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RESEARCH CLIMATE AND SCIENTIFIC ACCOMPLISHMENT: AN INTERVIEW WITH GERHARD HERZBERG

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Résumé

Étant donné l'accroissement phénomènal de la recherche et des activités de développement dans ce pays, les recherches actuelles sur la recherche ont été infinitésimales. Les études empiriques ont presque entièrement négligé le climat organisationnel dans lequel le scientifique travaille. Vue la mise en question des efforts et de la politique scientifiques au Canada, il paraît opportun de donner quelques pensées d'un scientifique de haute distinction de la nation, le Dr G. Herzberg, l'ancien Directeur de la Division de Physique au Conseil National de Recherche.

In the face of the past phenomenal growth of research and development activities in this country, social science research on research has been infinitesimal. Almost completely neglected by empirical study has been the organizational climate in which a research scientist works. With the present questioning of Canada's science effort and science policy the occasion was felt opportune to present the views of one of the nation's most eminent research scientists, Dr. G. Herzberg, the past Director of the Pure Physics Division of the National Research Council.

To this end the next paragraph introduces Dr. Herzberg to the personnel and management community and the interview comments follow immediately. The views expressed are the spontaneous opinions of Dr. Herzberg and do not necessarily represent official policy or procedure of NRC.

BIOGRAPHICAL SKETCH

Dr. Gerhard Herzberg was born in Hamburg, Germany on 25 December, 1904. He was married in 1929 and has two children. Dr. Herzberg came to the University of Saskatchewan on a Carnegie guest professorship in 1935. He later was made a research professor of physics, a position he held until 1945. From 1945 to 1948 Dr. Herzberg was a professor of spectroscopy at the Yerkes Observatory of the University of Chicago. He returned to Canada in 1948 and was made Principal Research Officer in the Division of Physics at the National Research Council and later Director of the Division of Pure Physics. Following his retirement as Director in 1969 Dr. Herzberg was appointed by NRC as a Distinguished Research Scientist to enable him to continue his research.

Dr. Herzberg has devoted his scientific career to the study of atomic and molecular spectroscopy and has authored over 180 papers in this area. His fundamental studies of the subject have led to widespread applications of spectroscopy in other branches of science. He is the recipient of 18 Honorary Doctorates of Science and has authored textbooks on molecular spectra and molecular structure which have been translated into four languages besides the original. Dr. Herzberg has been honored by national and international scientific bodies. Although a physicist, Dr. Herzberg recently was awarded the Willard Gibbs Metal by the American Chemical Society and the Faraday Medal by the Chemical Society of London for his contributions to fundamental advances in chemistry.

Dr. McCarrey is an organizational psychologist with the Public Service Commission who is currently interested in research on R & D.

NATURE OF SCIENTIFIC ACCOMPLISHMENT

Publications – Quantity and Quality

Dr. McCarrey: Dr. Herzberg it's certainly an honor to meet with you to get some of your views on the nature of scientific accomplishments and the effect that facets of the organizational climate might have upon a scientist's achievement in a lab. I'll specify these further as we go along but for the moment I wonder if we could first look at the nature of scientific accomplishment. What is this? Can it be measured? How can it be measured in an equitable way? Sometimes number of publications is used as a measure of the calibre of a scientist. . . . .

Dr. Herzberg: I have a feeling that quantity of publications has *some* relevance to the quality of a scientific worker. Of course, one can always argue and there are cases in which this may well be true where a person who takes several years to publish one paper may come up with a very good one; but I would say that by and large, with exceptions, an active research worker should be able to produce something worth publishing in a shorter period than that, say in two or three years. The very fact that he can sub-divide the area of his problems in such a way that he must get out something, that he must finish something to a stage where it is publishable rather than wait and wait and come to the point where he runs a risk of being a perfectionist; this is important. I know a person, who, whenever he publishes something, publishes first grade stuff, but it takes some five years to do so. This may be very good in a university environment where the person is teaching as well and earns his bread and butter by teaching and does the scientific work as a sort of an extra thing but for the ordinary worker in a research organization where he does nothing but research my feeling is that there should not be many occasions where he could drag his feet, so to speak, and not publish because he wants to do this experiment and then he wants to do that experiment, so that he can only arrive at the publication stage in several years. In this day and age, this just doesn't work too well in a pure research environment. That is, I don't mean in pure science, I mean science in general, in a lab, with no teaching or other obligations.

Dr. McCarrey: So you would tend to feel that the quantity of publication is an appropriate thing but does not tap everything. It is an appropriate part. . .

Dr. Herzberg: It *is* an appropriate part. Of course there are lots and lots of people who publish trivial stuff, and even good journals cannot avoid sometimes publishing trivial material. Acting as a referee one comes often into a situation where you are sent a paper and you look at it and ask yourself: what has he really contributed? What is it really worth? On the other hand, I would sooner see a person publish somewhat regularly rather than sit and wait five years before he publishes anything because he feels that he may not have hit upon something that is really worthwhile. The risk that what he is thinking of in the meantime, will be published by somebody else, is so great that I think it would be better to have this publication. I admit quantity of publications cannot possibly be the only judgement of a person's quality but it is something that at least we can look at.

Dr. McCarrey: This brings up or leads towards the question of the quality of publication – which is thought to be a relevant thing in many people's minds. It is

relevant, it is important, but then the measurement problem that goes with that is something extremely difficult.

Dr. Herzberg: Very, very difficult.

Dr. McCarrey: The idea that I have heard from some researchers would be, something similar to the awards procedure for post-doctoral fellowships or research grants where a group of specialists in a subject area, review a number of applications and place these in order of potential for future achievement and then this is in effect a quality judgement in that kind of a setting. I was wondering what your feelings would be as to the review of publication quality in a fashion like that?

Dr. Herzberg: As an example let us assume that one man has published something in the last year or two and has done so in very respectable journals even though we don't quite understand the significance of the papers while another person has not published anything. In our grants committees obviously the judgement is in favour of the first one. Now it happens quite frequently, that one of the members of the grants' committee knows the particular field or knows this particular piece of work and he can say: "Well, this is just routine stuff." or he can say: "Well that man has made a contribution." in which case a man is lucky or not so lucky. I, myself, always objected to the idea of adopting a refereeing system for grants because it is much too involved and the referee doesn't know how to interpret the committee's standards and, how are we going to judge the referee?

Dr. McCarrey: Have you ever, explored some king of solution, or some way that it could be done?

Dr. Herzberg: One way that has been suggested but that has also been attacked by others, has been, the Citation Index. Now you might say that a person whose work is being cited frequently has made some kind of an impact in his particular field. But it is very hard to prevent that from becoming unjust because a man may be in a field where not too many people work and yet his work may be significant for one reason or another.

Dr. McCarrey: His work is not of a special quality or it is not similar to others or he's thinking, explaining in a totally different way.

Dr. Herzberg: Of course, it could be that he has hit the nail so squarely on the head that nobody is left with anything to say anymore, that therefore he isn't being explicitly cited in the later work. And so, it is very hard. On the other hand, there is no question what the ideal situation would be if we had some measure of a significance of the person's work. In other words, the significance of the person's work is a far more valid criterion of his usefulness as a scientist than the number of his publications. That is obvious. But how to measure it? That is a really tough proposition. There is the other difficulty that even amongst experts quality isn't always unanimously agreed upon. A man may be a good scientist but opinions can differ vastly about the importance of his – a reasonably good scientist's – work.

Communication, Effectiveness, Interpersonal Relations

Dr. McCarrey: Another aspect might be a scientist's ability to communicate with others on work related matters. Some people feel this is really not a relevant thing. If he has an original idea, if he is creative, if he can operationalize this, do the experiment and verify, etc., this is all that is really required. There are other people who can communicate this information down to graduate schools and to under-graduate schools, but his work is at the frontier of knowledge and provided he can document what he is doing, this is adequate. . .

Dr. Herzberg: Yes, that is the crucial point. If he can't communicate with students, O.K., he should not be at university but he could be at an organization such as this. But there are cases where the person cannot communicate with his colleagues. Then, of course, the situation is serious. I know of many instances, in the history of science where a person has lost the priority of a certain idea, of a discovery, simply because he was unable to communicate it properly to his scientific colleagues. So I do agree that ability to communicate is one, and not the only one, not the most important, but one fairly significant item. Another consideration is the following: let me take a group in a lab. Say, perhaps, 12 to 15 people. Those people in the group that can communicate easily with others are certainly rather helpful to keep the morale up, to stimulate people, to ask questions, and this sort of thing. I can think of one particular person. He is not perhaps the world's best scientist but he goes around, he asks questions, and maybe he doesn't understand it himself. That is why he asks the questions and by doing so he can be of considerable help. I would think it is one of the things that's very important.

Dr. McCarrey: This leads to another aspect, the skill in dealing with other people, especially if the nature of the research problem is such that it is a group problem, rather than an individual scientist project. But if it is necessary to work as a team, possibly as an inter-disciplinary team, this being able to relate, get along with his colleagues, may be an aspect, a part of scientific accomplishment.

Dr. Herzberg: It surely is. You could also put it this way. Nowadays, in experimental research, as soon as people have passed the post-doctoral stage, they will need some technical help. Now if they are able to deal with the technicians, skillfully, they will be much farther ahead than others who cannot, who are never satisfied with the technicians' efforts.

Creativeness, Productiveness, Sense of Perspective

Dr. McCarrey: Another component or factor in scientist accomplishment or output would be his creativity. Here again, it is an impressive, almost over-powering measurement problem, the question of creativity. The point may be made that it is true we do have something which is called "publication and quality" and possibly, this is creativity or evidence of it. However I venture to say that there is more in the creative output of a scientist than what gets into print. Here again, this might be a variable that should be included in our notion of scientist accomplishment; creativity as defined by the breadth, the originality, the uniqueness of a person's ideas, even possibly the ability to ask the

questions in the first place. You mentioned this other chap who was asking people questions, he didn't answer the questions, but he stimulated others.

Dr. Herzberg: Well, I would certainly say that this creativity if you could measure it, would be at the very top of the list of what is most important for a research scientist. Certainly, it is creativity, originality. But how do you measure it?

Dr. McCarrey: I have done some work in attempting to assess creativity with some tests that psychologists have; possibly you have heard of them. But these, in my mind, are not satisfactory. They may differentiate along a very large continuum with research scientists clustering at one part of the very high end of the scale versus another group somewhere else, in which case, this kind of measurement may be satisfactory; but amongst a group of scientists the divergent thinking tests have many problems.

Switching to another problem, Dr. Herzberg, I was wondering what part productivity, in the sense of planning, conducting and evaluating one's efforts, what part this might play in the overall concept of scientific accomplishment on the part of a scientist?

Dr. Herzberg: I would certainly see that as a very important item, it's a question of the sense of perspective and judgement of a scientist towards his own efforts. There is the question of picking the right topic.

Dr. McCarrey: Asking the right question. . . .

Dr. Herzberg: Not only asking the right questions in a particular research problem but what I mean is, there are so many things you can do in a certain field. How to choose a problem that has a finite amount of work needed? It is possible within a year or two or even three years to accomplish and to see that beforehand? "Here is something that I can do with my ability" is most important for a research scientist once he is on his own. But as long as he is a Ph.D. student or a post-doctorate, he will have somebody, some supervisor, who will tell him "This is a good problem, you work on that". But this stage, he will have to pass if he wants to fully develop. Then he goes to the point to pick the right problem. The problem that is feasible, and a problem that is feasible, not only generally speaking, but feasible in connection with the means he has available; both the technical means and his own ability. He should be able to judge his own ability sufficiently to say "Well, this problem I should not touch. It is a good problem, worthwhile doing, but I am not the man for it."

Dr. McCarrey: A judgement, a sense of perspective in terms of the area that he is thinking about in certain sub-sections; a relationship of his ability, his capability at the time.

Dr. Herzberg: I have come across people who were unquestionably brilliant but they lacked this one thing – to choose the right problem and they fall behind. While other people by no means as brilliant; who simply by doing, if one likes, routine work at first, gradually progressing and doing something more interesting, . . . and so on, always realizing this can be done, I will finish it now, I will worry about the future later, . . . etc. have come out very much better.

Dr. McCarrey: I was just wondering, Dr. Herzberg, listening to ourselves talk I have always in my mind: Fine, this is a nice beginning. But is there something more? Is there something in your experience or your thinking about this that you've come across? Here is a part of research achievement, it is crucial. It is very much involved in the nature of a person's work.

Dr. Herzberg: Well, I don't know where you would put this. But one thing a person needs is *drive*. He's got to have ambition. If a person doesn't have the ambition to produce something of his own that is a real contribution to knowledge, I don't think he will make a good start as a scientist. It may be all right as a member of a team but he will not produce the ultimate of what he can do.

Dr. McCarrey: This would be a personality variable.

Dr. Herzberg: Yes. Possibly the ability to set a certain goal. This is what I want to do, possibly not next year, but the one after. This is where I want to be at a certain time. And the question of perseverance. A scientist needs the perseverance and willingness to sacrifice certain pleasures in life and my own feeling is that if a person wants to work from nine to five on five days a week, he won't get very far in science. But it's been done: It depends on the person. Enrico Fermi, I just read the other day, ended his work at 5 o'clock and for the rest of the evening he thought about other things, and he was a great scientist.

Dr. McCarrey: This might be an individual thing that fluctuates from some people to others. Possibly it takes some an awful lot of time, not just to directly focus on the work but to regenerate and to think of it from different view points. And then comes the next day and he accomplishes in one hour what might have taken him six or seven hours the preceding evening.

ORGANIZATIONAL CLIMATE

Freedom, Autonomy

Dr. McCarrey: At this point, it might be appropriate to look at the research milieu in which a scientist works. Can this be conceptualized meaningfully? Can it be measured effectively? Is there a cluster of organizational climate factors which, if present, facilitates accomplishment, and which if absent, precludes or impedes accomplishment? Obviously adequate financial support is a *sine qua non* but are there other less tangible yet equally significant factors in a research climate? Would there be things in the organizational environment which would be extremely important in terms of your being able to do good work?

Dr. Herzberg: Well, of course it is a little difficult off hand to make a significant remark on that. One thing that has always struck me as important for scientific work at a reasonably high level is that the particular individual needs freedom in more than one respect. First of all, he needs freedom to do what he wants to do, assuming that he is a really capable person. Then, of all the people around, he surely is the best one to judge whether or not he can do a certain thing. Of course, the moment that he comes to

mission oriented laboratories, this is different. But in a laboratory where people are working on fundamental problems without any pressing specific missions that they have to look at, they must be the ones to decide. And I remember, with some pleasure, an occasion that presented itself one or two years ago, when I had in this office, a visit from a group of people including the President of the Jugoslavia Academy of Science and they wanted me to explain what the Director of the Division of Pure Physics does. I started by saying "I am called the director of this division but I don't direct." This made an immense impression on the President of the Jugoslavia Academy. It pleased me because I do think that the moment you set up an organization where the director directs everyone "You do this, you do that and this will be the problem you will be doing", even if after that, you leave the freedom of how to do it to the scientists you are not on the right track in my opinion. He's got to let the individual find his own problem, if the individual is capable of doing it. Of course he has to judge that. He may make suggestions, strong suggestions, but still even when we have post-doctorate fellows coming in, I don't come in and say "You will be doing this problem". I tell him "There is this problem, there is that problem, that needs doing. You might talk to other people and then choose whichever problem you would like to work on"; (provided he doesn't come with a problem of his own, so much the better!) So I do feel that this is the first kind of freedom a scientist needs in the fundamental field. A second kind of freedom is a more prosaic one, in that he needs freedom in the sense not only that he can do what he wants to do, but also to have the facilities to do what he wants to do, without all sorts of restrictions, bureaucratic rules and this sort of thing, e.g., nobody has to say "You have to be here at 8:30 in the morning and we quit at 5:00." If he wants to appear at 5 P.M. and work till midnight, it is perfectly all right with us. In other words there should be no such rules that you are apt to have in a government organization to restrict the freedom of movement and the freedom of procurement. If he wants a certain piece of apparatus, there should not be a long session with some administrator, provided it is not a major acquisition.

Dr. McCarrey: In your opinion, then, in a very basic, divergent setting, to have the freedom to take problems as they come to you, wherever they may lead, is of paramount importance.

Dr. Herzberg: I think so. I think perhaps that I have not yet emphasized the freedom to change your project. I mean, for example, a person has been working on a problem, isn't getting very far, very fast, and suddenly has an idea about something and he comes to me or his immediate supervisor – "I have a very much better problem than the one that I am working on, can I change to it? " If the supervisor puts all sorts of blocks in his way, it is not the right thing. With us he can change in the middle of the year provided, of course, that it doesn't require another ten thousand dollars on our budget. That kind of freedom is also important. And it makes a happy man of a disgruntled one. Of course he will have to do some explaining, why is he leaving one to move to the other? We have not much formality on that, hardly any.

Supervisory style, Individual differences, Consultation

Dr. McCarrey: The style or the ways that a supervisor directs people, the word 'direct' being used in the sense that you mean, would you see this relating to the scientist

productivity of that group of people or would you see that being a liability in the efforts of a supervisor who doesn't do any kind of directing or motivating. Would you see a curvilinear function in a sense or. . .?

Dr. Herzberg: Well, I think that it may be quite true that there is a relationship. Rather than direct of course, the supervisor should be able to ask questions; "Why did you do it this way? Why didn't you do it that way? " He should be sufficiently familiar with the subject, that he can ask fairly significant questions and he should be able to help the person in overcoming some of the difficulties. There should be flexibility. Of course it is not always true. It all depends on the personality of the people involved. Some people like to be directed; some people like to be pushed and others object to it.

Everyone has, I am sure made mistakes, you can't help it. But the mistake that some of our committees and science administrators, are making, is to think that you could make the system so that there is a 100% yield. There is no such thing. About the best you can hope is that a small percentage of the research work you are doing really ends up by being truly significant. I don't know what percentage it is but it is a small percentage. By this time, I have published quite a lot of papers but none of these papers are equally important. There are some that are just routine and there are others where I feel I really accomplished something. When you really look over it, you might say, if you can have a 10% or 20% yield of *really* good stuff, you're doing pretty well. In a group of people, you can't expect that everyone will be a winner; you can't devise a method by means of which you can make the work of everyone fully significant. You have to expect that some people won't do as well as others. Some people will just go along and may be useful in some ways and if you want the top results, you have to put up with a lot of poor ones.

Dr. McCarrey: This goes back into your framework of freedom in which the administrator can operate knowing in advance these factors.

Another climate factor might be scientist consultation on policy decisions. I have talked to some researchers who are not interested at all. They feel that policy matters belong to administration. They just like to do 'their own thing' and it is the job of other people to actually make this easy for them. Other researchers have mentioned that while those policy decisions, although they may not affect the chap at the bench the next day, by and large, *are* going to influence his area, possibly his means towards goals, and so on. Therefore he should be brought into, at least be heard in terms of these decisions. I was wondering what would be your feeling, as you are uniquely able to comment on this having been both involved in policy decision making and as a bench scientist.

Dr. Herzberg: There is surely no question that consultation is a good thing as far down as possible. If a scientist has had no influence on a policy decision that affects him I think he would resent it. But on the other hand, it seems to me some decisions of policy have to be made without all the business of asking everybody; you can't always ask everybody. I mean, if you did, the director would spend all his time on these matters and he would no longer be a scientist. Then, you would have lost what you really need most, namely that the administrative person is a scientist.

Formality, Reporting writing, Competition vs. Cooperation

Dr. McCarrey: I was wondering Dr. Herzberg how you would see the level of formality in an active lab. . .

Dr. Herzberg: I think it is quite important that there is no formality. For example, if a man in this group wants to discuss something with a man in that group, there should be no barriers between the two, such that they would first have to go to the Head of the Section asking – "can I go over there" and he goes down from there. Or even worse, first go to the director and then from the director down there. All that, of course, should not happen in a real research organization.

Dr. McCarrey: Again, it is a question of something that is very fundamental to research; the idea of spontaneity, its here and now that a person wants feedback, not a week from now.

Dr. Herzberg: That is correct.

Dr. McCarrey: What are your feelings on report writing? Does emphasis on reporting relate to accomplishment?

Dr. Herzberg: Well, of course, there are lots of opinions on that problem but our method is such that every research worker is expected to write a very brief internal report, (one paragraph if there is nothing more to report, maybe two paragraphs) for what we call a quarterly report. My own feeling has always been that this could accomplish two ends. First of all, it makes it necessary for the individual to sit down at least once every quarter and say "Now, what have I really done, have I gotten anywhere? " And that is good for one's soul. Not everybody appreciates it though. The other is that he should write it down in such a way that the man in the other group or the man next to him can understand what he is talking about so that this increases communication.

Dr. McCarrey: About the question of competition. If there is a level of competition that is productive, that is not destructive in the sense that it harms people but actually raises the levels of output, is this something that you have noticed? Or is it better to orient a group toward maximum cooperation?

Dr. Herzberg: Well, I don't think its good to have real competition in a group. I have seen once or twice, occasions of that sort where people within the same lab were trying to do the same thing because it would seem to them an important problem. Well, I mean that doesn't seem to be the right thing to do and some kind of compromise must be found, either it should be combined and they do it together or one must do something else. Those are tricky situations that have to be resolved. I would feel that that kind of compeition is not good for a lab. But if you mean competition in the sense that one person wants to get out a paper sooner than the other, or this sort of thing, by all means, if they have the ambition to get there before the other chap in the matter of accomplishing something, I think it is a good thing.

Dr. McCarrey: It's beneficial as long as it doesn't get to the point that people are forsaking good work for speed.

Primary reference group, – local vs. cosmopolitan.

Dr. McCarrey: There is another issue that is interesting; namely the primary orientation or lab philosophy on the part of the scientist – the local versus cosmopolitan orientation – and the question would be: does the scientist's primary orientation relate to or affect his accomplishment? To elaborate, if a scientist's primary reference group is the international science community, the organization in a sense merely constitutes, merely gives him the facilities for doing what he would like to do and he could just as easily do it in one place or another and so on. . . On the other hand, there are individuals who arc very much concerned about doing their work *here*. Contributing towards the goals *here* and they would not be happy in contributing towards different goals in another institution. Would you feel that this kind of polarity would influence the research accomplishment in the setting if one or other of these philosophies was predominant? I realize this is an awfully difficult thing to talk about.

Dr. Herzberg: Yes, it is a difficult question. Let me put it this way. I believe, that in the past twenty years, NRC has established a certain reputation in the scientific field, internationally. Therefore, we, as members of NRC take a certain degree of pride in NRC, and it would be on that score alone, just so much harder to move us from NRC to another place. So, I don't know if that is part of your question.

Dr. McCarrey: Yes, this would be a part of it. I would be a question of a scientist's obligation or loyalty (those are really not the right words); some individuals seem to have a predominant first loyalty to science. This is where they would like their recognition, from their colleagues, university and other establishments as well as possibly the setting where they are. Others predominantly are interested in that setting and, secondarily, in the rewards and recognition from other sources. Would you feel those two would influence the accomplishment of the scientist?

Dr. Herzberg: It could easily do that, but I would say that in the pure research field, of course, the international science community, is the main orientation of each scientist. If he is doing something, he does it in the first place because he wants recognition from the international community of scientists in his field. And in the second instance, he might do it because he feels NRC, for example, is a good thing and since I am in NRC, here is something that can help the reputation of NRC.

Dr. McCarrey: Listening to you, Dr. Herzberg I would see that the question isn't properly phrased in the sense that you are looking for both the esteem from others here and very much also the enhancement of the status of NRC before the scientific community at large as well.

Dr. Herzberg: Exactly! The status of NRC in the scientific community we feel has been very very high. This is on account of the work in the labs and that is what we are doing for Canada, we think.

Dual Ladder, Administration of research

Dr. McCarrey: There is another interesting question in the personnel literature concerning research work and development work; the question of the dual ladder of advancement within a setting. Would it be a good idea for scientists to be able to advance in remuneration without having to take on administrative obligations?

Dr. Herzberg: I think this is absulutely true. There are people who are not good administrators and yet may be excellent scientists. It is probably recognized by some industrial research laboratoires that the moment you make a good scientist into an administrator, you might have done a great deal of damage both to the scientist and to your organization. He just may not be suitable!

Dr. McCarrey: That, of course, brings up the terrific problem of the administration of research. A man reaches a certain level, possibly in salary or position or whatever and, for many reasons, the question of administration comes up. And, as you have mentioned, is a good scientist necessarily a good administrator of research? Should, on the other hand, the administration of research be handled by people who are *not* scientists? Specialists in business administration, human relations, or market and cost analysis? This again is another problem.

Dr. Herzberg: Quite, but after all, there is a pyramid and not everyone will come to the top. And there is a choice of people who could be put into administrative positions. But I believe very strongly in having *scientists* as administrators. It might sometimes work to have a non-scientist as an administrator but as a whole, I think scientists want to be administered by scientists simply because they feel they are better understood by them. Because the gap between the scientists and the non-scientists is so great that any troubles a scientist might have with a non-scientist will always be brought back to that difficulty.

Dr. McCarrey: Would you feel that the technical competency in terms of area, is very important? In other words, the scientist is a skilled researcher but in a different area. By that he is forced to delegate decisions of technical matters to the people who are directly involved. It that a good thing or. . . ?

Dr. Herzberg: Well, I think it would depend greatly on the man. On the whole I don't think there is a very strong objection to that, and not by far as strong as against non-scientists being administrators. I think the scientist likes to feel that in matters of promotion and things of that sort, or matters of procurement, that the person, his supervisor, is a person who feels for him in this sense. Whether he is a chemist or biologist, he will always have the same kind of feeling: "Well, this man wants to get on with his job; why don't we make sure that he gets what he needs? " Or he is up for promotion: this man will look at his record and publications. He may not be able to judge the quality, of course; he needs some other evidence for that; but still, I feel that from the point of view of the scientists, it would be far better to have, if he is a physicist, a chemist, or if he is a chemist, have a physicist, than to have a non-scientist. The main point is, that you should never have an administrator tell a scientist in technical matters what he should do. I would think that a scientist on the bench would feel happier, if the

person in charge is a scientist himself and has some feeling for the scientist's troubles and needs and aspirations.

Dr. McCarrey: I suppose the question as to whether the scientist is actively working in the field, or has left the field of science, to take on administration would then be secondary?

Dr. Herzberg: It is secondary; but what concerns me is that the people who have been invited before the Special Senate Committee on Science Policy were almost all non-scientifically – active science administrators who had left the bench many years prior. This is dangerous because they may fail to recognize something. Even as a former scientist, as an administrator they cannot really appreciate what is reasonable, what is feasible, scientifically in the current 'state of the art'. They have lost their position on the frontiers of knowledge and possibly also their feeling for the psychology of scientists. A scientist does not know what he will be doing specifically two months from today, the forecasting has to be most flexible. There must be goals yes, but flexible routes to them.

Dr. McCarrey: Dr. Herzberg, your comments and opinions on this issue are both informative and interesting and will, no doubt, be of considerable value to our readers. I very much appreciated the opportunity to chat with you and I thank you for your time.