Dr. Howlett agreed to accept the responsibility of consultant. David Mann took on the job of building the equipment and is not yet finished with a second calibration unit.

We intend to calibrate the first group of cameras and then send them to the Bureau of Standards for a check on our calibration before we release them. After we gain confidence in ourselves and Dr. Gardner has confidence in our methods, we hope that the various governmental agencies that specify a need for topographic mapping camera certificates will follow the custom in England, which Mr. Odle has mentioned, wherein the manufacturer's certificate is accepted as adequate certification. Actually, there wouldn't be much sense in our having this equipment if the various departments of the government continue to specify that only the Bureau of Standards can give the certificate. Even in that case our equipment would help because we could eliminate one step. But, except for periodic checking between the Bureau and ourselves, I hope to see our certificate accepted fully by the departments.

The camera calibration laboratory will first be used to calibrate our own cameras as they come through production. This T-11 camera production has put quite an onus on us in that respect. Then, of course, we will recalibrate and recheck our own cameras as may be necessary and as called for by the various governmental specifications with regard to mapping projects. Thirdly and ultimately, we expect to be able to calibrate cameras which have the constructional features which will permit them to be used on our equipment, whether made by us or by anybody else.

DISCUSSION

Mr. A. H. Katz [Chief, Photo Physics Branch, Photographic Laboratory, Wright Field, Dayton, Ohio]: I assume that when Dr. Howlett invited me to sit up here he wanted disagreement. So I found some points to disagree on.

I want to define my own position for the group. We are exactly half way between the lens designers, manufacturers and research people, and the ultimate consumer. Since the gap is so great between the lens designer and the ultimate consumer, this half-way point is a considerable distance from each end.

My experience with distortion is very limited; so I will not say much about it.

Dr. Howlett and everybody else who has proposed a resolution test have found that in the past we have disregarded them all uniformly, without prejudice. I must say that this is based on some very good and sound experience, which I should like to describe. First, our function has been what Dr. Gardner has described, one of not telling a manufacturer what particular aberration to correct or to decide if this lens is better than that lens. It is not as simple as that because we have in this decision the matter of significant differences to establish. It is not enough to establish if lens *A* resolves 50 lines per millimeter, while lens *B* is resolving 1,000 lines per millimeter. We will pick lens *B*, when out in service we get 30 lines, but most frequently 15 or 12 and lower.

While the important function in the resolution test is to grade lenses serially, it is perhaps not important to establish numbers. But it is important to establish significant differences which may be qualitative.

We have file cabinets of data on the high-contrast, parallel-line target that we have been using. I am willing to commit myself to a statement that we have not yet made a poor decision based on that data. That is the really important point; the rest is academic, but not uninteresting. It is relevant, but academic. If our function is to pick good lenses, we have done it. When we tested a lens and it gave us a high number on our test, that lens has made good pictures. The material Dr. Macdonald presented this morning might lead you to infer that it is a coincidence. If so, it has been a coincidence without exception to date, and I am perfectly happy to have that condition continue.

I am in agreement with Dr. Gardner on that point but I find myself strangely in disagreement on the matter of what emulsion to use. If we pick lenses for the consumer, then the differences between lenses have to be significant. If the consumer uses a certain

emulsion and we grade lenses with different emulsions, and if the consumer does not know the differences when he is using them, then it is not significant for testing purposes.

For that reason, our new lens specifications have not gone away from that principle. They have introduced the Super XX emulsion which is commonly used and is the emulsion being used. If there are any significant differences, they will show up with that emulsion. That is an operational test. He can test his lenses any way he wants and we decide whether they are good or not.

We have found that this business of using short exposures is a practical matter because we are operating in a building where generators are working and people are running and walking around. The use of an Edgerton type of flash seems to improve the quality of the lens. It takes out one of the distracting phenomena.

Also, it is important (and I here agree with Dr. Gardner) that the photoelectric tests and all that be considered. They are very interesting and relevant, and much good information will come out. But, when a man wants to know about the lens and you show him all the equipment, he lacks inspiration in that method; unless you have an equivalent lens in your method, that is hard to establish.

Therefore, in the past we have stuck closely to resolution tests as being closer to the operation of the lens, and because of our lack of confidence in the use of interferometers as well as the fact that it is easier to interpret them.

Regarding this mythical conversion of the grading of lenses, where, if you use a low-contrast target, you will grade the lens in inverse order than when you use a high-contrast target, I am willing to be shown, but we will continue to use the high-contrast procedure. The logic is based on our own experience, but we are willing to be convinced.

Again, I must point out that high resolution in a laboratory has always meant high resolution on the photographic image. I am thinking of Baker's 40″ *f* 5.0, Gardner's 40″ and our 36″ lenses. This has been an excellently integrated experience. When the lenses are good in the laboratory, they make good pictures, and it is hard for anybody to argue that, especially when our business is to choose lenses that make good pictures.

I am in agreement with Dr. Pestrecov and am very interested in his use of the semantic approach. I wonder if he is trying to hint that perhaps calibrating is a boring subject. At least that follows from that dictionary definition.

I should like to put in a plug for the military services. Nobody else does, so I must. During the war I had the misfortune of serving on some ASA committees which labored mightily, meeting after meeting. We finally produced some very weak standards. This was in the field of shutters. ASA tried strenuously to produce optical standards, but it did not come through, because there was terrific disagreement. I should like to report here (and Mr. Pryor can add a few words later, since he had the most to do with it) that the military services, together with the manufacturers, got together and produced a standard which is a big, fat document; but strangely enough, they all seem to agree on the standard in fairly short order. It didn't seem to me to be short order when they were being coordinated by Pryor, but really, from the standpoint of the achievement, it was short order. I call the attention of everybody concerned to this particular standard which will be available in about three or four weeks.

Dr. Pestrecov seemed to emphasize (and rightly so) this whole business of operational definition, which is the sort of thing that Sewell did in his paper. Sewell is in the business of calibrating cameras. He therefore invented a system yielding a method which can be applied to the making of maps and does not mess around too much with fictitious points, with pure theory and academic elegance. He has an operational definition that can be translated into the map-making process. Since he likes it, it seems to be a pretty good method.

I had a brief opportunity to read Roelofs' paper. I like the analytical approach, producing a general analysis out of which Sewell's method drops out as a particularly special, but on the other hand a most important case. This course to an analysis of photogrammetry makes a very general analysis out of the oblique picture, while the vertical drops out as a special but important case. It seemed to be an analogous analysis.

Unless Sewell has mentioned it, I should like to remark that some of his recent tests seemed to indicate that this calibrated focal-length, this gadget by which you want to find the dimension on the picture, varies with the *f* stop.

Mr. Eldon Sewell [Aerial Photographic Branch, Wright Field, Dayton, Ohio]: Very slightly.

Mr. Katz: It seems to vary. This bears out an approach we have been taking in the field with respect to all cameras. It turns out that T-11 is the first camera in which we are following the sound approach of doing away with the diaphragm.

We believe that at all times the highest possible shutter speed should be used, and we then provide ourselves with a shutter (as we have with the Wilcox-Fairchild T-11) which can give us complete control of the speed. There is no need for the diaphragm; therefore, we take pictures at maximum shutter speed and we get the sharpest pictures. This is being extended as fast as the shutters come along.

Mr. Odle discussed the use of low-contrast targets. I voiced some objection earlier. In general, we feel that it seems to lose the type of sensitivity that is necessary. The same result is achieved as with a coarse-grain emulsion for the lens designers. There is a lack of sensitivity for us in being able to find the point. The numbers turn out to be all small, so we don't like it very much; as long as we get good results with the high contrast, we will use that.

I don't think there is much of a lens problem. By and large, lenses are much, much better than we are able to use them. The actual use in the field doesn't come up anywhere near to the actual value in the performance that the lens has. Of course, every once in a while somebody gives me a photograph made with another lens which we haven't tested and says, "Here is a remarkable enlargement." Then we find that it was not made under military conditions at 500 miles per hour, but at 100 miles per hour. So lens evaluation must be made very carefully and the conditions must be specified and understood.

Chairman Howlett: It is always a matter of great fascination to me when my good friend Katz argues a very illogical position. He does it so convincingly. I do not think it should be allowed to pass because I was almost believing him myself. There is something about his manner that carries you along and you cease to think and you go glibly along in his path. I do not think that I have ever been able to attack Dr. Gardner's position as illogical, although I have disagreed many times with him.

If one wishes to choose that method of grading lenses which gives you the highest sensitivity, that is a point of view that has to be given full recognition, but one cannot at the same time say that the important thing is performance and in the next breath say, "We want the most sensitivity, the most sensitive test. We do not care whether it bears a relationship to practice or not."

One cannot accept that. It is something like judging electric light bulbs by saying that the light output is very important, and for this I will use a photometer to measure its density, because that has a bearing on the amount of light that comes out. On the other hand, he is going to judge the usefulness of these two light bulbs for reading and say, "I will not use anything that is at all parallel to the radiation of the eye because there is an error in photometers. I am going to put up thermometers, which you can read to a thousandth of a degree, and I can distinguish between these lamps to a degree of great accuracy."

That is an exact parallel to being willing to accept the emulsion of service and at the same time saying, "We do not care if all detail in an aerial photograph is of low contrast. We will take a high-contrast target which is much more sensitive. From the point of view of performance, we are not concerned with the difference. If one is going to go into line drawing, that is a useful distinction."

I detect still the viewpoint that resolution is some intrinsic property of the lens alone. I think it is about time that myth was exploded. You cannot separate the light source, the target, the receptor that you are using to pick up energy in the image space. They are all rolled in together. To feel that resolution is the property of any one of them leads to a set of impossible conclusions.

It may be that whether you use a high- or low-contrast target, or you use many lines or short or long lines, they all tend to put most lenses in the same order, but, surely, that is not an excuse for an illogical position. One must have a philosophy which underlies all one's testing and is in itself the justification, because you never know when you are going to meet the odd case that does not fall in with it. In testing you cannot

assume that all the lenses you are given are going to be perfect. Some may have strange characteristics.

There is certainly a wealth of evidence that the English have produced at Harrow and at R.A.E. and which we have produced in the Research Council. The results are available in the published literature. The conclusion is that the rating you give lenses on a high-contrast target is by no means the same as on a low-contrast target. There are a few cases in which the order is reversed, and it is to be expected, since the receptors are of different kinds, the reactions will be different.

If you are to draw a specification for performance, you must be logical throughout. If one takes another point of view which I recognize as logical, as Dr. Gardner does, that is all right, too. That is a difference in the test.

I think photogrammetrists are largely concerned with performance and that is the sensible thing. However, you cannot say on one particular aspect that it is important and on another aspect that it may or may not be important.

Mr. Katz: It isn't often that I am accused of being illogical. But this was a good argument and I almost found myself in agreement.

Let's go over this slowly. First of all, I should like to say that our lens specifications are going to include both high contrast and low contrast. But I will justify the high contrast. If this were only academic, if we did not have a history of lenses, then we could toss out everything done before and switch over to low contrast. We cannot discard past experiences. We have to know if a lens being produced today has a relation to a lens we tested five years ago. There is a point of continuity.

Reduced simply to an operational argument, my argument is this: Assume that I am called upon by a military man who doesn't know anything about this matter. He wants to know if the system we are using produces for his use lenses which take sharp pictures and, in greater particular, if the system we use continually gets better lenses.

My whole argument rests on the point that that system has done exactly that, independent of all these other corrections, and these other things which may not have a bearing. We have a system by which we grade lenses *A*, *B*, *C* and *D*, not just a little ahead of each other. We say a lens is much better than another lens, if it produces pictures much better. That is a simple test, and, since we are working for the consumer and paid by him, you cannot evade it.

We are not unreasonable, so we would like to know more about lens performance, and we are introducing these other things as far as we can. It took us years to get the manufacturers to use resolution tests of the old type that Dr. Howlett would like to toss out. There is a virtue of presenting a matter of decision. If after ten years of arguing, we sold them on using a target that was a high-contrast target, and then we said that was no good and they should use the low-contrast target, they would be worried about the selling we put in to get them to use the first target or any target, for that matter.

These are matters which have to be considered.

Dr. Gardner: Dr. Howlett is so genial that I am not quite sure that he is always quoting my viewpoint. I am afraid I have been misunderstood. I didn't mean to say that those differences in resolving power that came on the fine-grain plate were unimportant. I meant to say that they were easier to see and did facilitate measurement.

Let's go back. Suppose we are testing electricity. I use an electric light to read by. One way would be to test a light back of the shoulder and try to change the lights and see which one I could best read by. However, I could test that light better by a photometer. A test such as the first is used when we do not have enough knowledge to go beyond that. We try to use the simulated test in all cases.

If we are buying insulating material to use in the walls of our house, we do not build a house and put the insulating material in it and sit in the house and see how it works. We measure the thermocontactivity and we know the story.

We are not measuring things that do not have meaning. We are making measurements to get information we want. We only have to simulate the conditions we are using when we are ignorant of certain characteristics. We have a certain amount of knowledge and I believe we should use it.

Mr. Odle: From the English side, it is true we have done a considerable amount of

work on the low-contrast methods and I have included a summary in this paper. We are in agreement with Dr. Howlett.

If the lenses of which we are talking are to be used in low-contrast conditions, you must influence lens design to get away from assessing lenses by high-contrast. It is no good for the lens maker to say that this lens results in 150 lines per millimeter. You have to explain to him that you are not getting the resolution you think you ought to get with that contrast target. The two are so far apart that they must think in terms of making lenses that give us low-contrast targets. You cannot hold up progress by saying we persuaded them to make lens tests and we mustn't change that.

For the general use of stereoscopic methods we spent years persuading our lens makers to produce lenses of the same focal length. It was very convenient for the simple methods of plotting. But after the war we said we were not interested in that and we wanted the distribution to be all the same and have the focal length vary. If we did not do that, we would not have progress.

Mr. Corten: We had about the same argument in Paris. Then the point came up that there is no objection at all to do the one and not leave out the other. That is the reason why we put in the draft to use high contrast and low contrast. It all depends upon the purpose for which we want to use the standards—what we want to do in setting standards for the future and from the viewpoint of the *user*, not for research, not for development and not for the manufacturer.

Of course, anybody can do what he wants. I do not see any point in arguing very long because, if we do use two types of targets as long as it is necessary, then we have a correlation of these lenses, the future ones with the old ones. As it is with all human beings, when a thing gets obsolete, the thing is thrown out. But it will take some time and we can just let it live as long as it wants to live.

Mr. Paul Pryor [Photographic Laboratory, Wright Field, Dayton, Ohio]: I should like to say that in reading over the standards from Paris, I was extremely pleased to see how well they followed the standards that we set up at Wright Field. There are only a couple of little points that we can iron out, and there won't be disagreement with the military standards that we have set up.

To get back to this matter of high- and low-contrast. Our position is of taking data which we can compare with past data. It is a compromise position; it may be illogical to test with a high-contrast target on fast film, but it is convenient. I think of the Sewall test and the tests that were run during the war in which Dr. Macdonald painted a bunch of high-contrast targets at Wright Field. We looked at a lot of pictures made from the air of both high- and low-contrast targets and we found there wasn't any point paying any attention to the low-contrast targets. We got all we wanted from the high-contrast targets and we now have only high-contrast fields.

We have found that most of the information we want can be obtained from the high-contrast line targets.

Professor Schermerhorn: The difference between high- and low-contrast targets now seems not to be too important. But for me it is a question whether, with the coming improvement of the resolving power of lenses, there will not come a moment when you can separate the smaller differences in quality better with lower contrast than with high contrast. It means there is a finer method of measurement with the low contrast than with the higher.

Mr. Pryor: That may be with the targets used; but with the collimator we can make that change gradually.

Chairman Howlett: Mr. Corten gave us the answer: That we should use both targets. Let us not insist that we have decided that at Wright Field low contrast is no good.

Mr. Pryor: Actually at Wright Field in the meeting for standardizing the military standards, we set up the same principles you set up in Paris.

Chairman Howlett: If we can agree on that, it is very useful. Obviously, there are several points of view. I do not think anybody can make an overwhelming case for the non-use of one or the other. Why not have targets from now on that will incorporate both; five years from now we can look them over and see whether one or the other is useless and we can then throw it out.

At this stage it would be unfortunate to flog our individual hobby-horses. One can produce results on high contrasts on lenses *A* and *B* that are in the order of 1 and 2, and on the low contrast in the order of 2 and 1. It is not big, but Professor Schermerhorn made a fine point. If there is to be progress, let us not tie down on any specific angle.

Mr. Sewell: I should like to make a remark about Professor Roelofs' paper. I have just discovered the difference between his calibrated principal point and the point of symmetry. He arrives at a point at which all distortions are symmetrical; and so do we. He seems to know how he got there by taking the various steps he did; and we do not have the least idea. We lumped together all kinds of radial distortions, whether built into the lens or the radial component of the decentering defect, due to the non-perpendicularity of the lens barrel or what not. Professor Roelof analyzes each one of these. He does say, however, that he is assuming the tangental distortion to be zero in this particular case, and that he will publish later on a more general case of what he considers to be tangental distortion. I am interested in seeing that.

With a particular range on which you have been working or with certain angular data, it is possible with a single plate to determine the point of symmetry in fifteen minutes. We do it with two sets of points close together. They must be on the crest of the curve where they will not change with the distance from the center. We determine two points of symmetry for each diagonal, and if they come very close (and they always do), we assume we have made no mistake in measurement. It is a very simple method. As I say, we do not know where these distortions come from. But I believe we come to about the same point.

I think we have not been calibrating cameras at Wright Field, but rather inspecting cameras, because if we find something wrong with the camera or a lens (of course, we never do), our policy is to be to send it back and have it corrected.

Captain O. S. Reading [U. S. Coast and Geodetic Survey]: I certainly think we have come a long way in the last year or so in point of agreement, and it is very heartening to one who has the charge of trying to advance the purpose of the International Society. It seems we have still a few slight differences before we can come to a conclusion. While we have so much talent together here, I hope you gentlemen can go a little bit farther into these final points to see whether they can be ironed out in a practical way. I think we are all agreed with Professor Schermerhorn that everybody will do what they want to do anyhow and we had better give them our blessing and encourage them. On the other hand, we are confronted with a very real difficulty in understanding each other, and in overcoming that we have made a tremendous amount of progress.

We have on display here a lens which is reported to be better than normal-angle photogrammetric lenses available to the user in the past. They have a quotation that looks very reasonable in terms of our proposals here today as to this test. But are we in a position to buy that lens regardless of the strength of that test? Nearly everybody would want to test it for himself before he put out the money. That is why we need an agreement mutually understood to help us along.

Another thing that has developed out of this discussion is the glare test. That has not generally been applied, as far as I know. I remember in the days when we tried to buy nine-lens cameras that we were disappointed in finding something between the 1- and 3-minute prism effect on lenses we thought were perfect, and the distortion was supposed to be down to a minimum.

We have this measurement of tangental distortion. We suddenly find the manufacturers have offered to change from tests which give 36 microns in a 6-inch lens to one that gives 15. So when we do get a test, the manufacturers will meet our needs. The manufacturers say, "Tell us what you want." We say, "All you can give us." But that does not satisfy them. When we get a test, we usually get the product at a fair price.

It seems to me this glare test is something that is yet to be worked out. It seems to be quite important, like our agreement that we must have a test that will give us tangental distortion, if it exists.

I hope all this talent can get together while here and tell us what is practical. Maybe we must do some more work. Dr. Gardner mentioned doing away with the collimator. At a moderate distance that might be a suggestion worth investigating. If so, let's get him to try it and people in other countries to do the same, so we can get agreement by 1952.

It seems to me that this glare factor, particularly when we have so much information about our low-contrast problem, is likely to have an important bearing on the quality of our lenses. At least, we will make certain that we will get satisfactory coating of the elements that way. When you go up to seven or nine elements in a lens, that might become very important. Can you gentlemen get together and see what remains to be done in the way of research? We have about one more year before the International Conference. Then we will have to say, "This is the best we can do."

Mr. Corten: I am very glad to say that I have an invitation from Dr. Washer and Dr. Gardner to go to the National Bureau of Standards and discuss these further points. As in all meetings, you do not have an ample opportunity to discuss all the aspects of a problem.

I spent a week in Switzerland, and I have taken this report of Mr. Deeg as the basis. We discussed all these points; that took us a week. That was done among a few people who are supposed to have some opinions on these matters. Our company is using Wild apparatus, so we would suppose there would be a general and common opinion; but still the discussion took nearly a whole week. It resulted in all sorts of papers and remarks. I am very happy to be in a position to accept this invitation of the National Bureau of Standards, and I hope that in two days we can settle these things.

The same can be done with the National Research Council in Canada. As I am happy to have an invitation from Dr. Howlett to discuss the same problems with him I have hopes that we can come to full agreement.

Mr. Macdonald: Mr. Katz made reference to a statement I made this morning. But it was an incorrect interpretation, and I should like to review that point. We have found in extensive aerial tests of 40″ *f* 5.0 lens, which was alluded to in this case and which started in 1946 and which were published in NDRC reports at that time, that the laboratory setting that gave peak resolution on a high-contrast target was not the same focal setting that gave peak resolution in aerial tests. There was a shift in focus.

As Mr. Katz pointed out, I painted some of the first aerial targets on the runway in Wright Field. I have been an advocate of high-contrast targets since that day, probably because I would hate the job of repainting them, if nothing else. However, we must be aware of the limitations of the high-contrast target. This point is one of them: that the setting in the laboratory will not be the same as the setting that will give you the best flight test results. I think it would be an exceptional lens where it would be the same.

In view of long-range research, it is in order to review how resolution targets came into being. Dr. Gardner has certainly made some strong statements, and I hate to tread on thin ice in stating that we have better methods. But, honestly, I feel that we do not know today what we want to know when we look at aerial pictures.

Resolution was a term invented by *Dawes* in 1850 as a result of a series of visual experimentations to separate two star images. In 1879 *Lord Railley* published a definition for the resolution of star or point images. The lens designer likes to talk about point images. In practice, the experimenter cannot use points. They are just too small to handle conveniently. So we have substituted lines or holey doughnuts for points in some cases; nonetheless, lines are used where we feel they can be observed. It represents an approximation to the resolving of two star images at that point.

There are two objections to all targets. The observer knows what he is looking for. He is either trying to see three lines or he is trying to see a complete ring in a circle. This is one objection in that there is a psychological effect on the observer.

The second objection to targets is the fact that the image deals with many coordinates that occur in the over-all azimuth.

Dr. Gardner has mentioned the problem of limited resolution. That is the step of finding the resolution at a point. Actually, the shape of the blur circle at a particular point, where you have resolution at right angle to one another of different magnitude, will not be an area in proportion to the product. The simple case is to consider a symmetric blur circle that is translated during the blur. That means the shape of this blur circle is a style E circle with a factor $T \times \text{cosine } \theta$. The azimuth is added so it looks like the figure 8 with a diameter in the center that is a finite area that can come out of these measures, these multiple perpendicular values of resolution.

If we are to find how small a point we can push into them, we can use that system.

I should like to summarize by saying that the problem, when you are looking at an aerial negative, is not necessarily to see the smallest detail. I think we can relate this to the visual process. *Lausch* and *Moss* in their recent series in the *Journal of the Optical Society* have pointed out quite clearly that the problem of seeing is one of edges, that the level of density on the film is unimportant. It is merely the fact that there is a boundary between two differences in density, and this is the visual mechanism.

Dr. Howlett pointed out a while ago that the problem of distinguishing edges is the important one. I think it is. It relates to an index number, certainly, because it requires either sufficient contrast or sufficient grading. There is a combination there. You cannot see a gradual transition from a light to a dark tone. That is certainly a lens that allows a step-grading; in other words, the factors of the grading and the magnitude contrast are both important. They both relate closely to the contrast-reduction function. In this case, high-contrast targets again serve the purpose because they let you offer a greater range of detail size in obtaining this function.

These are all the points we need to iron out. I would hate to say we should accept any target as being satisfactory, just because it apparently results in good pictures. This may be only because our present lenses and emulsions are just so bad.

Professor Schermerhorn: Before ending this panel, I should like to say a few words to you, Mr. Chairman, for the way in which you have presided at this panel discussion. You are well-known in this field to all of us. If there is one man who should be the chairman of this panel discussion, it would be Dr. Howlett, who has done so much good work in this field and developed a laboratory in Canada that is famous among all of us.

I must say that the panel discussion has made a great impression on me in that the differences between the European point of view, between what we brought here from Paris, and what is the general opinion here, are much smaller than I had expected to find.

I organized the Paris conference in order to have a little solid position to stand on here in this friendly panel discussion and to prepare for it as well as possible. It was not necessary for the reason I believed, but still I believe it was a good idea, because it shows now that things are developed much better and further than we thought during the conference or at the Congress in The Hague in 1948.

There has been quite a lot done on both sides of the ocean. That is now evident.

I should like to stress the fact that we, as users of the instruments, depend on the few men in the world who are working for us. The precision of photogrammetry nowadays is not determined by the plotting instruments. It is my strong conviction that they are more accurate than the material that we get. It is for that reason that the lens designers and the camera designers are the most important men in the field of photogrammetry. We must be grateful to these few men who are now designing for us better lenses of higher resolving power than we had before. We are here discussing the methods to prove their work, to check their results. But my last word might well be a tribute to these men, these three or four in the world, from whom we get further developments in photogrammetry.

Chairman Howlett: Professor Schermerhorn, it is extremely difficult after such kindly and undeserved remarks, as far as I am concerned, to say anything elaborate in the way of appreciation. I can just say: Thank you very much.

I should like to record publicly my great appreciation of the way my colleagues carried the burden here. I am duly appreciative to all of them.

I am sure the North American members of the panel wish me to express particular appreciation to our colleagues from Europe who have quite obviously spent a great deal of time and eflort in bringing to us what I feel can be the groundwork of complete agreement.

As far as my own views are concerned, I am sure this document could well be the basis that could easily be worked into something to which our laboratory could subscribe. I would rather gather that from the remarks of other people. If we accept two points of view for a while, this may not necessarily be a final solution. But at least something can be accomplished. I feel, with Captain Reading, that we can accomplish that much. Captain Reading, by the way, should also get a tribute in all this. He has been rather a quiet worker in the background, but he does prod from time to time and sees that we do not forget this important matter.

I now declare the meeting ended.