Committee of Atomic Energy Commission in 1952.

WEIDMAN, WALTER G.—Chairman of Defense Department Research and Development Board; on leave of absence as head of Department of Chemical Engineering at Massachusetts Institute of Technology.

WILSON, MAJ. GEN. ROSCOE—Aided General Groves in business management of Manhattan Project; served as commandant of Air War College at Montgomery, Ala.; now commander of Third Air Force in England.

WINNE, HARRY A.—Chairman of Defense Department's panel on atomic energy; former vice president of General Electric Company and a director of American Gas and Electric Company.

ZACHARIAS, DR. JERROLD R.—Director of Laboratory of Nuclear Science and Engineering at Massachusetts Institute of Technology; served in A. E. C.'s Los Alamos Laboratory.

Following are excerpts from the testimony of leading witnesses in the hearings on Dr. J. Robert Oppenheimer before the Personnel Security Board of the United States Atomic Energy Commission. The excerpts have been selected in the main to cover phases of the controversy not previously explored extensively in textual matter published in The New York Times.

Norris E. Bradbury

[Dr. Bradbury was cross-examined by Roger Robb, Council for the board.]

Q—Doctor, I have one question uggested by your discussion with he Chairman about what might e the result had there been a 'residential directive in 1945 or 946 to undertake all out work n the H-bomb.

It has been testified here, Doc-or, that something happened in he spring of 1951, and that ac-elerated the successful develop-ent of the thermonuclear so hat work came to a successful onclusion maybe 18 months hereafter.

My question is, Supposing that omething had happened in 1945 r 1946, what would have been he result? How soon do you hink you would have had the ermonuclear weapon perfected?

* * *

A—This is a question that I ould answer this way. Had this lea occured in 1945, 1946,1947 or 948 or almost any time before it id occur, we would not have nown how to use it in an effec-ve military fashion. We were lready pursuing in the year ollowing the war those tech-iques, specifically in the fission eld, which made the imple-entation of this idea a practical hing. We had already conducted periments. I can't describe em for security reasons. They ere in the fission field, and bore rectly upon this field. Frankly, I may go back to one of your otential questions, had there en a Presidential directive to oceed along thermonuclear lines ies in 1945, I would almost ubt in retrospect that we would ve done or could have done ything much different than we . In other words, the active ploration of the fission field as a necessary and essential erequisite known all along to e fusion field. Had there been ch a hypothetical decision, it is possible to answer. Had there en, we would have done exactly we did. We might have been rsuaded otherwise, and I think we had we would have found rselves farther behind in 1954 an we are.

Q—Your answer about not owing how to use this discov-y in 1946 or 1947, could you ex-din that a bit further?

A—I would have great diffi-lty in doing so without going o restricted data. Let me nk for a moment to see if I n find some way around this. There would be two possibili-s. We would not have been le to make the relevant calcu-ions for mechanical reasons. would not have been able to ke them for let us say techno-ical reasons, because only in course of those years did we gin to get some understanding of how to compute atomic or fission bombs. * * *

Q.—Doctor, in the years between 1946 and 1950, did you have the staff and the equipment then to do what you did subsequent to this discovery in 1951?

A.—Between when did you say, 1945 and 1950?

Q.—Yes, sir. In other words, assuming this discovery in 1945, 1946, or 1947, did you then have the staff to do what you did with the discovery in 1951 and 1952?

A.—As you are doubtless aware, in 1945 the laboratory of course was partly civilian and partly military. We had a couple of thousand SED, special engineering detachment of the military personnel. We had a number of officers. In 1945 and 1946, a great part of our civilian personnel left to return to school, to their industrial and academic jobs. The size of the laboratory reached its minimum roughly in September of 1946, at which time its size was roughly half, perhaps a little less than half of its size at the present time. From that time on it has grown steadily up to about the present time.

There were admittedly difficulties in taki : the laboratory through the transition pe iod prior to the Atomic Energy Act, while personnel straightened themselves out in their own desires. In 1946, throughout the entire year, or at least until the adoption of the Atomic Energy Act, perhaps we were lucky to keep ourselves alive. We had the Crossroads Operation to carry out, and life was far from easy. I don't say it has ever been easy, but in those days certainly our task was not simple. We were developing, as I have said earlier, our major directed effort, the efforts which come to the peaks of these pyramids of development, two things which would make the production capacity of the United States as effective in a military way as it possibly could be right then and there. We were also devoting our efforts to making atomic weapons as they then existed more effective as part of a weapons system for the country; in other words, an effort to maximize the immediate potential of the country.

As I have said earlier this was not to the exclusion of thermonuclear work but it was the focus of achievement which was in the fission field. We would have had a hard time and unprofitable time and I think in the light of subsequent events, and it would have been an error and mistake to try to hash about in a field for which none of the basic technologies then existed, and at a time when there were very clear things to be done in the fission field.

Dr. Luis Walter Alvarez

[Dr. Alvarez testified under direct examination by Mr. Robb.]

Q.—Doctor, directing your attention to September, 1949, when the Russians exploded their first atomic bomb, did that cause some concern on your part?

A.—Yes; it caused a great deal of concern on my part. I tried to make up my mind what was the right thing to do. I had been spending four years doing basic research again. I think of it as sort of being recharged after five years of military development work. I had to take a while to get back into the frame of mind of a practicing physicist. I had been concentrating my attention on that phase of my career and how, suddenly, it appeared that a crisis had arrived and perhaps I should get back into the field of atomic energy.

Q.—Why did you think a crisis had arrived?

A.—The Russians had exploded an atomic bomb, and I thought that your own program had not been going terribly fast. It certainly had not been going at nearly the rate it had during the war, but this is quite natural.

Q.—Did you discuss with any of your colleagues what ought to be done?

A.—Yes, I did. I saw Professor [Ernest O.] Lawrence the next day, and I told him that I thought we should look seriously into the business of constructing the super weapon which had, as far as I knew, been neglected in this four-year period. I had not followed the situation closely enough to be sure that it had been neglected but that was my impression.

Q.—Did you make any inquiry to see whether or not your feeling was correct as to whether it had been neglected?

A.—Yes. Professor Lawrence and I got on the phone that afternoon and called Edward Teller at Los Alamos and asked him if we could come down and talk to him in the near future, and, as I remember, within a day or two, we took a plane to Los Alamos where we did talk to Dr. Teller and found out the present rather inadequate status of the super program.

* * *

[Dr. Alvarez referred to a lunch with Dr. Oppenheimer in October, 1949.]

Q.—Did you have lunch with him?

A.—Yes. We went to a small restaurant in the immediate neighborhood of the Commission building, and that was the first occasion that Dr. Oppenheimer told me of his views on the building of the hydrogen bomb.

Q.—What did he tell you?

A.—He said that he did not think the United States should build the hydrogen bomb, and the main reason that he gave for this if my memory serves me correctly, and I think it does, was that if we built a hydrogen bomb, then the Russians would build a hydrogen bomb, whereas if we did not build a hydrogen bomb, then the Russians would not build a hydrogen bomb.

I found this such an odd point of view that I don't understand it to this day. I told Dr. Oppenheimer that he might find that a reassuring point of view, but I didn't think that very many people in the country would accept that point of view?

* * *

Q.—What was the impact of all this on you?

A.—Well, for the first time I realized that the program that we were planning to start was not one that the top man in the scientific department of the A. E. C. wanted to have done.

* * *

Q.—I will ask your opinion, Doctor. Suppose the thermonuclear program had gone ahead full-scale beginning in 1946, how soon do you think we would have gotten the weapon?

A.—That is a very difficult

David E. Lilienthal

[Mr. Lilienthal was questioned by Samuel J. Sliverman, counsel for Dr. Oppenheimer, about a talk with J. Edgar Hoover, Director of the F. B. I.]

Q.—Will you tell us of that discussion?

A.—Whether Mr. Hoover had one of his associates there or not, I am not sure, but from the Commission it was Mr. Joseph Volpe Jr., Deputy General Counsel at that time. I am not too clear, but I think the acting security officer was with us, whose name was Tom Jones. My recollection is not too clear here. My recollection of that conversation is as follows:

First there seemed to be general agreement, or I expressed the view that here was a man [Dr. Oppenheimer] who had certainly contributed a great deal to the military strength of the United States under circumstances of great difficulty and so on. Everyone we had consulted who had worked with him * * * were clear that this was true, that he had done a good job. Mr. Hoover said there could not be any question about that.

Then the question was discussed as to the relevance, as to the weight to be given this long series of associations with left wing and crackpot and communistic sorts of organizations or people of which the record contained a great deal of information. On this I reported to Mr. Hoover that we would like to know whether there was something in this that we had missed but that our evaluation of it was that on the whole record in view of what had happened since that time, that Dr. Oppenheimer had proved by his work, by his activities, by the things he had done for his country, that he was not only loyal, but that he had character that made him suitable as an employee of the Atomic Energy Commission.

Then Mr. Hoover said—this is my impression—of course, Mr. Hoover makes it a point not to evaluate these reports. * * * But when I asked him if there was anything that we had missed or any implication that we had not seen that perhaps he, with his closer knowledge of the file might see, he said, well, the only reservation he had was that he didn't like that episode about—what is his name, a French name, * * *

Yes, Chevalier. That Oppenheimer did report it finally, but he waited an awful long time, and he criticized that. He was quite critical of it. Of course, I completely agreed with that.

* * *

[Reference was made to a memorandum of his views that Mr. Lilienthal had written to the National Security Council.]

Q.—Would it be fair to say that your reliance on the G. A. C. [General Advisory C mmittee] was great as to technical matters and the further away it got from technical matters, the more your reliance was on other agencies, and, on your own judgment and on other departments of the Government?

A.—During the first phase of my participation in this matter before we had any important contact with the military or any contact with the State Department—obviously that didn't contribute to any views I had—I did have great respect for the views of the G. A. C. on technical matters. I took very much to heart their statement that their conclusions were planted in technica considerations. I had such respect for the wisdom of men like [James B.] Conant and Oppenheimer and [Enrico] Fermi and other men that I certainly paid close attention to what they said on matters that were not technical. I think the best evidence I came out with were the things I wrote at the time, some of which they would not endorse and were not included in their views It is hard to divide on these things. I am sure of the importance I assessed to the technical view, and the rest is another matter that is hard to define.

Q.—This memorandum was dated Jan. 31? A.—Yes.

Q.—And you resigned Feb. 15? A.—It took——

Q.—At least your resignation took effect Feb. 15? A.—It was the third stage, that is right.

Q.—You did not ask to have your clearance continued? A.—No.

Q.—So I take it you do not know whether the hydrogen bomb that we hear about in the newspapers has any relation if any to the things talked about in 1949? A.—No, I have had no access to restricted data since that time, and no occasion to use it.

Q.—As a result of your experience with Dr. Oppenheimer and your knowledge of him, have you formed an opinion as to his loyalty, his integrity, his character, all the other factors that go into forming a judgment as to his loyalty, security? A.—Yes, I have.

Q.—What is your opinion? A.—I have no shadow of a doubt in my mind that here is a man o good character, integrity and o loyalty to his country.

Q.—How would you assess hir as security risk? A.—I did no regard him up until the time m kn—[illegible]ge of the program censec and had no occasion to regard him as a security risk.

Q.—I think you already indi cated that in March, 1947, you consciously assayed the situation and came to the conclusion that he was not a security risk? A.—Yes. At that time we had this file before us and that was my conclusion, that in the light of the over-all picture, taking everything into account, the minus signs were very few indeed, and the plus signs very great indeed, and I thought he was a contribution to the security of the country. I have had no occasion since that time to change that view.

Q.—Has your experience with him confirmed that view? A.—My experience from that time di confirm that view. I am sure tha it is clear that he has made grea contributions to the security o the country.

Gordon Dean

[Mr. Dean reviewed the background of the hydrogen bomb. Lloyd K. Garrison of Dr. Oppenheimer's counsel questioned him.]

MR. DEAN—* * * in the fall of 1949 * * * the then Chairman of the Commission, Mr. [David E.] Lilienthal, had asked the General Advisory Committee very specifically to review this question of whether we should attach a high priority to a thermonuclear or fission program. They were asked this question. They were asked to consider it at their meeting which took place in October 1949.

They did consider it. I think they considered little else, I think for about three days, than this issue. They came in with their report to the effect that they felt it was a mistake.

The reasons that they gave I suppose appear in the minutes of the General Advisory Committee, but we had many discussions and those don't appear in the minutes. * * *

These were men who had developed the A-bomb. Oppenheimer had the big hand in it, as you know. He also had a hand in the measures for the international control of atomic energy * * *.

They were hopeful at that time that you would not have the world in the position where you had two great powers simply stockpiling weapons and no solution to the problem. Consequently, after two or three years of rather frustrating dealings with the Russians, when this proposal of building another bigger one hit them, as some said, as the answer to our national security I think it rather floored them and disgusted them. They lived through the A-bomb. They tried to get international control. If this was the only answer to the problem, namely, of building bigger H-bombs, this was not a satisfactory answer for those people. I think it was a stomach reaction along those lines.

I did not agree with it, but I think I can understand it.

* * *

There was another reason. This was how much of a diversion of Los Alamos—energies, scientific energies—could you safely divert to a project which might or might not succeed when the ball was rolling so beautifully in your A-bomb program, and we were getting more bang out of our fissionable material, more weapons for the same amount of fissionable material.

Those were all considerations. There may have been others in there that I have overlooked, but those are the principal ones.

* * *

Q.—What was your belief as to Dr. Oppenheimer's loyalty after you had been through [his] file and had talked with him [in 1950]?

A.—There was no question in my mind—I must say when I first looked at the file, I had doubts, largely growing out of these early associations—but there was never any doubt in my mind after I examined the file and based partly on my knowledge of Dr. Oppenheimer, which was very close, there was never any doubt as to his loyalty in my opinion. None. That decision had to be made one way or the other. It could not be half way. There were some very unpleasant early associations when you look at them in retrospect, but as far as his loyalty I was convinced of it, not that the file convinced me so much, but the fact that here was a man, one of the few men who can demonstrate his loyalty to his country by his performance. Most people illustrate their loyalty in negative terms. They did not see somebody. Here is a man whoe had an unusual record of performance. It is much broader than I have indicated so far.

Q.—Would you state to the board your general impression of his character as well as his loyalty, his integrity and sense of discretion? How would you rate those qualities?

A.—I would say that he is a very human man, a sensitive man, a very well educated man, a man of complete integrity in my association with him. And a very devoted man to his country, and certainly to the Commission. No question of these things in my mind.

* * *

Q.—* * * Would you give us your impression of Dr. [Edward] Teller's personality, particularly with reference to the problem of recruitment?

A.—Dr. Teller is a very, very able man. He is a genius. There is no question about it. He has contributed much in the way of ideas to our weapons development. He is a very good friend of mine, and I admire him. He is a very difficult man to work with, as sometimes happens. Dr. Teller did not work well at Los Alamos, and left there on two occasions. I was responsible on both occasions for getting him to go back. I was finally responsible, I think in part, for finding a haven for Dr. Teller, because we needed him. But you can't break up a whole Los Alamos laboratory for one man, no matter how good he is, and that was a problem.

SECTION II
EDITORIALS
Commentators—Books

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SUNDAY, JULY 25, 1954 1B

CURRENT EVENTS
Area Affairs, Country Livin'
SECTION II

Drama Packs Amazing Oppenheimer Transcript

By Alfred Friendly
Assistant Managing Editor, The Washington Post and Times Herald

THE Government Printing Office a few weeks ago turned up as the unlikely publisher of the most significant and controversial book of the year.

It is about the length of the Bible, has a plot more intricate than "Gone With the Wind" and has half as many characters as "War and Peace." Depending on your reading speed, it takes from 20 to 30 solid hours to complete.

It reflects on the troubled social and political world about us perhaps more deeply and disturbingly than any other book published during the cold war.

It is a treasure trove for statesmen, moralists, scientists and military intelligence officers on both sides of the Iron Curtain. It is a source book for a generation of historians to come. It is quite possibly the raw material for dozens of future dramatists, novelists and social philosophers.

You can get it, all 993 pages, for $2.75 from the Superintendent of Documents. Its title is: "In the Matter of J. Robert Oppenheimer: Transcript of Hearing Before Personnel Security Board."

Gen. Leslie R. Groves, former head of the Manhattan Project, plays a Falstaffian role in some of the testimony before the Gray Board.

It is a typographical monstrosity, a prodigious task to read, a frustrating and infuriating document. It is also absorbing, every page of it.

Its drama is Aristotelian, in the sense that the principal figure of the tragedy is a king whose fate illustrates and illumines the life and problems of the spectators, which is to say all of us. The story is that of a towering figure, the father of the atomic bomb, the center if not the founder of the American school of theoretical physics.

The drama is also Shakespearean, in that the central problems are those of motive. As in Shakespearean drama, even after the play ends the audience can argue endlessly about just what the motives were of everyone concerned, the witnesses and the judges certainly as much as the judged. The delineation of character, as 40 persons come to testify, is also Shakespearean in its richness and variety.

There is a touch of Eric Ambler, too, in allusions to espionage, although the master of the spy thriller would never have been guilty of concocting such implausible fictions as the suggestions of some of Oppenheimer's detractors.

There is a bit of opera bouffe in the allegation that a mysterious cabal, with the sinister name of ZORC, plotted to siphon off the budget of the Strategic Air Command.

A Tragic Love Story

THERE are a couple of love stories, both pathetic, ill-starred and told with merciful economy.

In one, the subject is Jean Tatlock, Oppenheimer's fiancee, seen searching vainly in communism for some solace or answer to a disturbed quest; shortly before she dies tragically she asks to see Oppenheimer again, because she still loves him.

The second tells of Mrs. Oppenheimer, immersed briefly in communism through love of a dashing and romantic Communist first husband. We see her, disillusioned with the party as early as 1936, follow him drearily and wearily to Paris, only to receive word that he died fighting with the Loyalists in Spain.

There is a monumental amount of military and political and technical information in the book, despite the deletions of the security officers who went over the transcript. It contains probably more information on atomic and thermonuclear progress than any single volume since the Smyth Report. It is the basis for a reasonably complete history of the hydrogen bomb, and some scholars and reporters are already at work compiling it.

There is in the book also one or two touches of buffoonery, with Gen. Leslie R. Groves, wartime head of the Manhattan Engineering District and Oppenheimer's boss when the atom bomb was made, playing the role of Falstaff.

The Two-Bomb Guess

GROVES, who is supposed to be testifying about Oppenheimer, gets off at once with the announcement that, before Yalta, he concluded that "we needed only two (atomic) bombs to win the war." With becoming modesty, he notes, "Of course, I also proceeded on the theory that I might be wrong."

He acknowledges that he consulted with Oppenheimer frequently during the life of the atomic bomb project and used him at least on one tough theoretical problem, "not to tell me what to do but to confirm my opinion."

But, Groves concedes, Oppenheimer was most valuable, and "he did a magnificent job as far as the war effort was concerned. In other words, while he was under my control—and you must remember that he left my control shortly after the war was over."

At the outset of Groves' testimony, he was warned by Chairman Gordon Gray of the special Personnel Security Board not to discuss classified matters. Don't worry, Groves replied in effect, "I will watch out for that. I have been watching out for that for so many years I don't think I will slip."

Asterisks in the transcript show that the security officers had to apply their scissors three times to the immediately succeeding 200 or 300 words of Groves' testimony.

750,000-Word Document

WITH supporting documents, such as the decisions, briefs and communications, the Oppenheimer case runs to some 750,000 words. Perhaps 100,000 words bear on a series of minor incidents. These were later made much of in the decisions of the Gray Board and the Atomic Energy Commission, but they are essentially secondary.

Some would see them as not much more than nit-picking. It is impossible to conclude that the case against Oppenheimer rests in any large part upon them. Space does not permit their discussion here.

The central items which constitute the core of the case against Oppenheimer are two. In roughly equal proportions, they occupy the rest of the transcript.

One is the Chevalier episode. The other is the matter of Oppenheimer's views and activities with respect to the thermonuclear bomb (the "superbomb" or the "hydrogen" bomb).

In their majority decision, Gray and Board Member Thomas A. Morgan made much of the thermonuclear bomb item, finding that Oppenheimer delayed its development by opposing it and failing to be properly enthusiastic about it. It was principally this aspect of their report that provoked a storm of criticism and contempt among the Nation's press and other voices of opinion.

When the AEC made its final decision a few days later, the four members ruling against Oppenheimer loudly and vehemently protested that the issue had utterly no bearing on their findings (except for a question of Oppenheimer's candor in his testimony about it). One might gather from their intense disclaimers that the idea never crossed their minds that J. Robert's H-bomb views and actions might have security risk implications.

The Essential Charge

IT may be assumed, however, that those views and actions crossed someone's mind, since the story occupies some 300,000 or 400,000 words of the transcript.

The charge against Oppenheimer on this point in the letter to him from AEC general manager K. D. Nichols was essentially this:

That from 1945 to 1949 Oppenheimer thought the hydrogen bomb was feasible and realizable, but that after the Russians exploded their first atomic bomb, Oppenheimer opposed developing the thermonuclear weapon on moral grounds, because it was not feasible, because it was politically undesirable and because there were not enough facilities and skilled scientists to push its development; further, that once it was decided by President Truman to push development of the H-bomb anyway, Oppenheimer continued to oppose the project and did not cooperate fully with national policy.

In a roaring and exciting three pages of the transcript, Dr. Vannevar Bush, the Grand Old Man of American science, told the Gray Board it should have refused to entertain such a charge. He said the board should have sent the letter back to General Nichols for redrafting.

The letter, he said, "is quite capable of being interpreted as placing a man on trial because he held opinions, and had the temerity to express them.

"If this country ever gets to the point where we come that near to the Russian system, we are certainly not in any condition to attempt to lead the free world toward the benefits of democracy.

"I think that in all fairness I ought to tell you my frank feeling that this has gotten into a very bad mess . . . we have been slipping backward in our maintenance of the Bill of Rights . . .

"I think this board or no board should ever sit on a question in this country of whether a man should serve his country or not because he expressed strong opinions. If you want to try that case, you can try me. I have expressed strong opinions many times, and I intend to do so. They have been unpopular opinions at times. When a man is pilloried for doing that, this country is in a severe state."

Allegation of Treason

GRAY and Morgan apparently did not concur. To do them justice, it may be assumed that, after the charge was made, there were a couple of implications which, if read into it, might properly be the subject of examination. The implications deal with Oppenheimer's acts, not his opinions.

One was that Oppenheimer's activities had sinister and disloyal motivations. The other was that, regardless of motive, Oppenheimer, as a Government servant and consultant, betrayed his trust by deliberately trying to oppose a national policy already decided upon.

There is one direct allegation in the whole hearing that Oppenheimer, who did indeed oppose an all-out effort to make the H-bomb in 1949, did so because he is a Soviet agent. It was made in a letter of November 7, 1953, to J. Edgar Hoover. The letter, distributed by Hoover to top Government officials, kicked off the whole investigation of Oppenheimer.

The author was William L. Borden, former executive director of the Joint Congressional Committee on Atomic Energy. He had access to derogatory information on Oppenheimer in the investigative files of the FBI and other agencies, but presumably had no other sources.

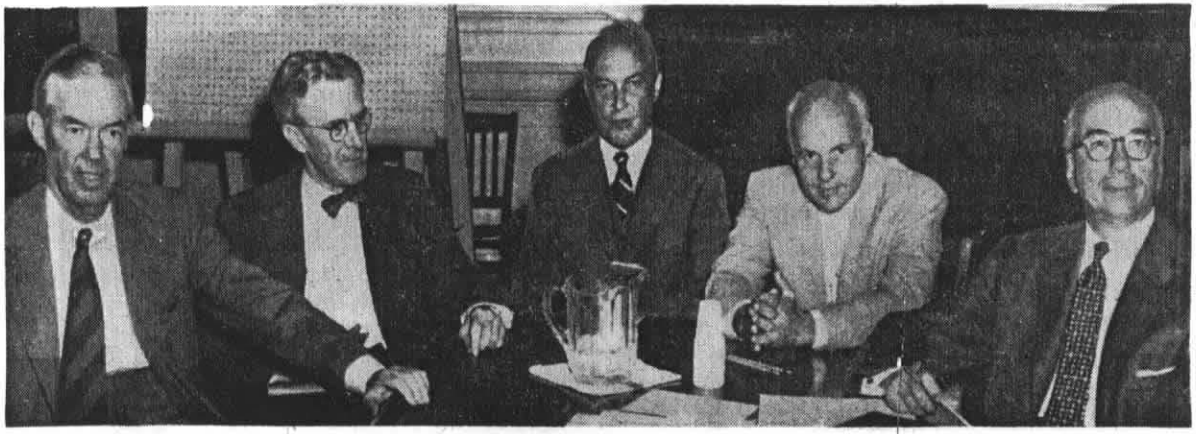
Borden demonstrated himself, to put the most charitable view on the matter, as an incompetent reporter. The evidence against Oppenheimer, which was surely spread in full on the transcript, comes nowhere close to his description of it; the essential conclusions he drew were as baseless as they were false.

His reading of the evidence and his conclusions were given no credence, indeed no mention, by the Gray Board or the AEC in their final reports.

Leading defenders of Dr. Oppenheimer, as shown by the transcript, included Dr. Vannevar Bush (left), Grand Old Man of American science, and former Ambassador George F. Kennan (right).

Dr. J. Robert Oppenheimer, towering figure of American science, is the protagonist in the drama which emerges from the 993 pages of transcript.

Detractors of Dr. Oppenheimer included the brilliant but difficult Dr. Edward Teller (left) and William L. Borden (right), former executive director of the Joint Congressional Committee on Atomic Energy.



The Atomic Energy Commission, which voted 4-to-1 to deny Dr. Oppenheimer's appeal to lift his suspension from access to security information. Left to right: Thomas E. Murray, Henry D. Smyth (who voted for Oppenheimer), Joseph Campbell, Eugene M. Zuckert and Chairman Lewis L. Strauss.

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Sunday, July 25, 1954

'Trial' Leaves Questions Dangling

THE WASHINGTON POST and TIMES HERALD Sunday, July 25, 1954

If there was no treasonable motive, the next question was whether Oppenheimer, regardless of motive, did indeed act against the national policy on the H-bomb, once it was adopted in the White House at the end of January, 1950.

There is no evidence whatsoever in the transcript of any positive acts that Oppenheimer took to delay the project once it was approved.

Gray and Morgan, however, went on to argue that since Oppenheimer did not wax publicly enthusiastic when President Truman overruled his recommendation against making the H-bomb, and did not try energetically to encourage scientists to join the H-bomb work at Los Alamos, he delayed the project.

It was this argument of lack of sufficient "enthusiasm" which aroused a storm of criticism from the Nation's press, scientists and public in general. It was the most vulnerable section of the Gray-Morgan Report. The third member of the Board, Dr. Ward V. Evans, who favored restoring Oppenheimer's clearance, put the matter in a nutshell:

"He did not hinder the development of the H-bomb, and there is absolutely nothing in the testimony to show that he did."

'Massive Retaliation'

IT is not enough for an understanding of the case to say that the AEC, even in ruling against Oppenheimer, rejected in its entirety this portion of the Gray-Morgan decision. The story of the H-bomb remains at the root of the case.

The hearing tells that story:

When the Russians exploded their first A-bomb in September, 1949, American expectations and American military strategy were knocked galley west. The basis of American defense, the atomic monopoly, was destroyed.

One school, which ultimately was victorious, reacted by calling for atomic superiority. It wanted bigger bombs—the H-bomb—and more bombs. Its ideas developed into what is now known as the policy of potential "massive retaliation" as a deterrent to war.

The other school, led by Oppenheimer, felt that this was a totally inadequate answer. Over and over again in his testimony and in that of such men as Vannevar Bush, I. I. Rabi, Hartley Rowe, David Lilienthal, George Kennan and many others, you find this reasoning, as of 1949:

- A decision to make the H-bomb would lead America into believing that that was all it had to do to answer the Russian threat. Yet it is obvious much more needed to be done—in atomic defense, in revising the balance of our forces, in new political, economic and social approaches to the world.
- The H-bomb, if developed, would be vastly more valuable to the Russians, by reason of available targets, than to us. Yet our effort to make it would impel the Russians to do likewise.
- Development of the H-bomb would end the possibility of any future agreement with the Russians on atomic control or disarmament.

The arguments may have been good or bad; they were at least arguable.

There were other reasons behind Oppenheimer's opposition in October, 1949-January, 1950, when he was chairman of the AEC's powerful General Advisory Committee. Militarily, it looked more sensible to build more A-bombs with available facilities. Technically, even if the H-bomb could be made, it would produce a lesser bang for a buck—or for a neutron, which was the limiting factor—than the equivalent number of A-bombs. Also, technically, the possibility of making an H-bomb did not look promising.

The General Advisory Committee went along with Oppenheimer virtually unanimously (one member took no position). So did three of the five AEC members, with Gordon Dean and the present chairman, Lewis L. Strauss, dissenting. The military and the Joint Congressional Atomic Committee were almost unanimously in favor of the H-bomb project (a notable exception was Rep. W. Sterling Cole, the present Committee chairman).

If many scientists saw the matter the way Oppenheimer did, some did not. Notably, the brilliant but difficult Edward Teller, who had been working on the thermonuclear problem since 1942—

Dr. I. I. Rabi, a "member" of "ZORC," told the board: "We have an A-bomb . . . and what more do you want—mermaids?"

with such fixity that he was removed from any work connected with World War II during the war—wanted a "crash" program at once. Of like mind was the famed Ernest O. Lawrence, at Berkeley, and the men around him.

The transcript shows an amusing, if weird, episode in this connection. To Lawrence and Dr. Luis W. Alvarez, it never occurred that our construction of the H-bomb would be anything less than automatic after September 23, 1949, when news of "Joe I," the Russian bomb, was announced. Checking with Teller, they dashed off into the most ambitious plans for staff and facilities and decided, all on their own, that, although without experience in the matter, they would build the atomic reactors to produce the neutrons to make the tritium to go into the H-bomb.

They proceeded energetically and blithely on this plan, only to find out when they came to Washington a month or so later that they were the men who brought the bull fiddle to a party at which no one wanted to hear music, much less their bull fiddle. Professionals, not the Berkeley group, were ultimately employed to build the reactors.

Impossible Concept

FOR a good 18 months after the White House decision, it looked as if Oppenheimer's views were even better founded than he knew. The H-bomb appeared increasingly more dubious; indeed, it is virtually certain that what was then contemplated can never be made. But in 1951, Teller came up with a "brilliant" invention or discovery or idea.

The classification officer's scissors leave us without knowledge of what it was, but one may guess. In any event, it is clear that the thermonuclear weapon now being made is a very different

See OPPENHEIMER, Page 7, Column 1

OPPENHEIMER, From Page 1

thing from the hydrogen bomb which people thought about in 1949.

With Teller's invention, Oppenheimer waxed highly enthusiastic and helped the project along, even by the testimony of Teller, who loses no love for J. Robert. In fact, Oppenheimer himself contributed an idea, "not very ingenious but . . . very useful," embodied in today's H-weapons.

Even so, Oppenheimer continued to insist that the H-bomb was not the whole answer. The hearing shows him fiercely energetic in projects pushing for continental defense, radar warnings and tactical, as well as strategic, use of A-bombs.

Air Force Enmity

IT is here that the transcript makes evident the furious and increasing enmity against Oppenheimer by the Air Force, or in particular, the Strategic Air Command. What Oppenheimer was saying was that war plans based virtually exclusively, on mass retaliation, by H-bombing Russian cities and installations, were not the be-all and end-all.

He wanted, in short, a more versatile, flexible, atomic arsenal. There are hints he was thinking even of atomic weapons to be used against oncoming flights of enemy bombers, and perhaps even atomic antisubmarine defense. But mostly he was insisting on vastly more attention to the tactical use of A-bombs and to continental air defense. The transcript shows that those who opposed him considered that point of view as first cousin to treason.

The transcript shows one active opponent in the person of Gen. Roscoe C. Wilson, former commandant of the Air War College. He testified at the hearings by order of the Air Force. Wilson told how Oppenheimer's "pattern of action" led him to go to the director of Military Intelligence to express "concern."

Some of the "pattern of action" he worried about has fallen to the classification officer's shears. But General Wilson said that one thing which made him worry was "my awareness to the fact that Dr. Oppenheimer was interested in what I call the internationalization of atomic energy, this at a time when the United States had a monopoly."

Origin of 'ZORC'

MOST ACTIVE of all, however, was David T. Griggs, chief scientist for the Air Force, who once admitted to Oppenheimer that he had impugned Oppenheimer's loyalty. Oppenheimer thereupon called him a paranoid, which doubtless did little to win Griggs' favor.

It was Griggs who saw in the Lincoln summer study project in 1952 a plot, engineered by a secret junta, to undercut the Strategic Air Command and deny it the budget it might otherwise obtain. He testified that a Massachusetts Institute of Technology scientist, Jerrold R. Zacharias, wrote the mystic letters ZORC on a blackboard at a scientific meeting, and explained that they stood for Zacharias, Oppenheimer, Rabi and Charles Lauritsen, another famous scientist. The implication was that this was a cabal of intriguers who plotted to damage SAC by the summer study.

Zacharias and others denied the story point blank over pages and pages of cross-examination. One may assume that every attempt was made to verify Griggs' account, but no verification appears in the record. Also, there is no explanation of why the cabal was not known as ZORL, with the L for Lauritsen instead of the C for Charles. ZORL would have sounded just as sinister as ZORC.

The ZORC story, incidentally, first saw the light of day in a Fortune article in May, 1953. The material for the piece obviously came straight from the office of the late Gen. Hoyt Vandenberg, Air Force Chief of Staff. It was a bitter attack on Oppenheimer, and the first public one by the Air Force.

By that time, Teller's invention had put a new light on the H-bomb, and Oppenheimer was vulnerable by reason of his 1949 recommendation. The heat was on.

Unequivocal Stand

IN HIS separate opinion condemning Oppenheimer, AEC member Thomas E. Murray rejected with devastating argument the Gray-Morgan viewpoint about Oppenheimer's H-bomb position. He insisted that Oppenheimer had every right to oppose the program for moral, political and technical reasons. In passing, however, Murray observed:

"Dr. Oppenheimer advanced technical and political reasons for his attitude to the hydrogen bomb program. In both respects he has been proved wrong; nothing further need be said."

Oppenheimer's technical opinion, as noted above, was both right and wrong: Right, in that the 1949 H-bomb notion has not been made into a bomb and probably cannot be; wrong in that Teller later came up with another idea for a different product, which could be made.

Other students of today's state of affairs are unable to share the dogmatic and doubtless comforting certainty that Murray enjoys about the wrongness of Oppenheimer's political reasons.

The Washington Post and Times Herald 7-25-54

H... ferring to failure to use the prospect of a successful American superbomb as a basis for another attempt at negotiating with Russia. He is speaking of a second chance we might have had, just before the Eniwetok test:

"The test marked our entry into a very disagreeable type of world . . .

"I still think that we made a grave error in conducting that test at that time, and not attempting to make that type of simple agreement with Russia. I think history will show that was a turning point that when we entered into the grim world that we are now entering right now, that those who pushed that thing through to a conclusion without making that attempt have a great deal to answer for."

And here is the testimony of former Ambassador George F. Kennan:

"It seemed to me at the end of this atomic weapons race, if you pursued it to the end—we building all we can build, they building all they can build—stands the dilemma which is the mutually destructive quality of these weapons, and it was very dangerous for us to get our public before the dilemma, that the public mind will not entertain the dilemma, and people will take refuge in irrational and unsuitable ideas as to what to do . . ."

Our Terrible Ability

GRAY asked him how he could make a distinction between the fact that he favored building A-bombs but opposed making the H-bomb in 1949. Kennan replied:

"It is because of the wonder on my part as to whether we did not already have enough of this sort of terrible ability to commit destruction . . . What has worried me, Mr. Chairman, about going ahead with this is that we would come to think of our security as embraced solely in the mathematics of whatever power of destruction we could evolve, and we would forget our security lies still very largely in our ability to address ourselves to the positive and constructive problems of world affairs, to create confidence in other people.

"I am convinced that the best way to keep our allies around us is not to pay outwardly too much attention to the atomic weapons and to the prospect of war, but to come forward ourselves with plans that envisage the constructive and peaceful progress of humanity . . .

"I have feared that if we get launched on a program that says the only thing we are concerned to do in the development of atomic weapons is to get as much as possible as rapidly as possible, that the attentions of the public and the Government will become riveted to that task at the expense of our ability to conduct ourselves profitably in positive aspects of foreign policy."

Oppenheimer was "convicted" by Gray and Morgan and by the four-man AEC majority primarily on the grounds of his association and dealings with Haakon Chevalier, a professor of French at the University of California, at Berkeley.

David Lilienthal, former AEC chairman, was one of the school which opposed the policy of "massive retaliation" proposed after the Russians exploded their first A-bomb in 1949.

Chevalier appears as an energetic parlor pink, to say the least, if not a member of the Communist Party. He was one of Oppenheimer's many Communist-tinged friends at a time when Oppenheimer himself was Red as a beet.

Here is the body of facts on which the transcript shows little or no disagreement:

In late 1942 or more probably early 1943, when Oppenheimer was doing secret work in Berkeley but before he went to Los Alamos, Chevalier made a startling observation to Oppenheimer. The scene was the pantry or kitchen of Oppenheimer's home.

... George Eltenton, a British scientist working in the Shell Development Co., had spoken to him about the possibility of transmitting technical information to Soviet scientists. This was a period when Russia was our "gallant ally" and when many people resented national policy which excluded Russia from a free and steady flow of knowledge of our secrets.

Oppenheimer replied to Chevalier in strong terms, saying he thought this was terribly wrong. The subject was dropped, immediately and finally.

Cock and Bull Yarn

SOME MONTHS LATER, in the summer of 1943, by which time Oppenheimer had gone to Los Alamos, Col. John Lansdale, Jr., an attorney from a distinguished Cleveland law firm who had become chief intelligence officer of the Manhattan District, told Oppenheimer of a worry about security in Berkeley. Lansdale's concern had to do with activities of an organization in which, Oppenheimer knew, Eltenton was a member.

Shortly thereafter, on a return trip to Berkeley, Oppenheimer volunteered to the chief counter-intelligence officer of the San Francisco area that Eltenton would bear watching. He was pressed to say why.

At this point, in order not to involve his friend Chevalier and, admittedly, partly because he was reluctant to involve himself, Oppenheimer told the security officer, Col. Boris T. Pash, a "cock and bull story." He said Eltenton had made two or three approaches to Berkeley people to get information for the Russians. He declined to give the names of those involved.

Oppenheimer later confessed he was an "idiot" to make up the story; his shame and contrition for the lie haunt the transcript.

Some time later Colonel Lansdale and General Groves pressed Oppenheimer for the whole story. Groves, apparently in no great hurry or dither, did not force Oppenheimer to come clean until **December, 1943. At that time Oppenheimer named Chevalier as the man Eltenton approached.**

Even after this, intelligence officers still labored under the assumption that there had been three contacts. It is by no means clear, however, that Oppenheimer failed to tell Groves there was only one. Conclusive evidence one way or the other is lacking.

Dinner in Paris

AFTER THE WAR, Chevalier visited Oppenheimer for a couple of days at Princeton. Then, in December, 1953, after President Eisenhower had ordered the "blank wall" erected against Oppenheimer—although Oppenheimer did not yet know it—he saw Chevalier again. He and Mrs. Oppenheimer were in Paris, where Chevalier was then living. Mrs. Oppenheimer phoned Chevalier and the three had dinner together.

Next day, they went together to lunch with Andre Malraux. A one-time Communist, Malraux has become a violent anti-Communist and served as the philosophical apologist and confidant of the fiercely anti-Soviet General De Gaulle. Chevalier was working with Malraux on translations.

Friends and foes of Oppenheimer agree that his temporary concealment of what was pretty clearly attempted espionage and his outright lie were inexcusable. But they differ deeply over the implications.

In a pounding and protracted cross-examination, Roger Robb, special counsel for the Board, made Oppenheimer admit some 13 times that he had told a lie about the story; finally he made Oppenheimer say he had told a "tissue of lies." The fact was, he told one lie.

Roger Robb, special counsel to the Gray Board, was a relentless prosecutor of Dr. Oppenheimer, dogged in his cross-examination.

Crucial Evidence

THE AEC majority report denounced Oppenheimer for this matter in the bitterest and most violent terms. Between the lines of Commissioner Murray's separate opinion seems to be a rage that Oppenheimer visited Chevalier in Paris. A responsible newspaperman has reported that one AEC member said the case against Oppenheimer hinged on this point, and that the Commission felt that by his visit Oppenheimer risked being kidnaped by Russian agents.

One security officer at Los Alamos, Capt. (later Major) Peer de Silva, built up for his and his superior officers' edification in 1943 an elaborate argument to demonstrate that Oppenheimer, in volunteering the information about Eltenton, was playing an intricate game, as a Soviet agent, to throw the bloodhounds off the track.

It is interesting to note, however, developments two years later, by which time De Silva had come to know Oppenheimer well. He wrote Oppenheimer the warmest sort of good-by letter, thanking him for "support and encouragement" and saying that Oppenheimer "contributed much to whatever success my office (the security office) has had in performing its mission."

Colonel Pash, who never had dealings with Oppenheimer in any degree after the one interview about the Chevalier affair, stuck to his doubts. In his testimony he indicated that he still questioned Oppenheimer's loyalty.

Moreover, he suggested that he believed Oppenheimer was lying in the hearings when he swore that he knew of only one contact by Eltenton, i. e. the approach by Chevalier to Oppenheimer. The first story Oppenheimer told, that there were three contacts, was more damaging to Oppenheimer if true, Pash argued. Therefore, by his logic, Oppenheimer was now faking a less damaging story.

The AEC majority picked this line of argument in its findings. "It is not clear today," Commissioners Strauss, Eugene M. Zuckert and Joseph Campbell wrote, "whether the account Dr. Oppenheimer gave to Colonel Pash in 1943 concerning the Chevalier incident or the story he told the Gray Board last month is the true version."

From this, the commissioners went on elsewhere in their report to question Oppenheimer's veracity.

On this point, however, it is interesting to note that the most diligent investigation of Colonel Pash and others never discovered that there were three contacts or even two. There was, as far as is known, only one—the one which Oppenheimer himself first revealed to Groves in 1943.

Lansdale Changed His Mind

THE testimony of Groves and his top intelligence officer, Landsdale, put a different light on the affair. Both vouched for Oppenheimer's loyalty.

Lansdale admitted that at first he was inclined to Pash's view and distrusted Oppenheimer deeply. But he emphasized to the Gray Board that it was Oppenheimer who took the initiative to tell, rather than hide, the Eltenton attempt. He said he believed Oppenheimer never lied about any other incident.

Groves' testimony made clear he felt it was very wrong of Oppenheimer to delay in telling the story, to refuse at first to name Chevalier and to lie about the incident. But he indicated that the important fact was that Oppenheimer had done the essential thing in disclosing the source of the danger to security. Regardless of whether there were three contacts or only one, "I felt I had gotten what I needed to get out of that."

Oppenheimer's own story, whether right or wrong, has been consistent since 1946, when he was questioned by the FBI. It remained so under all of Robb's cross-examination.

It was that he felt Chevalier was ignorant of what Eltenton was really trying to get him to do, and was in fact innocent of wrong intent. Accordingly, Oppenheimer tried to avoid implicating him in his interview with Pash and tried to protect him even later in not revealing his name to Lansdale or Groves until the bitter end.

Moreover, still feeling that Chevalier was innocent, he saw no reason why he should not have visited him in Paris last year. "I still think of Chevalier as a friend," said Oppenheimer.

Unused to Snitching

IN HIS dissenting opinion, AEC member Henry D. Smyth remarks that "if one starts with the assumption that Oppenheimer is disloyal," the incidents brought up in the hearings may arouse suspicion.

One could also start with the assumption that Oppenheimer was not disloyal, and come out with a perfectly logical picture of the Chevalier affair. For someone deeply immersed in the Communist movement, the process of coming out of it is not easy. In going to Pash with the news of Eltenton's espionage attempts, Oppenheimer was confronted with the prospect of having to snitch on a friend for the first time.

He made a botch of the job. One might guess that he was so naive that he thought the mere mentioning of Eltenton would be enough. Surprised that he was asked for more information, he began to improvise. It was a bad improvisation.

In describing Oppenheimer's attempt to conceal Chevalier's involvement, General Groves said he thought Oppenheimer was acting in "the typical American schoolboy attitude that there is something wicked about telling on a friend."

He went on: "I do know this: That he was doing what he thought was essential, which was to disclose to me the dangers of this particular attempt (of a potential spy) to enter the project."

Here is George Kennan, in somewhat subtler language, on the same point: "I also think it quite possible for a person to be himself profoundly honest and yet to have associates and friends who may be misguided and misled and for whom either at the time or in retrospect he may feel intensely sorry and concerned. I think most of us have had the experience of having known people at one time in our lives of whom we felt that way."

Kennan again, on Oppenheimer's later association with and visit to Chevalier:

"I don't like to think that people in senior capacity in Government should not be permitted or conceded maturity of judgment to know when they can see such a person or when they can't . . . I would always like to have felt that my superiors in Government had enough confidence in me to let me handle the problem according to my own best conscience."

Cleared by AEC in '47

EXCEPT for the 1950 and 1953 visits with Chevalier and for some unclear data connected with the "nit-picking" incidents, almost all of the significant derogatory information against Oppenheimer that was introduced at the hearing had been brought to the attention of the AEC in 1947. The five members took pains to study the data with great thoroughness. They got opinions from Groves, Secretary of War Patterson, Bush and famed scientist James B. Conant. All were favorable to Oppenheimer.

The AEC cleared Oppenheimer in August, 1947. One member who cleared him, Strauss, now the AEC chairman, was and is president of the Institute for Advanced Studies. He was responsible for Oppenheimer's appointment as director of that distinguished academy two months later, in October, 1947.

In the Gray Board hearings, five former members of the AEC testified in his behalf, including the two previous chairmen, David Lilienthal and Gordon Dean. So did 10 former and present members of the General Advisory Committee, including the present chairman, Rabi. So did Norris Bradbury, Oppenheimer's successor as head of Los Alamos.

Gen. Frederick Osborn told how much tougher Oppenheimer had been in his outlook toward the Russians than Osborn was himself during the futile negotiations for international atomic control. Rabi even testified—he confessed to being surprised at it—to something he said he had seen in recent years, "a certain tendency of Dr. Oppenheimer to be inclined toward a preventive war," not that he went all the way, "but talking and thinking about it quite seriously."

The two outstanding public leaders of American science, Bush and Conant, appeared for him. So did John J. McCloy, in effect the civilian head of the Manhattan Project. Lansdale testified to his security, then and now. Groves vouched for his loyalty and said he had never had reason to regret clearing him for the top post in the A-bomb work (although he conceded he would probably not clear him under the standards of the AEC regulations).

Lansdale and Groves were the two men who had studied the security problem raised by Oppenheimer during the war years with the greatest intensity and who had the greatest responsibility in the matter.

No Hint of a Leak

SAVE for Pash and Borden, no one alleged that Oppenheimer was disloyal. There was not one assertion, even by Pash, that at any time Oppenheimer was indiscreet. Oppenheimer has been under the most intense surveillance for 11 years, his phones tapped, his movements watched, his mail observed. There was no evidence or charge that he had spilled even a semi-demi-secret.

He was found guilty by the AEC majority of disturbing associations, continuing too long. The record shows his associations with dubious types after he

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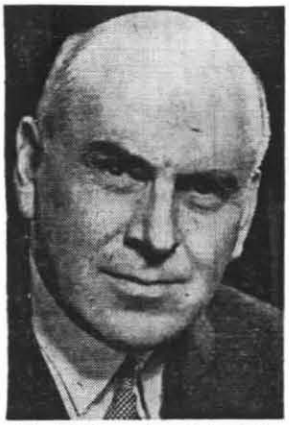

John J. McCloy, who as Assistant Secretary of War was, in effect, civilian head of the Manhattan Project, which fashioned the A-bomb, was a witness for ***Oppenheimer.***

Members of the special Personnel Security Board which heard the testimony contained in the transcript. Left to right, Dr. Ward V. Evans, professor of chemistry at Loyola University, Chicago (who voted for Oppenheimer); Gordon Gray, former Secretary of the Army, and Thomas A. Morgan, president of the Sperry Corp.

left Los Alamos in 1945 were surprisingly infrequent—one had to search to find them. The record shows that his constant and close associates were scientists and statesmen, the ones who came to testify for him.

There have been persons who were far more deeply involved in the Communist conspiracy, as agents, spies and traitors, than Oppenheimer. By acts of contrition and confession, they have won back the favor of many Americans who consider that they have expiated their sins and are now fit to serve and advise their Government.

The hearings went deeply into the question of whether this might not also be possible and proper for Oppenheimer. Witness after witness testified to the probity of his actions since his lie in 1943, and particularly to the enormous contribution he made to his country. If the AEC majority was not impressed, the Gray Board was. The Nation owes Oppenheimer in particular, it wrote, "a great debt of gratitude for loyal and magnificent service."

Dr. Rabi perhaps put it best:

"We had an A-bomb and a whole series of it . . . and what more do you want, mermaids?

"This is just a tremendous achievement. If the end of the road is this kind of hearing, which can't help but be humiliating, I thought it was a pretty bad show. I still think so."

The transcript, as tense as it is thick, shows vividly who some of Oppenheimer's enemies were, and why they were out to topple him.

There was Borden, with a wild-eyed conviction which led him to write unprovable impossibilities which were—to use precisely that abused word—incredible.

There was Teller, shown as a disputatious fellow, removed from war work at Los Alamos during the war, mortally offended because his baby, the thermonuclear weapon, had not been blessed by Oppenheimer. Even he, however, did not question Oppenheimer's loyalty or security—only his judgment.

Dr. Luis Alvarez was one of the Berkeley (University of California) group of scientists which pressed for a "crash program" to build the H-bomb while Oppenheimer was opposing it.

There were the scientists closely connected with the work at Berkeley. In the background, the record hints that the great E. O. Lawrence was miffed. Others—Alvarez, Griggs, W. M. Latimer, Kenneth Pitzer — may have felt that Oppenheimer grew too big for his britches after he left them back in 1943.

And throughout, as mentioned before, there were those in the Air Force, powerful figures, who loathed his ideas and feared his effectiveness in plugging them.

Phenomena Unexplained

ALL of these forces taken together, however, do not account for some of the startling phenomena that are part of the Oppenheimer case. The record shows they existed; it does not explain why or how. It raise the questions but does not answer them.

Why, for example, was the huge transcript released at all, and suddenly, on a few hours' notice to Oppenheimer's lawyers? Gordon Gray had promised that the AEC would not take the initiative in making it public, yet the AEC did, after the Gray Report appeared but before the decision of the AEC itself.

For months, wild horses—more explicitly, the press and public—had failed to drag out of the AEC even meager details about our atomic affairs. Suddenly the transcript, an Operation Candor a dozen times more detailed than anyone dreamed of getting, was made public, with a staggering amount of information on thermonucleonics.

Why was there the savagery of language by the four AEC members who condemned Oppenheimer, the bitterness of phrase, the violence of denunciation? The Gray Report at least acknowledged Oppenheimer's loyalty and discretion and praised his gigantic contribution to his country. It expressed regret for the recommendation it found necessary. The majority AEC finding threw the man no crumb. It even left open the question of loyalty despite 900 conclusive pages about it.

Why was the hearing not an inquiry, according to the explicit rules of the AEC, but a trial, which the rules forbid? After the second day's testimony, it is clear to the reader of the transcript that Robb is a relentless prosecutor, out to get Oppenheimer, out to get into the record evidence that will destroy and degrade him.

Oppenheimer found himself in the worst of two possible worlds. An administrative inquiry would have lacked the adversary nature of a trial; a trial would have assured him and his witnesses and his counsel access to the documents used by the prosecution. Instead, Oppenheimer was repeatedly trapped by being called on to recount a conversation in 1943 or 1946 and, when he had finished, being confronted with a transcript of what he said then.

One little passage gives a flavor of the even-handedness of the Board's special counsel's examination. Attorney Robb is questioning the great physicist Hans Bethe, closely connected with the work of the AEC and against whom no question of loyalty or security has been raised. Bethe had testified strongly in Oppenheimer's behalf.

"Q. Doctor, how many divisions were there at Los Alamos?

"A. It changed somewhat in the course of time. As far as I could count the other day, there were seven, but there may have been eight or nine at some time.

"Q. Which division was Klaus Fuchs in?

"A. He was in my division, which was the Theoretical Division.

"Q. Thank you. That is all."

Protection or Punishment?

THERE is another, much deeper, question raised by the record and the decisions. It has to do with the nature and purpose of the security program. Is it meant to be preventive or punitive, protective or retaliatory?

In the light of a record of discretion and loyalty since 1943, did those who found against Oppenheimer remove his access to security documents because they were really afraid he might disclose secrets in the future, either by design or by accident?

An if they were really afraid, did their ruling solve the problem? Oppenheimer still has perhaps as much security information in his head as any man alive. Is he safe, running loose at Princeton?

Or did those who ruled against him do so because they felt he should be punished?

Were Oppenheimer's well scrutinized actions for more than a decade being examined for what presumptive indications they could give on what his acts would be from now on? Or were his associations for the decade preceding 1943 being examined to determine what retribution they merited?

From 'Oppie's' Book

BY one of those ironic coincidences that history now and then turns up, a little book written by Oppenheimer himself was published on the same day the transcript was released, and offered for sale at the same price.

It is a collection of the six Reith lectures Oppenheimer delivered in 1953 over the British Broadcasting System, telling the story of the new physics of the last 30 or 40 years. It is the story of the monumental "breakthrough" of discovery and understanding that succeeded Newton's picture of the world. It is a discussion of the field of knowledge, from sub-atomic particle to cosmos, which is so intimately associated with Oppenheimer and to which he has so richly contributed.

The book, "Science and the Common Understanding," is technically difficult and for the most part hard going. But there are flashes in it that tell much about the man who wrote it, and his outlook. There is a quotation in it that may serve as a final word. It is from a letter written by a great man to a young friend:

"Science can never be retrograde; what is once acquired of real knowledge can never be lost. To preserve the freedom of the human mind then and freedom of the press, every spirit should be ready to devote himself to martyrdom; for as long as we may think as we will, and speak as we think, the condition of man will proceed in improvement.

"The generation which is bowing off the stage has deserved well of mankind for the struggles it has made, and for having arrested that course of despotism which had overwhelmed the world for thousands and thousands of years. If there seems to be danger that the ground they have gained will be lost again, that danger comes from the generation your contemporary.

"But that the enthusiasm which characterizes youth should lift its parracide hands against freedom and science would be such a monstrous phaenomenon as I cannot place among possible things in this age and country."

Oppenheimer was quoting from a letter by Thomas Jefferson.

Dr. James B. Conant, one of the two outstanding leaders of American science, testified for Oppenheimer, as did the other, Dr. Bush.

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2 H-Bomb Leaders at Odds Over Oppenheimer Attitude

Bradbury Testimony Disputes Teller View That Nuclear Physicist Held Back Program by Lack of Enthusiasm

By ELIE ABEL
Special to The New York Times.

WASHINGTON, June 16—The Atomic Energy Commission, in seeking to determine whether Dr. J. Robert Oppenheimer held back the development of the hydrogen bomb by his lack of enthusiasm, pondered today a conflict of testimony between the two scientists chiefly responsible for the bomb's success.

Dr. Norris Edwin Bradbury, director of the Los Alamos Scientific Laboratory in which the hydrogen bomb research was centered, contradicted the testimony of Dr. Edward Teller that the weapon might have been developed four years earlier if Dr. Oppenheimer and others had given the project their "moral support."

Excerpts from testimony at the hearing on Pages 20 and 21.

Dr. Teller, Professor of Physics at the University of California, and Dr. Bradbury were among dozens of scientists and others who testified in hearings earlier this year by a special Personnel Security Board of the Atomic Energy Commission on the Oppenheimer case.

The board found Dr. Oppenheimer both "loyal" and "discreet" but withheld his security clearance, which had been suspended by order of President Eisenhower months ago.

Dr. Oppenheimer appealed his case directly to the commission.

Even if President Truman had issued his go-ahead order in 1945, rather than in 1950, Dr. Bradbury testified, "I cannot see how we could have reached our present objective in a more rapid fashion."

The physicist, who succeeded Dr. Oppenheimer as director of the Los Alamos laboratory at the end of World War II, rejected the contention that there had been any kind of delay in the program.

Before a successful attack on

Continued on Page 22, Column 4

Two H-Bomb Leaders at Odds Over Attitude of Oppenheimer

Continued From Page 1

the hydrogen project could be launched, Dr. Bradbury said, it had been necessary to learn a great deal more than anyone knew in 1945 about the nature of fission bombs. This knowledge, he asserted, was essential to fruitful work in the thermonuclear (hydrogen) field.

Another obstacle cited by Dr. Bradbury was that until 1948 or 1949 the electronic computer had not been developed to the stage where it could handle problems of the complexity encountered in computing thermonuclear reactions.

Tempers were fraying as the Atomic Energy Commission prepared today to sit in judgment on the Oppenheimer case. The five commissioners will review the 2-1 finding of the special Personnel Security Board.

Officials of the A. E. C., which decided yesterday to make public the 991-page printed transcript of the Oppenheimer hearings, were angered at the prior release by the scientist's lawyers of their brief to the commissioners. They accused the Oppenheimer attorneys, Lloyd K. Garrison and John W. Davis, of "jumping the gun" in an attempt to sway public opinion.

Summary Reported Lost

On Capitol Hill, meanwhile, two members of the Joint Congressional committee on Atomic Energy said the transcript had been made public because a 100-page summary of the hearings had been lost or stolen over the week-end and the commission feared it might have strayed into the hands of a newspaper man. It was found on Sunday night.

When the commission gets down to the merits of the Oppenheimer case it will have to resolve in some way the conflicting accounts of Dr. Teller and Dr. Bradbury. Both are highly regarded by the commission. Both were early advocates of the hydrogen-bomb project.

Each is credited with a major contribution to the success of that project, though Dr. Teller has received more publicity. In their opinions of Dr. Oppenheimer, as on other matters, the two scientists were far apart.

Dr. Teller, a native of Budapest who came to the United States in 1953, said he considered Dr. Oppenheimer a loyal American but that in view of his attitude toward the hydrogen bomb project he believed the Government would be "wiser not to grant clearance."

He contended that if Dr. Oppenheimer at the end of World War II had given moral support to the thermonuclear project, the theoretical posibilities of which had been glimpsed earlier, the weapon might have been ready four years earlier.

As chairman of the General Advisory Committee to the Atomic Energy Commission, Dr. Oppenheimer gave "not the slightest" help in the recruitment of scientists for the bomb project even after President Truman had decided to go ahead with it, Dr. Teller testified.

On all these points Dr. Bradbury took the opposite position. He testified that Dr. Oppenheimer was "completely and unequivocally loyal to the best interests of the country," no less so in the postwar years than during the period up to 1945, when the Soviet Union and the United States were allies.

Defends Scientist's Loyalty

"I think that while loyalty is a very difficult thing to demonstrate in an objective fashion," Dr. Bradbury testified," * * * Dr. Oppenheimer in his direction of the Los Alamos laboratory during the war years did demonstrate such loyalty.

"I do not regard him as a security risk."

Nor did he believe, Dr. Bradbury said, that Dr. Oppenheimer had used his influence to diminish the hydrogen bomb effort. He had found Dr. Oppenheimer and the General Advisory Committee "extremely helpful and cooperative," Dr. Bradbury said.

This was in contrast to Dr. Teller's testimony that the committee had retarded progress and that it might have spent its time fishing without affecting the work.

Dr. Bradbury also testified that he knew of no instance in which Dr. Oppenheimer had persuaded any other scientist not to work on the hydrogen project, and that he had never "known him to act in a way other than was a help to the laboratory."

When the Los Alamos research team, of which Dr. Teller was a leading member, came up in 1951 with a new approach to the problem that suddenly made feasible the hydrogen explosion of which many scientists had been dubious, Dr. Oppenheimer and the General Advisory Committee became enthusiastic about the idea, Dr. Bradbury testified.

"Their recommendations to the [Atomic Energy] Commission were at least as enthusiastic as ours, and actually went somewhat beyond, in terms of support, what we had originally drafted," he said.

Dr. Teller himself was at pains in answering a question by the board to make clear that this new approach had been developed at Los Alamos, rather than at the laboratory in Livermore, Calif., where he is now working.

Decries 'Exaggeration'

"Livermore is a very new laboratory and I think it is doing a very nice job," Dr. Teller said, "but published reports about its importance have been grossly and embarrassingly exaggerated."

Among the thirty-eight other witnesses heard by the board, Dr. Vannevar Bush, president of the Carnegie Institution and wartime head of the Office of Scientific Research and Development, disclosed that he had tried to persuade Dean Acheson, then Secretary of State, that the first hydrogen bomb test should be postponed at least until after the Presidential election in November of 1952.

The test, however, was carried out four days before the election. It was not announced by the Atomic Energy Commission until Nov. 16.

"I felt that it was utterly improper—and I still think so—for that test to be put on just before the election, to confront an incoming President with an accomplished test for which he would carry the full responsibility thereafter," Dr. Bush testified. "For that test marked our entry into a very disagreeable type of world."

A second reason, Dr. Bush said, was his strong feeling that the test would rule out the only type of international agreement he believed possible at that time, one to prohibit nuclear tests by either Russia or the United States.

Calls Test 'Grave Error'

Such an agreement would have been self-policing, Dr. Bush said, because its violation on one side could quickly be detected on the other. The decision to go ahead with the Nov. 1 test was a "grave error," he asserted, adding:

"I think history will show that was a turning point when we entered into the grim world that we are entering right now, that those who pushed that thing through to a conclusion without making that attempt (at agreement) have a great deal to answer for," Dr. Bush said.

The opinion that "more probably than not J. Robert Oppenheimer is an agent of the Soviet Union" was given to the board by William L. Borden, staff director of the Congressional Joint Committee on Atomic Energy from 1949 until mid 1953.

The conclusion was set forth in a letter to J. Edgar Hoover, Director of the Federal Bureau of Investigation, dated Nov. 7, 1953. Mr. Borden, now assistant to the manager of the Atomic Power Division of Westinghouse Electric Corporation, testified that his view had not changed since then.

Counsel for Dr. Oppenheimer did not cross-examine him, holding that the letter expressed personal conclusions by Mr. Borden, rather than evidence. Mr. Gray, on behalf of the board, also pointed out that the panel "has no evidence before it that Dr. Oppenheimer volunteered espionage information to the Soviets or complied with a request for such information" or that "he has been functioning as an espionage agent," as suggested by Mr. Borden.

Mr. Borden said in the letter that his conclusions were based on a study of information given to the Atomic Energy Commission by the Federal Bureau of Investigation and "a mass of additional data assembled from numerous other sources."

He cited "evidence indicating that as of April, 1942" Dr. Oppenheimer was contributing money to the Communist party; that his wife and younger brother were Communists; that he had "at least one Communist mistress"; that he belonged only to Communist organizations aside from professional affiliations, and that he was in frequent contact with Soviet espionage agents.

Other evidence, according to Mr. Borden, indicated that Dr. Oppenheimer afterward "repeatedly gave false information" to authorities concerning the period from 1939 to April of 1942.

In addition, he stated, the evidence indicated that Dr. Oppenheimer was "remarkably instrumental" in influencing suspension of hydrogen bomb development from mid-1946 through January of 1950, and "has worked tirelessly" since then to retard the work.

McCloy Backs Scientist

John J. McCloy, former High Commissioner to Germany and now chairman of the board of the Chase National Bank, said he could not be "too emphatic" in describing Dr. Oppenheimer as a loyal, patriotic citizen who, moreover, was "very sensitive to all aspects of the security of the United States."

He was certain, Mr. McCloy said, that Dr. Oppenheimer was not a security risk. He contended that the term was imprecise, that no human being was "absolutely secure" and that there was a positive aspect to be considered as well as the negative one.

"There is a security risk both ways in this thing," he testified. "It is the affirmative security that I believe we must protect here.

"I would say that even if Dr. Oppenheimer had some connections that were somewhat suspicious or make one fairly uneasy, you have to balance his affirmative aspect against that."