Muehlberger

MSC-06806

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

APOLLO 16 PHOTOGRAPHIC AND SCIENTIFIC DEBRIEFING

MAY 9, 1972

PREPARED BY
SCIENCE MISSIONS SUPPORT DIVISION
SCIENCE REQUIREMENTS
AND OPERATIONS BRANCH

MANNED SPACECRAFT CENTER
HOUSTON, TEXAS

FOREWORD

This is the transcription of the Apollo 16 Photographic and Scientific Debriefing conducted at the Manned Spacecraft Center Building 30 Auditorium May 9, 1972. The Apollo 16 astronauts were John W. Young, commander; Thomas K. (Ken) Mattingly II, command module pilot; and Charles (Charlie) M. Duke, Jr., lunar module pilot. The debriefing chairman was James A. Lovell.

Where possible, the last names of those who asked questions are indicated at the extreme left of each page; otherwise, the word "QUERY" is used. In the transcribed text, a series of three dots (...) is used to designate garbling caused by multiple speaking or recording problems. Two dashes (- -) are used to indicate an interruption by another speaker. If a word could not be verified as valid, the phonetic equivalent is provided followed by a bracketed question mark [?].

CONTENTS

OPENING REMARKS

CHAIRMAN First of all, let me say thank you from S&AD for the scientific help on Apollo 16. I personally think that you all did a very creditable job in helping the Science Support Room and, preflight, to get all the experiments ready to prepare the crew and the flight for Apollo 16. I think that, looking back now, we can say that we had a very successful mission, that we attained a tremendous amount of information from the flight. I know that the rock boxes at the curatorial facility are bursting at their seams, and the geologists are going to have a lot to do. I think also that, because of the problems that arose and the necessity to change the Flight Plan, you responded quite well to get the maximum science from the time that we had available. I realize that, in all of our flights, there hasn't been one flight yet that ran according to schedule; and I am very sure that Apollo 17 will be the same, that we will have to change things real time to get the maximum science from the mission. I think the response on this flight was superb.

Today, the briefing is in two parts and for two purposes. First of all, I'd like each Principal Investigator to briefly describe the results so far obtained from his work.

CHAIRMAN (CONT'D) This is to educate the crew on what we have obtained so far from Apollo 16. Also, I'd like to have him describe any anomalies that might have occurred that we do not know about now. Second, we'd like to have the PI have the crew answer any questions that might still be puzzling him so that he might further his analysis of the data. We have a lot of people to go through today, a lot of science to cover. So we'd like to keep each briefing short, and we don't want to cover things that are general knowledge as of today.

LUNAR SURFACE EXPERIMENTS

LUNAR SURFACE MAGNETOMETER (S-034) AND PORTABLE MAGNETOMETER (S-198)

CHAIRMAN The first PI is Palmer Dyal, Lunar Surface Magnetometer. After each experimenter describes his experiment, we'll have questions, Palmer, so we can ask the crew any questions after your particular talk.

DYAL The main functions of both instruments went nominally. I'd like to cover both the portable and the surface magnetometers. The ALSEP instrument, the surface magnetometer that you deployed first, looked like it went according to plan. The field that we measured as soon as it was turned on (it was turned on about 15 minutes or so after you deployed it) was 230 gamma and in a downward direction. At that particular time, that was the highest field that we had ever measured on the lunar surface. The calibration of the instrument went straightforward. We did do a gradient determination of the field at the site. The site survey, as we call it, functioned normally. Its thermal control subsystem is the best that we've put on the Moon so far. We have a DELTA-T from lunar day to lunar night of 51° C which is a factor of 2 better than the Apollo 12 thermal subsystem. I noticed in the photograph yesterday that the bubble level was right in the center ring. The

DYAL (CONT'D) level sensors that we have are accurate to a quarter of a degree, and they show that the instrument is level to 1° accuracy right now. The instrument has new sensors in it. These are more stable, and it's really the first chance we have of doing network-type measurements of the fields on the Moon. We have seen simultaneous data now from 15 and 16. We see the magnetic fields due to eddy currents that are driven in the entire lunar sphere. We have always made the assumption that we've got an instrument that's setting at one point on a sphere and that we're looking at the properties of a whole sphere, and we had some experimental evidence that that was the case. Now, we have unambiguously shown with this second instrument that that is indeed the case and that the assumption is correct that we are looking at a whole spherical response of the Moon.

YOUNG You mean you're seeing eddy currents all the way through the Moon?

DYAL We were seeing eddy currents that travel around the whole sphere, but with these new sensors now and with long-term data, what we're going to try and do is look, as you say, right at the center of the Moon as they go all the way through. That's what we're waiting for during the lunar night as a nice step function and a long term, both before

DYAL (CONT'D) and after, so we can see these currents diffuse right through the center of the Moon. The other thing that is unique about this instrument is that now we have a chance to look at the azimuthal variations in conductivity. We can not only look at radial dependence of the electrical conductivity and calculate a temperature, but now we can look at the azimuthal or angular variations between the Apollo 15 and Apollo 16 sites. That spread is far enough that we ought to be able to extrapolate those measurements to a great circle around the whole Moon, as far as azimuthal dependence is concerned. The portable magnetometer was really exciting. First of all, the first field you measured was 180 gamma down; in the Cayley, it looked like all the fields were, in essence, pointed in a downward direction. At the ALSEP site, it was 230 gamma; up near Spook, it was 180 gamma; and then on the other side of the LM, where you parked the Rover at the last station, it was 120 gamma. At that station, you put a rock on it, and we measured about 4.7 gamma from that rock. So, the rock was large enough and had a large enough moment that we did see a difference. These measurements do have an error bar on them that is plus or minus as much as 5 to 10 gamma because the solar wind and all the other inductive fields that are around that have to be subtracted from the

DYAL (CONT'D) measurements from the tape and data reduction. The measurements at station 5 were pointed upward, and the measurements up near North Ray Crater were pointed downward at 313 gamma. I think that there are some things that we could probably say, making a lot of assumptions, but it looks like this highland material is older than the other; it looks like we have at least a chance of looking at the paleomagnetic history of the lunar crust from these measurements. If the perming source (the source of this field) remained stable over the time period that the maria were cooling, the high field would indicate that it indeed had a time variation in its magnitude or that the flooding of the maria basins (or whatever caused the maria basins to be as they are today) demagnetized the material that had been there originally. In other words, the maria material is less magnetic, it seems, than this material. The interesting thing, too, is that the samples, as you know, are from the regolith, and they have been physically modified over the years. The measurements that we've obtained over a scale size on the order of 10 kilometers indicate that we're looking at a depth well below the regolith, and that this is indicative of fields that were at the Moon on the order of 3 to 4 billion years ago. The other thing that we can say now from simultaneous measurements of solar

DYAL (CONT'D) wind and magnetic fields at the Apollo 12 and 15 site is that these high fields that you measured at the Apollo 16 site modify drastically the direction and interaction of the solar wind with the Moon at these places. It should channel the charged particles and different locations asymetrically on the lunar surface in these areas. In some cases, one could now state that the scale sizes of the field are large enough so that you could form a shock and actually stand off the solar wind over small regions of the Moon. I guess that covers both of them, Jim.

CHAIRMAN Do you have any questions you want to ask the crew concerning the deployment or anything about the experiment that you don't know?

YOUNG Palmer, let me ask you. What was the effect of that rock out there by the big LSM? Did that hurt it much?

DYAL No. Where you parked the Rover the first time near the TV camera, we were extremely disturbed because the angle was such that it looked like that rock was as big as the electronics box, and it looked like the PRAs were oriented so it was shining right into them, and all the IR radiation would really heat us up during the daytime. But then, the other view showed that the rock was relatively small

DYAL (CONT'D) compared to the dimensions of the box, and it didn't affect the thermal subsystem at all. Magnetically, they really don't contain that much oriented field to do anything.

YOUNG I guess one of the things I'd like to say is that what we intended to do was to drive 100 yards away in front of the lunar module with the Rover and do a sort of a north-south traverse looking for the best place to deploy the ALSEP to get it away from all these things. We ran into problems with the UV in that it took longer to do the last measurements than I anticipated, and we couldn't do that. I'm sure that somebody looking at the photographs can find a better place out in front of the lunar module to put the total package, but I'd like to say that package is so big and that surface is so blocky and so full of craters that, under the circumstances, I almost believe we had to take what we got. I hate to say that, but I just wouldn't believe that surface was as rough and as covered with blocks as it turned out to be.

DUKE I think I could have still been walking up there with that package if I had been looking for a level spot. I got up on top of the ridge and I looked off and said, "Well, there's a good place over there." And I ran over there, and it didn't look any better than the place that I had just been;

DUKE (CONT'D) and, "Well, there's a good place over there." And I ran over there. Finally, after about the third time, I said, "Well, look, I'm just going to put this thing down here - best we got." But it's really blocky and a lot of little fresh craters there, secondaries.

DYAL I looked at each of the photographs that I could find where you had taken a picture of both the ALSEP magnetometer and the portable. You didn't put them next to rocks or craters on the scale size that was big enough to affect the instrument. I think that's the main criterion which was observed during that.

CHAIRMAN John, did you know the rationale for doing that unplanned portable magnetometer reading?

YOUNG No, but it doesn't make any difference. I mean, we did it. You can't explain it in real time. That's all right.

DUKE On the sun shield on the LSM, the latch didn't come loose. I kept pulling the arms up to try to get that latch loose, and I finally had to hold the arm down and get the latch loose with the other hand. And then, as I tried to lock the thing, the latch didn't fall off; it tangled up into that little wire that locks into the little ball.

DYAL Yes.

DUKE I almost left it like that without locking it. In fact, Houston said go ahead and leave it, but one more little effort and the thing finally dropped off. I thought I was going to disturb the level, but it seems that you're satisfied with the level.

DYAL The only thing that we're worried about there is that we've got a level sensor in the thing, and if you disturb that you can see that jiggle. When you read that shadow graph off, that's the only measurement we get in azimuth, ever.

DUKE Yes.

DYAL So as long as you didn't disturb the twisting of it, then it's fine.

CHAIRMAN Okay. Thank you, Palmer. Next experimenter will be Dr. Gary Latham, Passive Seismic Experiment.

QUERY May I ask a question from the floor?

CHAIRMAN Yes. Go ahead.

QUERY I wondered what observation in radiance from there? ...

DYAL Radiance from which instrument?

QUERY Over there.

DYAL There? Oh. We've got to take out the fluctuations from the solar wind from the other instrument using Apollo 15.

QUERY ...

DYAL No. We don't have that ...

QUERY The actual photo you took of the LSM was taken after you had deployed the sun shield, right?

DUKE The 3 footer I think was. That's correct. Yes.

QUERY There wasn't any disturbance?

DUKE We didn't touch it after that.

PASSIVE SEISMIC EXPERIMENT (S-031)

LATHAM Our fun began with the S-IVB impact on this mission. We lost tracking on it prematurely, which meant that we were not able to get the coordinates and time of the impact independent of our own measurements. Nevertheless, we could locate it fairly well from the two near stations - 12 and 14 - which made it a useful impact at the greater range up to station 15. We're looking at those signals now. I think we can say that this peculiarly high velocity mantle as we call it that we had found in the 12 and 14 region can't be a global feature unless it is exceedingly thin, LATHAM (CONT'D) a thin slab of this high-velocity stuff. And I must say that there is always the uncertainty that we really didn't see the first arrival up there because it was at 1100 kilometers, and the first signal you see is quite weak. There's always the uncertainty as to whether or not it is the first, the fastest traveling wave in the Moon and not something else. But, if it is, then this very-high-velocity material that we call mantle is not global or an exceedingly thin layer. It looks as though we get velocities approaching 8 km/sec at depths of the Moon on the order of 100 kilometers; not 9 km/sec as we had in the 12 and 14 region. There is also very weak evidence from that signal, and I haven't convinced my colleagues of this yet nor myself really, but the possibility of a reflection from a very deep interface, perhaps 550 kilometers deep, is there. We're looking for ways to see whether or not that can be verified. In other words, it is a primitive core, perhaps, or some other reflector at a very great depth. So this impact will, I think, provide very, very useful data despite the loss of tracking. We would have been, of course, much better off had we been able to photograph that impact area, and I understand that the curtailed time in orbit precluded that. The deployment was good. I think the pictures tell the story as far as I'm concerned. The instrument does

LATHAM (CONT'D) get hot during the lunar day as the other instruments have. This has been the case in every one. It's just not possible to keep dust off that shroud when you have to work that close to it. And that degrades the thermal control some. It does not degrade the seismic data. It simply means that the controllers have more work to do trying to maintain the thermal stability. It's a problem we have faced in every one of the missions. In fact, I thought the 16 deployment, the configuration of the shroud that I saw and so on looked very, very good. There is one little place where it's raised up where the cable comes out underneath. It's turned on its edge a little bit, and that causes something of a heat loss; but it's not serious at all.

YOUNG Yes. We patted that rascal down because of the 15 problems. Before a guy leaves that ALSEP site, if he's got a problem like that, maybe - is ALSEP on 17? It's not, is it?

LATHAM ALSEP is on, but the seismic isn't.

YOUNG The seismic isn't. Okay. Well, those rascally things assume some different kind of orientation than they did before we left. Maybe you ought to go back one more time and make sure those things haven't changed. I think maybe they outgas a little, and then take up a different shape.

LATHAM Well, it's not only your near activities. I think when we saw that TV picture degrade on LM ascent, it's obvious that a lot of debris is being thrown around, and you just can't avoid a good dusting down from that source. So our carefully prepared thermal surfaces act more like black-bodies than anybody figured on as a result of all this. Then we saw your Rover signals, which this time provided very, very interesting data in that they showed rather abrupt changes in signal level as you moved around. We're not sure yet what to make of that. We're going to work with Bill Muehlberger and his crew carefully on the traverse to see whether or not we can identify specific provinces in which the signal level is quite a bit higher. I guess I would like to ask your impression as you were rolling along the given EVAs that you felt at given times that the Rover was bouncing noticeably more than at other times that might have generated higher signal levels.

YOUNG Sometimes she was off the ground, there's no doubt about that.

DUKE To the south on EVA-2, that area was a lot rougher than the traverse route to North Ray Crater. My impression of North Ray Crater traverse: once we passed Palmetto, it was really a boulder-free area, very subdued old craters, and

DUKE (CONT'D) the Rover just sped along much like on a West-Texas-type terrain; whereas to the south, it was really rough and we had to maneuver.

YOUNG Particularly on Survey Ridge when we were traversing that area with all the secondaries and blocks, we managed to be up in the air quite a bit, simply because there were so many secondaries and blocks that we had to hit some small ones to avoid the big ones.

DUKE Another subjective opinion of mine is that, at least at stop 13 where we actually got off the Rover, the regolith did not seem as loosely compacted as to the south. In fact, at North Ray Crater, station 11 and 12, it was no more than a couple of inches deep because we couldn't get the rake in without bending the tines. So the regolith up there was very thin. And I don't know whether that means there are just some very cobbly, densely compacted blocks under there that were thrown out or that are now that much covered or whether we just picked some bad sites. But we tried to rake twice, and both times the only luck we had was kicking stuff into the rake. We couldn't pull the rake through the regolith.

YOUNG And you couldn't stick the tongs in either.

DUKE Yes. The tongs wouldn't go in. Every other place, you could take the tongs and stick them into the ground, and they would stand up for you.

LATHAM Well, in general, this area, from the general character of our signals, gives the appearance of being the thickest pile of what we might loosely call regolith of any of the sites. I guess Bob Kovach will talk on his results on that, and we await his mortar firing to give us a little more information on that. We'll be looking at these Rover signals and see if we can somehow pin them down to roughness of terrain or just what from the pictures that you took along the way. We now have the quiet, nighttime period, and we're waiting for the first moonquake of this session, which ought to be before May 12. Of course, with this last station we now have completed a very nice triangular array. The other three gave us a very narrow-based thing. Now we have a thing with a thousand-kilometer baseline, which, if it lasts for as long as they appear to be lasting, will give us the tools to really do the job for the next couple of years. And, of course, we'll be using that in the S-IVB impact from the next mission.

I'd like to turn to one observation reported from orbit that has interested us a great deal, and that is the flash

LATHAM (CONT'D) that was reported. I understand the transcript has not yet been typed.

MATTINGLY I haven't seen it yet. That's my only hope for pinning down the time. I can peg it to about 5 minutes with that, I think.

LATHAM If you can help us pin down the time and roughly the location, we'll certainly look at our records and that would be an important piece of data if we recorded that. I would like to ask was this a colored flash or a white flash?

MATTINGLY It was white.

LATHAM How does if differ from the kind of thing you get with a cosmic-ray impact on your brain?

MATTINGLY I didn't see any of those.

LATHAM Well, we were very much excited by it. As far as I know, it's the first report of a transient event of some nature that's been seen from orbit.

MATTINGLY I should have written it down. It just didn't occur to me to write it down.

CHAIRMAN Did we get a time on it at all, Ken?

MATTINGLY It's on the DSE.

CHAIRMAN Then it would correlate with any information we get from the seismic devices.

LATHAM Yes. How about its persistence, did it - -

MATTINGLY No. It was just a flash and the way I happened to notice it, I was looking at a horizon that was showing up from solar corona, on zodiacal light or whatever you want to call it in that region. It was very shortly after we lost signal from Earth. I was watching stars pop up over the horizon. And I got this flash which - I wasn't looking directly at it at the time it happened. It happened down in the side of my vision, but it was brighter than the brightest star that I had in the field of view at the time. I had the feeling that it was, in physical or angular size, equivalent to the size of the larger stars in my perceived vision, but it was just an instantaneous flash. It took a couple of seconds for it to soak into me that it wasn't just a star popping up over the horizon but rather it had been distinctly below the horizon.

LATHAM Is there a way for me to get that transcript? I don't know if I would normally get it.

CHAIRMAN I'm glad you asked. I forgot to mention this in the opening remarks, but we have about 50 copies of this technical air-to-ground voice transcription right back there for the PIs and the co-Is and there's a copy for you, Gary.

MATTINGLY That's not what you're after. It would be on the DSE, and that hasn't been completed as far as I know. One other thing that I'd like to mention. From orbit, it appeared to me that there was a distinctly different unit up around North Ray and that area. I'd say a third to a half of the traverse to North Ray went across this unit. And that may or may not fit in with your seismic deltas.

LATHAM But at some point between North Ray and the LM, they would cross a contact of some kind.

MATTINGLY Yes, sir.

LATHAM Well, except to add my thanks for a very fine deployment, that's all I have.

DUKE Thank you.

CHAIRMAN Any questions from the floor?

QUERY I understand this flash was not in the vicinity of the S-IVB impact.

MATTINGLY Well, I don't know where the S-IVB hit, but this is on the back side of the LM, so I would assume that the S-IVB didn't hit there. And it was well after that, a couple of lunar days.

QUERY Where did the S-IVB impact?

LATHAM It hit about 150 kilometers north of station 12.

QUERY Ken, when you were observing stars, did you mean through optics or through the eyeball?

MATTINGLY No. The time I was doing this, I was looking out the window, and I had darkened the cockpit in preparation for one of the low-light-level photographic exercises. And that's how I happened to be noticing that there was this distinct horizon, which surprised me. I just happened to be kind of puzzling over that at the time.

CHAIRMAN Okay. Any other questions?

QUERY Can stars be seen from the surface, John?

YOUNG No.

DUKE Not when your visor's down.

QUERY When the visor is up?

DUKE I never looked with the visor up.

YOUNG Not supposed to do that.

MATTINGLY I suspect, from what we saw on EVA on the way home, that the inner visor alone has sufficient attenuation to block out stars.

YOUNG But you could see them through the AOT in the lunar module. Of course, that has a light shield around it. On our last alignment, even with a crescent Earth, in the AOT, we could see Achernar really so good that we didn't have to roll up the window shades in the cockpit. So if you look through a tube, I'm sure you could see every star out there.

DUKE The only thing we saw on the lunar surface was the Earth, and it was directly overhead. That was the only thing I saw in that sky.

QUERY John, when you are looking through the sight on the camera ..., did you see any ...?

YOUNG You see your helmet reflected. You have to raise your visor so you can get rid of all those reflections.

ACTIVE SEISMIC EXPERIMENT (S-033)

CHAIRMAN Next one is active seismic experiment. Dr. Kovach.

KOVACH We had several objectives on this experiment. I'd like to summarize the following basic questions. How thick is the seismic regolith? What were the in situ physical properties of the lunar near-surface material? Thirdly, are there any distinct seismic horizons, and how do they correlate with our estimates to geological horizons? Finally, were there any regional differences in seismic velocities; i.e., something characteristically different between the mare and the highlands? The deployment and the execution of the thumper experiment was outstanding. The records are clear, the background noise was sufficiently low, and we got clean first breaks completely down the geophone line. We could not have asked for a better - -

YOUNG I'm sorry about that first one. I was so really happy when that rascal worked that I started walking to the next one.

KOVACH Well, the record shows that, for some reason, you inadvertently didn't hold it in the charge position long enough, and that's the reason when you did it the second time, it worked.

YOUNG No, I thought I started walking too soon after the first one went off. The one that failed - that was pure procedure.

KOVACH Well, if you walked too soon, it didn't hurt us any.

YOUNG Didn't bother you? Okay.

KOVACH The data need yet to be corrected for topographic effects. There are some severe undulations in the topography, and we can see this in the data, but I can give you some first impressions of our results. Number 1, there is certainly no variability in the first arrival of the velocities across the geophone array. The velocity is again close to 100 m/sec, which seems to be the magic number for the regolith at many different places now on the Moon; i.e., out in the mare and finally up here in this highlands site. There was no evidence of flows beneath these geophone lines. I feel sufficiently confident that we would have recognized that. About the fact that we didn't recognize any variability in the velocity, we're able to say one more thing, because we recorded the LM ascent when we turned on the geophone line and that was a position some 140 meters away from our first geophone. We did get a faster apparent velocity. It's very close to the value measured for Fra Mauro for breccias. And so, with this type of a number

KOVACH (CONT'D) now; i.e., 2 to 300 m/sec underlying this regolith, we can put a thickness bound on the regolith at this site; and it is indeed very thick, at least 40 meters. I'll be able to refine that number a little bit when we get the mortars fired. We also did turn on the ASE geophone array and recorded when you were approaching the LM during the end of EVA-3. We also got very interesting signals, and we hope to analyze these in an analogous way as Gary has suggested. That's about all I can say with a quick look of our data at this point. I do have the concern about the grenade box deployment. I'm sure we have asked you that. I haven't seen any of the pictures yet, so maybe it will be self-explained. Maybe you could reassure me that it's level.

YOUNG It's level. I guarantee you. The fact is that was probably the only level place we had around there. I was really pleased to see when we got out to where we could deploy it that it would be level. It's really good. I don't remember what azimuth heading we put it on, seems like it was 330 as opposed to 333 that it should have been on. That's off the top of my head. We had to go back and look, but I reported it. And I found out later that you could break that pin by pulling on the leg, but I certainly

YOUNG (CONT'D) didn't know that. If somebody had told me that during the training, it would have gone right over my head. But we do have three good legs in there, and I'll bet you that rascal can't get out of the ground because of the way it went in. It's sort of like pushing it into quicksand. Once it gets in there, I defy anybody to get that mortar box back out.

KOVACH Again, I'd like to offer my thanks for an outstanding execution of that experiment. You couldn't ask for anything better.

YOUNG Thank you. It was our pleasure. Boy, it really worked good. I was really pleased.

CHAIRMAN Any questions from the floor?

QUERY Yes. Bob, how do you define the regolith site? You said ... with a - -

KOVACH How the geologists define the regolith may not agree with how we define it. But we define it as material which

KOVACH (CONT'D) apparently covers most of the lunar surface and has this characteristic velocity of 100 m/sec.

QUERY And what comes after that?

KOVACH Underneath this particular site?

QUERY Yes.

KOVACH Something that has velocities like Fra Mauro breccias.

MATTINGLY You said something about you were sure there was no flow material underneath this. I guess I missed the conclusion on that.

KOVACH We've got enough experiments on earth to say that we've been able to recognize flows because the velocities are characteristically much higher. If you want to argue that there may be very thin flows; i.e., thinner than our sampling wavelength, which is 2 to 3 meters, they could be there; but we certainly, on the average, didn't see any big sequence of high-velocity flows. And we would have recognized - -

MATTINGLY When you said high velocity, this 2 or 300 that you're talking about from the ascent stage - -

KOVACH That's not high - -

MATTINGLY That's not high - -

KOVACH In kilometers per second.

YOUNG Yes. Every small crater that we looked into, with the exception probably of Buster Crater, just looked like more of the same, it looked like regolith. We never saw anything that looked like outcrop, and we were sure looking for it.

CHAIRMAN This 2- to 300-meter velocity you're talking about from the LM ascent, that was this breccia material that you say is underlying the regolith, is that right?

KOVACH That's my first look at it.

CHAIRMAN First look.

MATTINGLY When do you fire the mortars?

CHAIRMAN Oh, that's a very good question. What's the latest word on mortar firing? Do we have any idea about what time on that?

KOVACH Well, there's a meeting at 1 o'clock this afternoon, and I'm going to request that they fire it on May 23.

CHAIRMAN Bastille Day, is it? (Laughter)

YOUNG And it's armed, too.

SOLAR WIND COMPOSITION (S-080)

CHAIRMAN Our next subject is the Solar Wind Composition. Dr. Meister.

MEISTER Unfortunately, there is not much to tell about the solar wind composition experiment. The foil was transferred to Switzerland at the end of last week, and we don't have any results yet, of course. The foil was deployed during the first EVA and retrieved at the end of the third EVA, with a total exposure time of 45 hours and 5 minutes. That's some 3 hours longer than the record of the previous missions. It was about 42 hours on Apollo 15. The main difference between the foil of Apollo 16 and the ones of the previous missions is that some pieces of platinum foil have been attached to the previous design, which was composed of a pure aluminum foil. These platinum foil pieces can be cleaned by fluoridic acid, which allows us to remove all the possible lunar-dust contamination. This technique has been tested in the lab on bombarded foils and showed that you can remove essentially all the lunar-dust contamination

MEISTER (CONT'D) without losing any measurable amount of trapped rare-gas ions or atoms of solar-wind origin. This technique should allow us to determine the isotopic composition of the rare-gas elements of solar-wind origin up to the mass of possibly krypton. The first visual inspection of the foil here at MSC showed that the foil is crimpled but essentially free of lunar dust. That's, of course, only a visual observation; we don't know what the foil looks like under microscope. I would like to thank the crew for the proper deployment and retrieval of the foil. We are pleased with what the foil looks like. Thank you very much.

YOUNG You can't miss when it tells you where the Sun is.

DUKE Yes, "Point this at Sun." The thing didn't roll up like I thought it was going to. And I'm sorry I had to crinkle it, but it was so big that I had to squeeze it down to get it into the bag; and it ripped once, too, I guess you saw that.

MEISTER That's only a problem of esthetics. It doesn't hurt it.

DUKE Okay, good. I didn't think it did.

CHAIRMAN Don?

QUERY If the local magnetic field is standing off the solar wind here, ...?

MEISTER We don't know yet. That might be. We have discrimination between the lighter and heavier elements in the solar wind. We have to check that maybe there's a dependence on the height over the lunar surface of the composition between the heavier and lighter elements; but we have to check that first and see.

QUERY Stand off the lighter particles - -

MEISTER The lighter particles would be deflected much more than the heavier ones.

DUKE This is probably a stupid question, but these particles that cause the light flashes, I was seeing them on the lunar surface during the sleep periods. Do those things register on your experiment?

MEISTER No. They have higher energies and they go through the foil.

DUKE I see. Yes. Huh?

CHAIRMAN That will show up on Fleischer's experiment.

QUERY ... Could you tell any difference in the material between the time you deployed it and the time you brought it back? Was there any brilliance or anything you might have observed?

DUKE No. No difference there. Looking at it one time out the window, I thought I saw some white streaks on it, but it might have been just the way the Sun was or it might have been those platinum strips that I never noticed when I emplaced it. I really don't know. It just looked like it had a couple of randomly oriented streaks on it to me, from the LM window. But when we rolled it back up again, instead of rolling straight up, it rolled out in a big long thing, and I had to redo it again. When that happened, I ripped it and then had to crunch it down.

MEISTER We don't see any difference between the foil we sent up and the foil that came down, except some lunar dust on it. Nothing else. Can I ask the crew a question? Do you have an idea how the foil was oriented? Was it essentially vertical to the - -

YOUNG The photographs.

MEISTER I mean along the gravitational force lines or was it reclined or inclined? It's hard to tell it from pictures.

YOUNG Yes. You can't tell it from the pictures.

DUKE I think I put it in almost parallel to the gravity vector. It's on a little slope, but if I recall, it's aligned almost vertically.

MEISTER Thank you very much.

COSMIC RAY DETECTOR (S-152)

CHAIRMAN Thank you. The next subject will be the Cosmic Ray Detector, Dr. Bob Fleischer. Is he here? Okay. He's not here.

SPEAKER The PIs couldn't be here this morning. They're busy at home studying the data that they got back. I have some words from them that I'll pass along as to what they think they'll be able to see. They are very excited about the possibilities they have. Early in the mission, a solar-particle event occurred that will enhance their data very significantly, they think. They think they'll have the opportunity to see particles from the Sun that, on an ordinary mission, they would never have had the opportunity to see. When they got the experiment back, panel 1 did have considerable dust on it, and this, from the best we can tell without any analysis, came from the landing itself, from the blast up from the DPS. Panel 1 was hot in taking the panels apart, panels 2 and 3 were cooler, and panel 4 was of the same order of temperatures as panels 2 and 3. The cosmic ray data itself in the plastics appear to be degraded somewhat because of the temperature, but they are very hopeful that a great part of the data will be retrievable. At the first look on panel 2, Dr. Fleischer says

SPEAKER (CONT'D) that the number of particles they see on the plastics indeed are a great deal higher than they would have nominally anticipated, indicating the effects of the solar-particle event. And they really haven't etched the plastics or analyzed them in detail. They are just beginning to do that.

YOUNG But they think they are usable?

SPEAKER Yes. The plastics.

YOUNG I was really surprised, because we had some lengthy discussions about this preflight, and I could never understand how we were going to fly this rascal to the Moon and get it there with these long periods of attitude hold that we're going into where it might see plus 250 all the time. And, of course, I'm sure those three revs in lunar orbit prior to landing didn't do any good either, because we were oriented many times so that we were facing - to maintain communications, we had the Sun shining on that rascal all the way around. And I really think that those 140 temperatures that we saw on the panels - if you go back and look at it thermally, you're going to find out they had to be there long before we ever got the thing on the ground. I was really concerned about that as to why we didn't put some kind of shielding over it, but it was too late, I guess, to do that sort of thing.

SPEAKER The thermal design was such that it could sit, I believe, in direct sunlight almost indefinitely without any degradation. It's the IR heating off the lunar surface that really cooks it. That's what the thermal people say. The early picture did show that the temp labels had already changed early at the beginning of the EVA.

DUKE They were black the first time we looked at them.

SPEAKER Right. The thermal analysis says if there's 15 percent dust on the panel after 20 degrees or so of Sun angle, you sort of exceed the 140 degrees on the frame. It appeared that there was probably 50 or 60 percent dust on the lower part of the panel or maybe even higher than that. It spent approximately 15 hours on the surface. So it may have just turned out to be a number of problems that probably could not be avoided under the circumstances that caused that lower panel to overheat. It was the hottest. The next panel was about 20 degrees cooler, the best we can tell, and the third panel up was 10 degrees cooler than the second panel. It seemed to be a dust problem. It doesn't seem to have hurt the data too much. Panel 4 was the one which we were afraid wasn't going to be activated because of the anomaly on the red lanyard. But after they had taken the panel apart, apparently every portion of the experiment

SPEAKER (CONT'D) was activated to some degree or another. The neutron portion was partially deployed, and they think they will get some data from it; not as much data statistically as they wished they had. Every portion of panel 4 does look like it will provide some useful data. They have much higher hopes now than they did when they first saw the gear. Apparently, the problem - and we haven't, again, sent the hardware back to the manufacturer for analysis - was a malfunction in the assembly of it that caused that thing to jam.

YOUNG If I had known about it at the time, we could have got that pair of pliers out there and pulled harder.

SPEAKER The investigator's opinion was that it would have done no good.

YOUNG It wouldn't have helped?

SPEAKER It was very severely jammed. He doesn't think any additional effort would have freed it. It was jammed pretty badly.

YOUNG Okay.

SPEAKER But I think, in all, they're very excited about the data and I think they're very optimistic now that they'll get a considerable amount of data from it.

QUERY Tim, it's probably worth mentioning that although those plastic ... overheated, they didn't have calibration plastics in there. The effect of the overheating was to anneal the cracks and reduce the crack density. With the calibration in there, it would be able to take out most of that effect. The other thing is that on panel 4, those experiments that are activated by pulling the cord are only a small fraction of the total part of panel 4, there's a lot of panel 4 including ... left in.

SPEAKER You'll still get a lot of good data. Just as good data as if the jam had never occurred.

YOUNG You may find a fingerprint on there, but we really were careful to get it out of there. Once we got it loose, we gave it the usual tender loving care in folding it up. So I don't think there's any crew fingerprints on the panel surfaces.

SPEAKER That's correct. We looked at it very carefully and except for the bottom part of the panel 1, you could see that it was a spray pattern. It was very clean and the PIs were very pleased about it.

QUERY Do you know when the solar particle event occurred?

SPEAKER Yes, it started on about Monday, which was the day after launch and went through about Wednesday. So about a 2- or 3-day type event. I think the peak must have been about Tuesday. Some satellite data are available; I don't have them with me. It was a small event, but for this solar cycle, it was very surprising that it happened at all during the mission. To have it during the portion when the cosmic ray was deployed or available to accept data, the probability of that is very small, so it was a very gratifying thing to occur.

QUERY Tim, I'm curious about the dust being kicked up by the DPS. Did you guys observe any dust on the thing or any other part of the LM at that level?

SPEAKER I guess not.

QUERY It's always been our impression that the dust is sent out in a pretty thin layer, that you don't get a billowing effect.

YOUNG I'm sure it's sent out in a thin layer, but with all those ... blocks around there, it's a possibility that some of them could come back at you. That's for sure.

SPEAKER It's extremely hard to see. When we photographed it, the lights were not similar to the Sun, probably much less in

SPEAKER (CONT'D) intensity, but yet the photographic lights did wash out. It's a very pale kind of dust, but it's very predominant. It came up from the corner, Dave, and in a pattern such that you'd expect it to be blown. There were some black streaks in there that appeared to be melted something or other on it. I think they're going to attempt to chemically analyze the material and try to find out what it is.

QUERY How high above the surface ... the LM?

SPEAKER That first panel must be about chest high, is it not?

QUERY Okay.

YOUNG Yes.

CHAIRMAN Are there any other questions?

DUKE Just one comment; the frame of that thing was hot. It was the only thing I felt through my gloves the whole stay. It was jammed and John tried to pull it out. I was holding the frame and I started feeling it through my gloves.

SPEAKER Did you get any feel for why it was jammed or where it was jammed. Did you have any clue to what caused it?

YOUNG It was jamming at the base.

DUKE Yes, at the bottom.

YOUNG Right at the bottom part. Something that broke free right at the base and then it just came out like it had grease on it.

SPEAKER Maybe you spilled some orange juice on it. (Laughter)

YOUNG No way. (Laughter) That's the only thing we didn't spill orange juice on. (Laughter)

SPEAKER It's not that you didn't try, though?

DUKE It's a good cement, you guys. You ought to start thinking about that. Expose that stuff to vacuum.

ULTRAVIOLET CAMERA (S-201)

CHAIRMAN Okay, our next experiment was the Far UV Camera. Dr. Page.

PAGE For the benefit of some of the geologists here, I'll account the goals, the purposes of this experiment which were to obtain photographs, for ultraviolet of the geocorona and the upper atmosphere of the Earth which these photographs, of course, ... spectra as well; solar wind, clouds, possibly either stellar or hydrogen; colors of stars, in the far ultraviolet and possibly intergalactic hydrogen. We had a lot of troubles. I guess John well knows. Before launch, the difficulty was to keep the camera dry because its optically sensitive surface would immediately run away if it

PAGE (CONT'D) got damp. This was accomplished with a bag that caused Captain Young a little bit of trouble in practices in advance but apparently worked all right on the lunar surface.

YOUNG Just great.

PAGE The second difficulty was getting through the Van Allen belts without fogging our film. I'll show you in a moment that we did that all right with a rather small amount of shielding around the film cassette. Then we had some difficulty with the LM being in the way. This came about because of the delay in touchdown, the high Sun angle, and the high necessity to keep the camera close to the LM so it would be in the shadow. With its gold surface, it would have heated up very rapidly if it had been in the full sunlight. The accomplishments that I will show you in a moment on the screen include 92 photographs in Lyman-alpha imagery and 53 spectra, some of them extending from 500 angstroms to 1550 angstroms. I think that's the farthest into the ultraviolet that anybody has ever taken astronomical pictures. We may get evidence of gases in the lunar atmosphere on several of our pointings which were low across the lunar horizon. If anything is coming out, like geysers of water or whatever it is, we'll certainly pick it up. The data on these photographs are extremely numerous, and it'll take

PAGE (CONT'D) us an estimated 6 months to a year to get them all out, which will be done with the big computer here at MSC. Dr. George Carruthers, who is the PI who designed the camera, happened to be free today from work on an Araby [?] flight is here and will undoubtedly make comments whenever he sees something to comment on on the photographs. Do we have the first slide? I might say that I got into trouble with the American Photographers Union in taking the prints of these because I'm not a member of that union, and they tried to throw me out of the darkroom over here.

This is probably one of the most dramatic pictures of the Earth that shows the auroral belt on the dark side. You're looking at the Earth with the Sun to the right. The south pole of the Earth is down and that funny liplike thing sticking off to the left at the bottom is aurora, we think, around the south magnetic pole. The most striking thing is the next lip up which, on the original you can see better than on this slide, goes around the full back side of the Earth. It's called the equatorial auroral belt, and this appears back of the dark side in the upper left corner. The third lip is another belt; it looks at first as if that was all one but there are two separate belts there. It was quite unexpected.

QUERY What's the condition of the Earth ...?

PAGE Well, you know, it's 8000 miles across. (Laughter) There's some halation here and I guess I'm not too sure; perhaps George knows. This is a special Eastman NTD3 emulsion - very, very thin. Of course, the exposure was made with electrons, not with light. An electronographic camera. But, the dimension works out right. You can't see it too well on this print. There's a ring or a limit to the field which, on the original, is 30-millimeters across. It's 20 degrees in the sky and the Earth is 2 degrees and it checks out. The dimension you see there is 2 degrees across the full diameter of the Earth.

QUERY Did you say that ... the equatorial auroral belt ... bottom. Is that the back side of the ...?

PAGE No, you can't see through the Earth.

QUERY That's why I'm asking.

PAGE The geometry is such that that is probably inclined about 30 degrees to the magnetic equator, and it is an unexpected auroral belt. It will take a little more figuring to figure this out.

QUERY What was the latitude of that third belt?

PAGE As I just said, my guess from looking at the pictures - it hasn't been measured accurately - is 30 degrees north. You will notice on these pictures that they're overprinted. That's because I'm not a member of the Photographers Union. The originals have a good deal more on them. This picture was taken excluding hydrogen light Lyman-alpha. We have two filters on the camera. This one is taken in light between the wavelengths 1230 and 1550 angstroms.

QUERY Is the whole diameter you're seeing there, is that all the Earth or is that twice what the Earth is?

PAGE No, it's just the Earth.

QUERY That whole thing you're seeing is the Earth?

PAGE Right.

QUERY Not twice the image?

PAGE No, as I said, there's a little halation that makes it a little bigger than it ought to be.

QUERY Only 2 percent.

PAGE Yes, 2 percent. Now, on this one, it's exactly the same view as taken after John so actively pointed the camera at the Earth. The Earth is in the middle there. If you look

PAGE (CONT'D) real hard, you can see the dark side off to your left. This is the geocorona, the streaks are an instrumental matter. Actually, we have a barrier membrane that George put very close in front of the film to keep visible light from getting in there, and the barrier membrane wasn't quite uniform and that's where the streaks come from. The circular thing on the right is an overlap, another defect. The little motor that advanced the film between exposures didn't pull it quite far enough for this one, and the dust specks are my pipe tobacco on the slides.

YOUNG Thornton, that rascally thing was moving. At first, it wasn't moving. The wheels weren't going as far as they did toward the end.

PAGE Yes. No, we had the complete transcript of everything you said about it and, in looking at the film, which I did in very great detail, you can see the most serious defect in that film advance was during your short exposures on the Earth, John. Everywhere else it worked fine. I have an idea that pushing the button so frequently sort of confused the motor and it didn't turn as far as it should have. In any case, you'll notice the shape of the geocorona is as predicted by Dr. Myer [?] at the Naval Research Lab; it's got a dimple in the back, down-Sun. The Sun is still at

PAGE (CONT'D)

the right here. Our other photographs show that it extends at least twice as far as you see here off to the right. The print, of course, can be printed dark or light. If they had printed this one lighter, I'd have got the background all over the whole slide and you wouldn't have been able to see as pretty a picture as this.

The next slide shows the one farther to the right. The Sun is still to your right, the Earth now is off the edge of the picture to the left. This was one of the sequences taken through the night between EVAs 2 and 3. You see the star background here. And again the geocorona is those streaks that are not real which are extending actually right across this frame if you print it lighter. The next slide shows the two of these combined, and it printed a lot darker and is not as artistic an effort as I had hoped. The geocorona is hydrogen in 1216 angstroms Lyman-alpha. The next slide shows the spectrum actually taken on the first slide. The dispersion is vertical and that white band across, mostly horizontally, is Lyman-alpha. You see how strong it is. The Earth is in the middle of this picture and the spectrum of the upper atmosphere is right out on that vertical line from far ultraviolet down at the bottom to nearer ultraviolet up at the top. The bright line

PAGE (CONT'D) just above Lyman-alpha is 1304, which is the oxygen line. George, do you want to describe this in a little more detail?

CARRUTHERS So far, we've made only a very crude analysis of the spectrum by comparison to laboratory spectra made in the preflight calibrations. However, we have tentatively identified the 584 line of helium, the 834 line of ionized oxygen, and the 1026 line of Lyman-beta of hydrogen. These three lines are the first spectral measurements in the Earth's upper atmosphere. All previous measurements have been limited to wavelengths longer than 1100 angstroms, which we also cover and which include the Lyman-alpha line at 1216, the 1304 to 1356 lines of atomic oxygen, and the Lyman-verge [?] hot field bands of molecular nitrogen between 1200 and 1600 angstroms. The Lyman-alpha line, of course, is by far the strongest emission that we have seen in any of our spectra, and it is the only one that we have conclusively identified in any of the spectra that do not include the Earth. However, by comparison of spectra taken with and without our lithium chloride corrector plate, which cuts off at 1050 angstroms, we will be able to determine whether we see a general background in the 584 line of helium and the 1026 line of atomic hydrogen. As we expect, though, they will be much weaker than the Lyman-alpha line.

PAGE

George, you have that other spectrum coming. Can you hold on a minute? I think the order you've got them in has the Magellanic clouds - Can we have the next slide? This is without the corrector plate. You notice that the Lyman-alpha here is broader, the definition is poor, but, as George was just saying, we get lines further down the ultraviolet on spectra taken without the corrector plate than with it. You see here, too, these other lines are not uniform. This is a spectrum that was obtained with the Earth off the edge here. It shows that the geocorona goes right across this slide as I said last time. By the way, that's a hundred thousand miles. And the other lines here show that these are other materials in the geocorona or beyond the geocorona; I guess it's going to take a little while to figure out which is which.

Well, John was worried about what LMC meant and this is the large Magellanic clouds and that's what the initials refer to. The picture on the left is with hydrogen with Lyman-alpha; the picture on the right is without. In this far ultraviolet region, all that you are seeing here are the very hot blue stars and over here you see the clouds with hydrogen gas. The difference between these photographs

PAGE (CONT'D) is hydrogen. The Magellanic clouds are nearby galaxies so we have a galaxy with all the stars and nebulas, whatnot, spread out in front of us to study. ... has made special studies of these Magellanic clouds and is very much interested and already has copies of all these photographs to study. You notice that hydrogen is not just from a Magellanic cloud, the stuff up here is out in the open sky and just what caused that background, I don't know.

We had a couple of questions for John. One was that sticking which had us worried. The instrument may fly on Apollo 17. ... is very interested in it. What is the sticking? Did it continue right through - you didn't mention it during EVA-3.

YOUNG It got worse all along. I just got the feeling that there was some kind of hangup, possibly in long-term vacuum exposure or something, to the operation of the way it was working in azimuth and I was never able to -

PAGE It never got better?

YOUNG No, it never got better; in fact, the longer it sat there - maybe the cold - it just seemed to get stickier and stickier. Toward the end there, every time I got to a new setting in azimuth, it would completely destroy the level. Unfortunately, we were on that slope right there under the footpad of the LM. We had to go back and relevel it every time. We really were working at the limits of the level-ability of the machinery, I think, there.

PAGE You see that it didn't make much difference. For instance, that Magellanic could should have been out in the middle.

YOUNG I know, I know.

PAGE You were off by that much.

YOUNG If we were on a level slope, it would have been no problem getting that bubble in the middle every time; and if the azimuth would have worked easily, it would have remained level. But I sure don't know what it was.

PAGE It couldn't have been dust that got in.

YOUNG No.

PAGE We thought that was impossible.

YOUNG No, I don't think we had any dust. In fact, I'm sure we didn't have any dust on the legs, up that far.

PAGE The film cassette you brought back has very little dust on it. In fact, it looks brand new.

YOUNG If it got dust on it, it got it inside the LM. There was plenty in there once we got to zero gravity. Although I don't know how it'd crawl through two bags.

PAGE That's why I was quite surprised there was no dust, but I was a little disappointed because we were going to collect a little bit of dust off it. (Laughter)

CHAIRMAN Any questions from the floor?

YOUNG I'm sure it got some dust on it when I removed it, though.

PAGE I got a little, a very small amount. Which I'm illegally keeping in my desk. (Laughter)

YOUNG Don't tell anybody, the FBI will be around to see you.

QUERY Did you see evidence of the magneto sheets or ...?

SPEAKER So far we have not seen any discontinuity in the up-Sun geocorona. It just sort of dribbles off the ... to go sunward from the Earth and there's no sharp drop.

PAGE But we're really not out as far as the ...

SPEAKER Ten radii should have been in the middle of that shot.

PAGE I get the point. Is it only 10?

SPEAKER Twelve, sometimes.

LUNAR GEOLOGY INVESTIGATION (S-059)

CHAIRMAN The next subject is the Lunar Geology Investigation. Dr. Muehlberger.

MUEHLBERGER For the benefit of the astrologer in the crowd, there has been little evidence for water and none so far for geysers. Our experiment may not be as far reaching as theirs but on the other hand, I think we have made a large step toward understanding the history of the Moon and therefore the Earth and therefore the solar system. And if you guys can find anymore, we're out there with you. So far, we've completed the mission itself, and all major objectives for the preplanned traverses were reached, sampled, described, and

MUEHLBERGER (CONT'D) photographed. As far as we can tell with our quick look at the photographs, all of these were done well. We've produced a so-called "green book" at the end of the mission operations itself, which is a summary of our knowledge at that time. It was based on crew observations, TV, and the premission data. Included in that thing are station sample maps which were derived from the TV in real time, and that was a very useful tool to us; a sample inventory; a film usage inventory; and station locations, again resected from the TV pans. By the end of last week, we'd received black-and-white two-time enlargements of all film. These are the typical drugstore-quality prints, and sooner or later we'll get good quality, so our analysis is very brief. We have assembled all the panoramas. Sample location studies are nearing completion. The film inventory is also nearing completion. The geological analysis is still in its infancy and I suspect should progress rapidly now that we've got all this inventory done. You guys took so many pictures, it takes time to get them organized. We've had one session with a crew in the LRL with samples. Most of our questions need samples in hand and those, I'm sure, we will be able to discuss later. So, the questions I've got relate more to surface observations that maybe we can amplify. Some of these are questions that we've asked you

MUEHLBERGER (CONT'D) before, but I have a few polaroid prints of these photos that maybe can jostle our minds and assist in refining some of the answers that we've gotten on the debriefs after the EVAs or on transearth.

Our photogeologic mapping group primarily talked about rays by their whiteness. There were a few spots where they thought they were seeing dark rays. You recognize both light- and dark-colored rays; particularly, the South Ray pictures. If I could have slide 1, please, which is the South Ray panorama and the print I've just given to the crew is a single frame from that. There I think we can see light and dark. The real question I'm after is what lead you to say that you were on or off a ray and, secondly, were there any visible differences in the rock types as you drove by or had a chance of sampling ray material? This is your pan from station 4.

YOUNG You see that black line coming over the - This photo really doesn't do that some justice.

MUEHLBERGER No. Polaroid prints never do.

YOUNG I mean, you really have to see it to believe it. That white is so much whiter than the contrast that we are able to get out of our photographs. I think when I looked at it, my assessment was it was a black ray coming out.

MUEHLBERGER The right rim there on the photograph.

YOUNG Yes, and it probably went right down that - the photograph that showed the black area coming out of South Ray. There's a series of black blocks. There's a bunch right there.

MUEHLBERGER Left edge.

YOUNG Charlie described this in real time. There's some more black blocks in there. They seem to come out in ray patterns, too. Don't you think, Charlie? It's one spectacular crater. That ray coming down the south right edge might have been the ray - or at least part of it - that made Survey Ridge. Because it just ran right across those Wreck and Trap Craters. We could see it going all the way down.

MUEHLBERGER Is that the bright ray you're talking about in the lower right?

DUKE This ray right here, Bill, came right through Stubby, right up over and right out Survey Ridge, this one right here.

MUEHLBERGER So that was the source of your terrific block field, there?

YOUNG Man, this is amazing how much rocks it laid down. Just couldn't believe it. ... close to that thing would have probably been very interesting. I wish we had had another EVA to find out.

DUKE The reason we called we were on a ray was really an abundance of secondary craters and an abundance of blocks. And they thinned and thickened as we, I think, traversed rays, but we never did get totally out of blocks on the traverse to the south. At least 3 to 5 percent of the surface had cobbles of 20-centimeter blocks and larger.

MUEHLBERGER And the blocks were generally asymmetrical enough to the crater or the crater shape was elongated or just a great spattering of craters is why you're calling them secondaries.

YOUNG I think, subjectively, we felt they were oriented from South Ray all the time. It was really just a subjective feeling as we were driving by them.

DUKE There were a lot of the blocks that were not associated with any craters. There would be a series of secondaries but around for a couple hundred meters, there would be more blocks. In fact, as far as we could see, there'd be blocks just scattered over the surface that were not associated with any secondaries. We did see some secondaries that were classic in which the ejecta was down range from and pointed right to South Ray. Station 4, for instance, was one.

MUEHLBERGER Can I have slide 4, please?

DUKE I might add that these black rays and the black blocks that you see here were also evident at Baby Ray. And to a lesser degree, the sampling at North Ray had an appearance of being black and white rocks also.

MUEHLBERGER The North Ray rocks?

DUKE Yes.

QUERY Do these black streaks go into the crater themselves, vertically down into the sides of the crater?

DUKE At South Ray, yes. The one on the left that you saw there, you could track it over the rim and back right across the rim.

QUERY Ken, could you see that from orbit? You can see a lot of these fresh, looked like black-streaked craters.

MATTINGLY South Ray does not have nearly the obvious dark streaks that run down inside and outside that many of the other craters do. The dark-appearing material was very obvious in the crater interior, but I never recognized it as being a ray that was thrown out with a radial dimension.

YOUNG I think, looking at the photography, the feeling we got from looking at South Ray was that that black streak was just an absence of any rays. But I think from the blocks, it must be black ray material.

MUEHLBERGER Maybe the dark matrix rocks would produce that darkening?

DUKE It petered out a lot faster than the white rays, though. Within a quarter of a crater diameter.

YOUNG I think it was from the bottom, though.

MUEHLBERGER Those could disappear just because of their general appearance?

YOUNG Could.

MUEHLBERGER Like the local regolith, the white ones being so obviously different. They'd be easy to recognize. I suspect that's why the mappers did the same thing. Here's your pan, John, or part of it, looking up-Sun, and this crater you suggested was a secondary. Therefore, the sampling that you were doing primarily over there to the left corner where the Rover is would be South Ray ejecta as the principal source and is one reason we wanted to move you on. I think that was a good decision now that we look at your photographs and from what you had told us.

YOUNG We sampled this crater up on this close-in rim and down the rim a little bit. But if that's not a classic secondary, I never saw one.

MUEHLBERGER That's type locality, if you will pardon the geological verbiage.

DUKE You can see the debris from there is scattered all out in here, too. The Rover's back over here. Downslope.

MUEHLBERGER You can see it right in the left corner, just the edge of it, and I think you are standing there, Charlie. Related to this are some of the craters that you called indurated and that had clods around them. Photo 7, please. That's Plum. Plum's got a little bench in it. In the premission work, there were several of them that had benches and suggested that there was an indurated layer that was shallow compared to what the crater count said that the age of that whole surface had to be. We just heard Kovach tell us that there is a very thick regolith here, which is what it should have had by the cratering stories, and yet there is always this little bench. We are wondering now whether that bench has some relationship to the ejecta from the South Ray and North Ray Craters and, therefore, might be

MUEHLBERGER (CONT'D) what you brought up here on the rim of Plum as some of the preray indurated regolith. Do you have any thoughts that could go into that?

DUKE That wasn't what I was calling the indurated regolith, Bill. The indurated regolith I was talking about was 2-meter-sized craters. Two-meter size and there were very small clods, no bigger than a grapefruit, that were symmetrically around a very shallow crater that had a hackly black glass right in the center of these little craters. The biggest one was no more than 2 meters.

YOUNG You see those outlines of rocks down in Plum, there. I wouldn't be surprised but what the most likely candidate for the rock that was from down either in the bottom of Plum or from the lower part of Flag was that piece of rock that we chipped off of.

DUKE It's located right in this area here.

SPEAKER Out of view of this picture.

YOUNG It was submerged rock and I got the feeling that it was sort of local to the area. I mean I don't see how it could have been from South Ray unless that was extremely soft when it plunked in there. And I'd rule out that, but that would be the most likely candidate of the ones we could get to. There was no way we could have got down in there and got those rocks. And gotten back. (Laughter)

MUEHLBERGER Since they took your tether away from you, I don't imagine you want to explore those craters. The Buster Crater; photo 8, please. This is your partial pan of Buster. You described that there was a southwest-to-northeast boulder field in here. We're looking almost down-Sun and I'm wondering (I can't tell in these photographs) could you tell whether there are blocks over in there and your impression of this streak across it. Could it not be salted [?] because you are looking down-Sun?

DUKE I really didn't mean to say southeast-northwest. The predominance of blocks in Buster were oriented in this direction, across out this way, which is northeast-southwest.

MUEHLBERGER Yes. And that's what you said. And you're reconfirming that.

DUKE I reconfirm that. Over in this area, you can see some of the blocks, but not nearly as many as here. We can barely see the bottom of Buster, but the bottom of Buster is covered with blocks that were up to 2 meters across and showed no orientation in any direction. My feeling over in here, there were blocks in this area but they were not as numerous as this pattern in here.

MUEHLBERGER So the blocks that were on the side you're standing on would have been derived primarily from that hole and not from the impacting material. It has the appearance of a secondary from South Ray in this kind of a photograph and your description of it. You do not go with that kind of interpretation?

DUKE Well, if it is, it's the biggest one we saw. That is a big crater. From here to here, it was at least 50 meters.

YOUNG At least, yes.

DUKE From the size of those blocks in the depth of that crater - and South Ray is 6 kilometers away - that had to be one big block that pounded in there, and I don't know if you could even excavate something that large.

MUEHLBERGER Much too large to be secondary.

DUKE I don't really think it's a secondary.

YOUNG My feeling looking at it was it was a primary because I couldn't conceive how you could get one from - Of course, it could have maybe been a secondary from one of those big craters way down south; that is a possibility.

MUEHLBERGER So that's either local bedrock or some of the flat or thrown-out material from Spook that we are seeing there in the floor and in the walls.

DUKE Spook didn't look like that at all. This was a lot fresher crater than Spook.

YOUNG I'd hate to oversimplify this business. There is no telling where that rascal came from.

MUEHLBERGER I'd like to have slide 9, please. This is North Ray Crater. There were arguments among you guys and there are arguments among ours as to whether we are seeing layering in there. I think the layering that you are talking about, John, is over in the right part where the dominance of blocks is, is that a true statement?

YOUNG Right there.

MUEHLBERGER Yes, that is certainly one of the best candidates.

YOUNG Yeah, it's just hard to imagine how the blocks would either all slump down there and end up in that spot or slump down there and end up there. You know, in our geology trips, we ran across a lot of contacts that were a great deal more subtle than that one right there. And that's why I picked it.

DUKE There is a better view of that as you get on further to the north, Bill. I think it shows it a little bit deeper down.

YOUNG There was one other crater that we were looking into as we were driving up Stone Mountain where we could see what looked to be more like an outcrop than anything I've seen so far in that it was the same kind of thing only it wasn't broken up like that, it was just one solid piece. I don't think there was any way we could have gotten up the hill to it. We got some photographs of that; it shows up on the 16-millimeter photography as we were driving up there and I can point it out to you. It was about one-third of the way down from the top to the floor. But that's the only two places that I'd really hazard a guess that were outcrop.

MUEHLBERGER That was outcrop. Off to the left there, you can see a vertical string of blocks, right in the middle, the far wall. Were there any visual observations that you made of that?

DUKE Again, I got the impression we had white areas and dark areas. Maybe the dark in it was just because of the shadows caused by the rocks.

YOUNG I got the feeling those were the dark blocks like House Rock, just like Charlie said, and I got the feeling there were places where there were strings of white blocks, too.

DUKE These rocks here that are more buried, and the regolith is deeper in this area, are all white matrix rock. House Rock sits here and that is predominantly black matrix rock. And I counted nine radial block trails out of that crater. That gave you the impression that you could track them from the floor all the way out over the rim, at least as far as we could see. Distinct zones of blocks.

YOUNG The actual craters don't really exhibit the classic overturned flat that we've been looking at. They are more complicated than that when they all get shuffled around on themselves.

DUKE In here, we had the impression that the regolith was more loosely consolidated because of a footprint impression I had. Down at House Rock, we already commented, you couldn't even get the tongs or the scoop in, and around the Rover, which was sitting right back out here, we had the same problem.

YOUNG You don't get the impression from here, but the slope going down to that block where we had to go down to get a picture of the bottom of the crater was not the kind of thing I'd want Charlie to be doing without having that 100-foot line on us, so we didn't get any pictures of the bottom of it.

MUEHLBERGER We'll wait until we get the pan camera stuff and look into the bottom I guess. Or Ken's words on it.

YOUNG I wouldn't mind him going down there, but I'd like to be able to get him back.

DUKE Yes, me, too. (Laughter) As you can see, the white matrix rocks had more fillet and appeared to be more covered with regolith than the larger rocks over to the north where the black matrix rocks, the House Rock was. It had some filleting, but it was not nearly so pronounced as this area up to the south.

QUERY ... is the layer you're looking at just below the surface?

YOUNG I guess it's a quarter of the way down to the bottom. What's really hard to tell, you know that is half a mile across, so some of those rocks sitting in there must almost be as big as House Rock.

DUKE It's kind of hard to say. We didn't ever see the bottom, and it's about a quarter of the way down from where we could see. I would imagine that the bottom from the North Ray looks very much like the bottom of Buster with bigger blocks. Hopefully, the pan camera will show that. You had a feeling you didn't want to get close to that beauty. There was a bench down in here that you could probably have walked out on and seen the bottom, but if you'd fallen off, it would have spoiled your whole day. (Laughter)

MUEHLBERGER Besides that, you would get the flight controllers mad because you didn't get back to the LM on time. Okay, slide 13, please.

QUERY It almost looks on the far wall there that there are some boulder tracks coming down the side. Could you distinguish any distinct tracks left by rolling stones?

DUKE Not visually. There might be some in the 500. That was one impression I didn't get, that we had any boulder tracks.

MUEHLBERGER In a brief look at the photos, I don't think so, Jim. You see some little slumping bits, but no really good tracks. Are there any other questions on North Ray from the group before we move out on the ejecta blanket?

SPEAKER I'm almost sure that ledge is the same ledge that we saw in the 500 millimeter.

MUEHLBERGER Yes, on the premission stuff. Yes, I think you're right, which is, therefore, about a third of the way down into the crater.

QUERY ... speculate on the origin of the dark rays?

MUEHLBERGER No. They're dark rocks. The crew has told you that black matrix rocks apparently are the ones that are doing this dark ray. Charlie was just describing to you the sequence of blocks. Maybe we ought to look at this terrific TV pan. Slide 11, please.

QUERY Jim, should we be able to maintain a steep slope like that with a lot of loose material on it, unless there is bedrock very close to the underneath?

SPEAKER It's possible.

MUEHLBERGER Talus goes up to 30 degrees.

QUERY How steep, I mean? You haven't had a chance to measure it?

MUEHLBERGER Oh, that's too small a scale.

SPEAKER ... 40 degrees.

MUEHLBERGER This is just to the right of the last one, but Charlie was pointing out a series of blocks that were dark and these are the ones. The great big thing that's dark is the shadowed side of House Rock. And that's, by any description, a big mother rock.

DUKE These blocks that you see back over here are all the way over on the flank of Smoky Mountain.

MUEHLBERGER Yes, and they also have a dark albedo from the orbital photographs of previous missions.

SPEAKER There's a crewman; see him right there.

MUEHLBERGER There's only one light rock in there, and that's Frog Rock. That's a whole string of darks ones. On the premission plan, we thought that was right out of the bottom of the

MUEHLBERGER (CONT'D) craters; the blocks in the floor of the craters are dark in their albedo. That was one point of having station 12 plotted right at that block, to get a sample at the very bottom of the crater. You guys have brought us back a whole pile of them.

QUERY Jim, how tall is that crater?

CHAIRMAN That block?

QUERY How big is that rock?

MUEHLBERGER Well, there's an astronaut in the right corner. He stands there a little bitty - -

QUERY Right in here?

MUEHLBERGER Yes, one of you guys was just barely off of that white part of it. That's half an astronaut. (Laughter) The other half is in defilade.

MUEHLBERGER That's a big rock, 20 meters, roughly, by - -

DUKE I said 20 meters in here.

MUEHLBERGER Yes.

QUERY Have you speculated on the origin of North Ray as the secondary from the ...?

MUEHLBERGER No, that's a giant impact from somewhere. The House Rock should be derived from the bottom layer visible in the floor of North Ray Crater. I'd like to go to slide 13, please. At Shadow Rock, the TV wasn't quite on you. Therefore, to try to figure out whether that's a 1-lunar-day shadow, we would like to have you guess for us where, with respect to where you're standing, you scooped that soil. That'll give us a little refinement on that.

DUKE Okay, it's way back up under there.

MUEHLBERGER To this side of that little projection then? Maybe you can spot it a little better on this print than on that Polaroid.

YOUNG No, there was a hole under there like a gopher hole where Charlie ditched in his shovel.

DUKE Way back up under here, see this little - -

YOUNG Yes, it has two spurs.

DUKE Yes. Okay. That's right.

YOUNG That's right, right in here.

MUEHLBERGER The pictures you took in here show the rock fine, but they don't show that absolute black of the soil below. I think we'd like to know.

DUKE I put my hand about right in here and leaned in as far as I could with the scoop and reached back to the base of the rock, which is, I'd say 2 - a meter back up in there. I got it right up next to the rock.

YOUNG I would think that because that rock was on a slope that went back up that way, that it's a good candidate for a permanently shadowed sample. When the Sun got low enough where it could shine down in that gopher hole, it was probably behind that slope.

MUEHLBERGER That's something we wanted to get a fairly accurate spot on so the guys with their Sun-angle stuff can turn that shadow around and know whether it really did stay.

DUKE Well, this was a big overhang, Bill, and it was above my head, as you can see. I had to bend over and put my hand into here and then reach up under, and the Sun comes across more to the north - -

MUEHLBERGER On the other side?

DUKE Yes, on the other side, up in here.

MUEHLBERGER It looks like a good shot, but the guys that will be doing the experiments on it would like the guess as to whether it's single day or forever. Great. Now, unfortunately, the film counting ran out and you didn't get any closeups that we can find of the drill holes on this rock, that you talked about.

DUKE Well, there's one here, there's one there. There are some down in here. There's one. They were all on this side of the rock, which is the east side, and they were circular.

YOUNG South side, Charlie.

DUKE They went into the rock and looked just like drill holes to me.

MUEHLBERGER Just went straight in, roughly normal to the surface?

DUKE Straight in, yes.

MUEHLBERGER So, that gives them a consistent orientation. That's interesting. It makes them sort of parallel to the layering, which you can see there dipping off to the right at about 30 degrees.

YOUNG Yes; you get the feeling they're like those you find in volcanos, where gas comes out through a single hole.

DUKE But they weren't vesicular in the hole. It was an open hole.

MUEHLBERGER No visible lining to the hole, like some of the rocks.

DUKE No. They looked just like a drill hole.

MUEHLBERGER Okay, in other words, they aren't irregular, they're just straight-as-an-arrow drill holes. Now, the photographic procedures we had set up premission for studying lineations didn't work too well because of the three-rev delay and then sleeping first. But, we've got some material that resembles earlier things. I'd like to show you one slide. Slide 14, please. This was from Apollo 15, you have seen that one before. Now I'll show you the next slide, 15, which has the same type of lines, and then slide 16, which is just to the right of this. That's Crown that just disappeared, and Crown is at the very left edge of this. At the time, you were describing these as resembling the ones from the 15 mission, and you said you couldn't really see them on the ground. The impression you get here is that you'd have a

MUEHLBERGER (CONT'D) major undulation that you'd be climbing up and I would gather that. You want to try amplifying any of these thoughts that you had? This is to the right of where you traversed up the mountain. Anything you could add beyond what you remembered or have already said in the transcript?

YOUNG I guess the major ones appeared to us as benches, and when we got in them, we just didn't have a feel for the little bitty ones at all, or I didn't.

DUKE My impression was then, and still is, that it is topographic in nature. The slight changes in the angle that the Sun shines on those particles cause those lineations. Why they are so continuous, I don't know. I don't know whether you can actually take one and track it all the way across the picture. That might be an interesting task for someone to do.

MUEHLBERGER I've tried it and you can't.

YOUNG Well, when you get to standing right in the middle of them - -

DUKE You can't see them except out at Flag Crater in that loosely compacted regolith. When we first got off, I think I took

DUKE (CONT'D) a closeup with a 70 millimeter, and there were thin pencil-like lineations that were almost cross-Sun, maybe 30 degrees to the Sun. So that'd make it northwest-southeast. The lineations were randomly oriented and only a couple of inches long. I got the feeling that it was all small scale due to the particles, the way the regolith was sitting in there loosely - -

MUEHLBERGER Just little hillocks that would make a pencil shadow?

DUKE - - in little hillocks. It was on these particles that the Sun was casting a shadow that caused those little fine lineations.

MUEHLBERGER Do you mean rather than randomly oriented, randomly spaced, on the surface in that area?

DUKE Yes, randomly spaced, and they were mostly oriented 30 degrees away from the Sun line, in the long dimension.

MUEHLBERGER Good.

DUKE Ken might amplify something. From orbit, in just about every crater that I looked into before we manned the LM, you could also see these lineations.

MUEHLBERGER That were going diagonally into the craters, would that be the way they were?

MATTINGLY I don't think you could say that they were diagonally.

DUKE No.

MATTINGLY I think the thing you can say is that if you ever found a place in the Moon that didn't have these lineations, you'd find a very unique place. But they aren't necessarily diagonal, and they do reflect surface expressions of other topographic features. They bend around small craters and big crater walls and things like that. Whether we are seeing always the same thing or whether there's several things that contribute to this effect, I can't sort that out.

QUERY Could you see them from 1600 miles without the binoculars?

MATTINGLY I would say no. I really don't remember offhand, but I think not, because it was so dramatic to me when we came down in the parking orbit, as we got closer to the surface, they started popping out and they were very significant at 10 to 8 miles range. However, I remember seeing them always, and I think it must have been using the binoculars that showed them up.

MUEHLBERGER By "always," do you mean the low Sun-angle parts of your orbits or even at high Sun azimuth?

MATTINGLY Always.

MUEHLBERGER Always.

MATTINGLY Like I say, if I ever found a place that didn't have that pattern, it would be very unique.

MUEHLBERGER Yes.

QUERY In a local area, is there much deviation of orientation?

DUKE In the Cayley area, where we were?

QUERY In the Cayley area, is there much deviation of orientation?

MATTINGLY In many of the crater walls, you could see reflections of other features. For instance, in Theophilus, which is a place I noticed it, as you look at what would have been the eastern wall, there are some craters that are around the rim of Theophilus. You could see places where these lineaments that were in the walls kind of bent around the surface expression. I don't know whether you could ever see any of that on this scale or not.

YOUNG I guess a lot of conclusions you reach from looking at photographs are very dependent on the Sun angle, and I guess everybody knows this. But it came as a surprise to me, before I went to bed that night, to look down at Stone Mountain and see what appeared to be a giant lava flow going down into Stubby Crater. When I got up the next morning, it was the shadow that had produced that appearance of the really dark material flowing down into Stubby Crater. It turned out to be clearly just a big lineament of debris, or a lineament-like appearing debris pattern going down in there. I think maybe a lot of conclusions everybody's made from some of the orbital photographs are very dependent on Sun angle, and you have to be very careful.

MUEHLBERGER Could that be the so-called slide that was mapped?

YOUNG Yes. Yes.

MUEHLBERGER It was certainly a very distinct escarpment just beyond it on your surface LM pictures.

YOUNG Yes.

MUEHLBERGER It looks like the margin of a slide or a flow or whatever you want to call it.

YOUNG It was just that that was the way the flow lines appeared to be going. When they were really dark like that, they looked like stuff right out of Hawaii.

MUEHLBERGER Well, the premission pictures were about 60 degrees Sun angle, and you were seeing something different there. They thought they were seeing something. Maybe it's just a topographic expression of what produced your Sun line.

YOUNG I think so.

MUEHLBERGER Great. I have no further questions to ask now.

QUERY Does the general orientation of the lineament seen here differ considerably from the orientation of Apollo 14?

DUKE I can't answer that. On Stone Mountain, the lineament seemed to start towards the Kant Plateau and climbed up the mountain along the northwestern face and the northern face of the mountain and disappeared over behind South Ray Crater. So these were mostly northeast-southwest types, at least these that you just saw. What the other orientation was, I don't know.

MUEHLBERGER Are you talking about a little surface near field 1 that you see in the regolith?

SPEAKER On ... surfaces, they're on it the same way ...

DUKE Oh.

MUEHLBERGER Here they've gone to about the same Sun azimuth and elevations. When you look at them ... you don't have much chance.

DUKE You mean those little doodlebug-hole lineaments that they saw in the photographs on 14, is that what you're talking about?

MUEHLBERGER Yes, I believe that's what you're talking about, isn't it?

YOUNG Little bitty ones that just went - -

MUEHLBERGER Yes, little short streaks.

YOUNG - - maybe for a hundred meters, right across the surface in a straight line.

MUEHLBERGER Or more like a ... meters.

YOUNG Look at them in stereo and they look like maybe a fracture, maybe where the dirt has gone back in.

MUEHLBERGER Yes. Is that the type you're talking about?

DUKE I was looking for those things, and I don't remember seeing any.

MUEHLBERGER We haven't had a chance to really take a look at the photographs to spot those.

SPEAKER This is also higher Sun so maybe ...

YOUNG Yes, you might not see them in high Sun.

MUEHLBERGER Yes, they'll fade away in the Sun. I'd like to thank the crew. You guys did a great job. This is what we expected, of course.

QUERY Bill, do you have any words on what the crew found compared to what you had supposed would have been there? I mean, you had some - -

SPEAKER Well, we certainly didn't expect all this breccia.

YOUNG If that vesicular basalt is around, it's probably in a rake sample, because we never were able to grab one outright. We got a vesicular breccia but that's not really good enough.

SPEAKER No.

YOUNG That doesn't do it.

QUERY What are you going to do with ... rock?

MUEHLBERGER How about bury it? I'll drop it on one astrologer's head. (Laughter)

QUERY During EVA-2, at station 8, you saw a huge boulder, approximately 2 meters across. In the attempt to move it ... higher ... a little farther away from the Rover, ... rocks approximately a half meter across and you thought you could move it and you kept trying to push it and you couldn't do anything; you said, not a chance. If you say this is an outcrop, do you think that was a big rock buried under it?

YOUNG Did we even - -

DUKE Yes, we sampled that.

MUEHLBERGER That's the second rock you went to.

DUKE That's to the east of the Rover, yes, the second we went to. We sampled that rock but we couldn't move it. My impression was that it looked like an iceberg.

YOUNG No, I don't think that was outcrop.

DUKE No, I don't think it was outcrop.

SPEAKER ... these rocks are buried.

YOUNG It was just a big rock, like the tip of the iceberg. I bet it wasn't outcrop, though.

MUEHLBERGER Yes, you know, most rocks sit with their one big side on the bottom.

QUERY ...?

MUEHLBERGER Could be only 2 inches down.

YOUNG We could move that first rock, but we'd never turn it over; it had too much bottom and not enough - -

MUEHLBERGER Handle to pull on.

YOUNG - - not enough handle.

DUKE That first one was sitting flat on the surface and you could feel it move. But the other one, you could not budge it.

YOUNG Fortunately, the closer we got to it, the bigger the base got.

MUEHLBERGER It suggests that the very planar bottom - -

DUKE I don't know; I wouldn't guess.

YOUNG It was in a flat bottom.

DUKE I couldn't guess, really.

CHAIRMAN Okay. We have to move on.

MATTINGLY I still would like to know what we're supposed to tell people when they ask us, "What was Descartes?" which they'll ask next week.

CHAIRMAN That's why I asked Bill, I guess you haven't gotten any big picture of what the Descartes region is.

MUEHLBERGER They're opening rocks at a painstaking rate, and the ones that have been opened so far are exactly as guys described them. I suspect you could run through the transcript and come up with 90 percent being breccias. The origin of the breccias is the problem that I don't think we're going to resolve for a while - at least the arguments on them. I think that is the question you're asking, Ken.

YOUNG Yes, those drill holes may provide us a clue. Well, maybe those things happen in big impacts, too. You can't really believe they do.

MUEHLBERGER If you partially melt a rock by an impact, you can get the vesiculation in it. But drill holes, yes, you're right, they're probably - -

YOUNG Sounds like a long-term settling of something.

QUERY Have you opened the bags from the station 1 sequence?

MUEHLBERGER Have they or not?

DUKE Yes, the rock box was opened and that's where station - -

MUEHLBERGER That was 5, 6, and 8 with the rock box.

DUKE Yes, I guess you're right. Wait a minute, we filled up a rock box on EVA-1.

MUEHLBERGER Yes, but they opened up the second rock box.

DUKE Oh, okay. Yes, that's the one with all the core tubes in it.

MUEHLBERGER It's still sitting there degassing and stuff like that.

SOIL MECHANICS (S-200)

CHAIRMAN Okay, thanks a lot, Bill. We've approximately 2 hours to go on the briefing. We should close at 1 o'clock, so keep that in mind as we keep on. Dr. Mitchell on Soil Mechanics. Jim.

MITCHELL I'd like to address several aspects of the experiments. First, looking at some of the things that are related to soil observations in general, I'll make a few comments of things that we've observed. And then look more specifically

MITCHELL (CONT'D) at the results of the penetration test. The descent and landing suggests that there was less dust kicked up than in previous Apollo landings. Our suspicion is that this does not reflect any fundamental difference in soil conditions but probably indicates that it was a result of the higher rate of descent. In using the callouts that Charlie was giving during the landing and timing them later with a stop watch, we figured that your descent rate was a bit faster than in the previous missions. Also, there was a higher Sun angle. If you were descending more rapidly, of course, there's less time to kick up dust and, also, it would mean that you had a lower thrust, which in turn would mean less dust. A preliminary look at photographs indicates very little if any footpad penetration. Perhaps we'll comment on that later. Similarly, a look at the ALSEP pan photography suggests little erosion underneath the LM, and we'd be interested in any comments relative to baking of the surface and striations caused by blowing dust and the like. Moving to the ALSEP area, several things lead us to the conclusion that the soil there was probably a little less dense than it was at the Apollo 15 ALSEP site. The bore-stem drilling was faster than on

MITCHELL (CONT'D) Apollo 15. It's our understanding that you had to hold back a little bit during the drilling to keep it from advancing too fast. Footprints are moderately deep. Further, the evidence now is that the drill stem did not completely fill during the sampling. All of those things are suggestive of a somewhat softer soil at the ALSEP site than in Apollo 15. An observation of considerable interest to us was the fact that the deep drill hole stayed open. And you dropped the rammer-jammer in, which is a depth of something on the order of 2.2 meters. This is particularly interesting to us because it, in part, compensates for the fact that the soil-mechanics trench was not excavated at the end of EVA-2 because of insufficient time. By virtue of the fact that the drill hole did stay open, it provides a basis for us to make the calculations of lower bound cohesion for the soil; it also provides some information on stiffness of the material because it didn't squeeze into the hole; and I think it's also of great interest to Don Burnette [?] for his neutron probe experiment that's being considered for 17. So that 1 minute or less expenditure of time to drop that thing in there was more than worth it. There was a photograph, I think, taken of the ALSEP thumper imprint, which I haven't seen yet. I think it was the last thumper print that was made. We intend

MITCHELL (CONT'D) to use this also for an analysis of the dynamic bearing capacity of the material surface.

SPEAKER It's a stereopair.

MITCHELL It's a stereopair. Great. It's going to be very useful. The core tubes again appeared to work well, as they did in Apollo 15. I think the estimate on samples by core tube was about twice as much material as was obtained on Apollo 15. We're interested in whether or not the core tube stayed open after you pulled the tube out of the ground. I haven't seen any photography yet.

DUKE We didn't get any. They appeared to be open, though. I don't know how deep.

YOUNG I would say at least the one on station 10 was open when we pulled it out.

MITCHELL As in all missions, there are many, many footprints to look at, and these will be analyzed statistically to see if the average porosity falls in line with what we've observed now from the other landing sites. We have looked in considerable detail at the near-surface porosity as evidenced by footprints and find that, interestingly enough, the porosity is averaging about 43.6 percent at every site. But there is considerable variation plus or

MITCHELL (CONT'D)

minus at any site. An interesting observation is that, on the average, it's everybody's ...

Now let's consider the penetrometer measurements. There were some 11 tests done. If I could have the first slide. At station 4, Charlie made four measurements. And one of those things we're hopeful that you can help us out with is locating these a little bit better.

This is a preliminary station map that was done by the LGE people and it's in the green-backed book that Bill Muehlberger was talking about a moment ago. The first measurement was with a half-square-inch cone penetrometer. The other three were with 0.2 square inch, and our locations are somewhat indefinite. We see pen one, two, and three. I don't know where four was, and perhaps we can get together for a couple of minutes later and try and pin those down a little more closely. We think, according to your transcript though, that the first was uphill and that the second and third were more or less on more level ground and the fourth was on a downhill location.

DUKE

Right.

MITCHELL

Station 10 was the site of an additional seven tests. Two of these were plate-load tests and five were done with the

MITCHELL (CONT'D) cone penetrometer. The depths of these ranged anywhere from about 9 inches up to 30 inches, which is the full capacity of the instrument. The next slide shows the location of these things at station 10. Without going through all the details, the important point here is that these measurements were made on a line between the double core tube here and the deep core done during EVA-1 over here. There's a string of four measurements in there, and there were two plate-load tests in this area plus an additional cone penetration test with a different penetrometer. Our data look very good. The penetrometer appears to have performed in an excellent manner throughout. It seems to have dutifully recorded both the soil conditions and Charlie's acrobatics as he did the measurements. If we could look at the next slide, please. These were just a couple of the pictures that were taken from the TV in real time, and that shows one of your penetration tests up at station 4, and the next one, please.

CHAIRMAN It looks like he's falling on a sword there.

MITCHELL This was the fourth measurement up at station 4, and there was considerable concern both then and later about what might have happened if he'd fallen on it, rather than beside it.

DUKE No sweat.

MITCHELL No sweat. Oh, okay.

YOUNG Orange juice would have plugged up the whole thing. (Laughter)

MITCHELL Well, we're not taking the data off the drum. This slide shows us a picture of what the drum looks like, if we may have it. There are lots of scratchings on there, but they make a surprising amount of sense as I can show you on some viewgraphs. We have taken these off approximately; we don't have any calculations made at this stage but they are of themselves revealing just to see the nature of the curve. This was the first test you made up at station 4 with a half-square-inch cone. Your observation at the time was that you thought you hit a rock. Based on this, I'm not sure, because that's a good steady penetration. You also indicated that you were concerned about the spikes. We're not too worried about those. You can see what they look like, it's the prick on loading and reloading, and it doesn't degrade the data at all. Here you reach the penetration depth of about 20 centimeters, and this thing reached its maximum capacity of 40 pounds or in newtons, 220-odd or whatever it is. But then you change to the 0.2 cone and did a test. I don't know

MITCHELL (CONT'D) whether it was adjacent or at another location; perhaps you can enlighten me on that. In any event, the soil was much, much softer as you can see. It went full depth under a load of 16 newtons - 15 pounds - something of that sort. So we were definitely looking at a very soft soil up there in the station 4 area. This is one of the traces from station 10. This was between the double core and the deep drill. In this particular instance, I think we are looking at a layered kind of a system where you had a firmer zone right here, a softer soil here, and a firmer material here where you bottomed at about the full capacity of the penetrometer. We have some 13 such traces to examine, and our next steps are to get the data off in more precise form and then to analyze these in terms of making some comparisons between Stone Mountain and station 10, to examine more carefully the layering and see how it relates to what is revealed in the core tubes and the deep drill, to study the variability, to develop a profile between the deep core and the double core tube, and to deduce some of the densities and strength parameters on a more quantitative basis, which we think we can do from these kinds of data. All in all, we're very pleased with what you were able to get for us, and we think it worked out very well. Here are a few things

MITCHELL (CONT'D) that we would like to know some more about before we are finished: the location of the penetration tests, the visibility during the landing, erosion under the exhaust, and any evidences of slope instability and layering that you might have encountered during the drilling. That's probably a pretty tall order, but any comments that you would have on any of those would be most welcome.

DUKE Okay, on the location, if I had a photograph that John took looking to the north, I could tell you exactly where they were.

MITCHELL I don't guess we have it. Did you bring it over, Dave, the station 4 pan?

DUKE There is one photograph where it's looking down right toward the Rover, and it has all four of them in there. One was just like the checklist said. I think we were within an old subdued crater, and there was a crater within the crater, and we were in the bottom of that and the Rover was parked. The first one was on the upslope of the subdued crater to the south. The next two were in the bottom just to the south of the Rover, spread by about 15, 10 meters, probably, on the flattest area there. The next was north of the Rover and on the steepest slope we had, which was on the outer rim of this old subdued crater.

MITCHELL Was this the small crater that the Rover was parked in or by?

DUKE Yes, it was about twice as long as the Rover in the east-west direction.

MITCHELL Is that the photo?

DUKE Yes. Okay, the first one was up in here and this is an upslope in here somewhere. One was over here, and one was over here, and the next one was down back behind the Rover in this area over here on the steepest slope down, and that's where the core tube was taken, too. Down over there.

MITCHELL Great. Thank you very much.

YOUNG Visibility during the lunar module landing. Dust started at about 80 feet and you could see all the way to the ground - all the way to engine contact and about a foot thereafter, which is where we shut the engine off.

MITCHELL So it was never obscured completely.

YOUNG No, never obscured. There is probably more than one reason. The last 100 feet or so of our descent, we were just flying over a little crater that was right behind us

YOUNG (CONT'D) and that might have been keeping some of the exhaust dust from going out in front of us. The erosion under the DPS exhaust will probably show up very much in the pan photographs that Charlie took around there. I don't remember there being any significant amount of erosion, certainly not as much as I expected.

DUKE There was a whitening of the regolith under the descent stage that gave it sort of a baked appearance, if that's the word you want us to use.

YOUNG I'm not so sure that that just wasn't that actual color of it because there was also whitening on the wall of that small crater behind us that we passed over.

DUKE It didn't look as loosely compacted under the descent stage as around.

QUERY Did it look a little bit scoured?

DUKE Yes. Yes. Radial outward just like everyone else has seen. I didn't see any evidence of any slope instability, unless that slump that John saw in the Stubby from off of Stone Mountain is slope instability.

QUERY Okay.

DUKE Larry, during drilling, when I started the second set on the deep core, I ran into a more resistant layer. The same layer was evident on the heat flow probe, the first one. It really started going in for about 30 or so centimeters, and then it stopped and then took maybe 10 centimeters and then it picked up again. I thought it was a rock since there were so many rocks lying around. But looking back, the second stem on the deep core had the same resistance at, I would estimate, basically the same level, but it was shortly penetrated and it went on in. Then there was no evidence of any layering from then on.

YOUNG My guess is that it would be in the lower half of the core at station 10, too.

DUKE Yes.

YOUNG I mean the double core.

DUKE The double core might have gotten the top of that layer.

MITCHELL Our penetration, as I showed you there, shows some layering.

CHAIRMAN Any other questions?

QUERY How do you put layering in the context with estimates of the regolith going down so deep. Is that typical to find layers in regolith?

SPEAKER This may be just a difference ... the region. ... find ... here within ... then you could have an ejecta layer that is a little different than the - -

SPEAKER That's not inconsistent.

MITCHELL No. You could have differentially compacted layers of varying thickness, and it appears now that this seems to be more the rule than the exception.

CHAIRMAN Okay, now we'll go into the inflight experiments.

YOUNG Now, let me say something before you get into that, and I know how bad everybody feels about the heat flow. Nobody feels any worse about it than I do. I certainly don't know what we can do at this time, except that, in the future, I think that these things ought to be made just a little more astronaut proof. In training, we broke every infernal wire that came out of the ALSEP and maybe in training we ought to put them up in the air and make guys jump over them, but all you are going to do is work harder and you're not going to avoid the problem. I think if - and this is hindsight - I think that we would have

YOUNG (CONT'D) saved that if there had been enough support on that cable so that you would have known when you touched it. You see, when you stand in one-sixth gravity, your feet are behind you. The only way you can see what your toes are doing is to make a conscious effort to do it. Also, you are sort of involved in limitations, and too much jumping around of central stations is going to kick dust all over it and all over the PSE. Maybe what we ought to do is put some of those strain reliefs on those cables and maybe the ones around the ALSEP ought to be designed so that they will lay flat, and if the astronaut inadvertently steps on them, he doesn't destroy what's certainly one of the most valuable experiments that we ever tried to put on the Moon. I know how bad Mark feels about it, but he doesn't feel any worse than we do.

QUERY How long are the ALSEPs going to last?

SPEAKER From 2 to 4 years after they place them.

INFLIGHT EXPERIMENTS

GAMMA RAY SPECTROMETER (S-160)

CHAIRMAN Gamma Ray Spectrometer. Dr. Howe.

HOWE As Jim says, we're getting now into a new group of experiments, the ones that were carried in orbit in the SIM bay. I want to quickly go through the major scientific conclusions.

I think you all know the experiment worked well. There were none of the minor annoyances we had on 15 with instrument peculiarities. We lost some time, of course, with the late landing and early departure and also some in transearth, but that was to a very large extent made up, at least some time was made up on every one of our objectives. We are very happy with what we're seeing. I think I can only give you the cream of the thing in the time available. I wanted to run through, for orientation, a few slides from 15, and then I have one viewgraph from 16, and we'll move on from there. If I can have the first slide, please. This shows the count rate in the broad-energy channel which responds to the radioactive elements - potassium, uranium and thorium - as observed from thrift

HOWE (CONT'D) data on Apollo 15 early. The low values correspond to the back side, except for a tip there right near the back end. There are very high values in the western area associated with Oceanus Procellarum and Mare Imbrium. We've tried to present these data in all sorts of ways, most of them totally invisible. Finally, we have found a method that seems to show up, and that's given in the next slide. This is to make the kind of relief map, as I think they call it in the geography books, only we're not plotting altitude; we're plotting radioactivity. Unfortunately, the latest slide that I have through the draftsmen (because they always keep bringing 16 data in before they finish the 15 stuff) isn't completely accurate, but there is a high spot near Aristarchus. This corresponds to a concentration on the order of KREEP. And then there's another one over here which shows in this gap for the data toward the eastern limb of Mare Imbrium. There's some radioactivity in the eastern maria; a little on the western edge over in the highlands probably represents mixing from the Oceanus Procellarum area. On Apollo 16, we had a considerably increased capability for real-time data reduction, and the results paid off. We also knew more about

HOWE (CONT'D) what we were looking for. This is a map again of the radioactivity of the traverse. Apollo 16, as you know, is a little more two-dimensional than 15, especially without the plane change, so a two-dimensional map gives you more of the picture. What we're seeing here in the western highlands over the same area that we traversed is the two cross points between Apollo 15 and 16: one over here around the Mare Orientale ejecta blanket, the other over east of Mare Smythii. The checks are excellent; that's very gratifying. We see in the region of the western maria, again, a very sharp rise as we're coming west. This begins over the landing site. You can just begin to see a curve at the landing site there. The highest values we reach are in a place which we recognized when we looked on the map. It's called Fra Mauro. (Except that we're not over the landing site itself, but at the lower end of that north-south-trending feature.) There's a kind of secondary shoulder or maximum over here, which according to one of my friends who has lots of imagination and intuition, is south of Kepler, whereas this region is south of Copernicus, of course, if one wants to consider ejecta as the cause of it. Over here in the eastern latitudes, this

HOWE (CONT'D) bump is probably rille down in here, but we're generally seeing throughout a low radioactivity characteristic of the highland regions. On Apollo 15, we have seen this high spot at the southern edge near Van de Graaff. We had thought that when we went over some of the big dry basins on this mission, we might see something similar. There is some structure, and it's possible that some of that will turn out to be slight increases over the dry basins, but that's the fine structure.

QUERY ... background ...?

HOWE No, this is the same kind of plot that we showed before. All we're doing for a simple first look is to take the total count rate in that channel and say that the lowest regions, as we can see from careful analysis of a few long spectra, have very little thorium and uranium in them, and they are baseline. The more refined analysis requires much more computing and much more time, and requires the prime data, which we don't have yet on 16. Now I'd like to summarize what we think we're seeing. I'd like to pass out to the people up here at the head table, with apologies to the rest of you because our draftsman will work overtime but he won't work Sundays, and so this is my own drafting, and that needs apologies, too. But I do have a contour map of the same sort for 16. This one shows both the 15

HOWE (CONT'D)

and the 16 data, and it's up to date. The major scientific conclusions I would make at this point would be these. First, now that we've seen two missions in which most of the radioactivity is concentrated on the western maria, we're inclined to believe that on that relief map, if we had the data everywhere, we could color the whole of Oceanus Procellarum and Mare Imbrium yellow, orange, and red. Now that's an extrapolation, of course, but it seems a reasonable one. Secondly, we do not see any region yet in either mission outside those maria which is as high as the lowest point within them. In other words, there seems a sharp separation of ... Third, there are interesting secondary maxima in our first crude look at the data. We missed these, but on more careful examination, it seems quite obvious that there is some radioactivity over in the eastern maria, that we are seeing mare-highland differences. Those of you who have the advantage of the map can see the thing getting blue over the Descartes region, and then getting green and light green again over Fecunditatis.

We are seeing finer green structure. We have claimed a resolution of 70 kilometers or so on the groundtrack. We certainly feel confident that we have that resolution.

As far as deeper interpretation is concerned, putting together what we know about the samples (particularly the soils) from the various landings that have taken place up

HOWE (CONT'D) to but not including 16, putting together the results of the X-ray and the gamma-ray experiments, the most plausible interpretation, at least as far as we are concerned, is this: we can get a long way in saying that the lunar soil, the regolith which we sample, which we observe, has three main components — two maria, if you will, and one highland. On the east, we have the material recovered in Apollo 11, which we can call loosely mare basalt, high in iron, high but variable titanium, and so on, relatively low in radioactivity. In the west, we have some of that material, too, presumably, but mixed into what Paul Gast calls KREEP. I like these acronyms very much because they don't conjure up all sorts of terrestrial geologic analogs, words like basalt, anorthositic gabbros, and so on do. So there are two basaltic compositions, the western one high in radioactivity, high in all sorts of other trace elements, rare earths, phosphorous, and so on. Then there's a highland component which has been given various names; John Wood turned up the anorthositic component after Apollo 11. The best acronym I've seen for that is one that Klaus Kyle [?] uses, ANT. The "AN" is for anorthosite, and I don't remember what the "T" is for, but again, it's not a loaded word. ANT seems to be high in aluminum and quite low in radioactivity. So we believe that those three compositions undoubtedly don't tell the whole story of the Moon, but

HOWE (CONT'D) the Moon may not be quite so complicated after all, if the first-order story of that kind can be told.

QUERY The viewgraph shows the radioactivity levels slightly higher than those that occurred on the back-side highlands. Is that true?

HOWE Yes, we believe so. We think we're seeing some curving up, and the thing we're interested in when the detailed exploration gets made, when the component mapping takes place, is whether there is higher radioactivity in the Descartes region. We stuck our necks out by saying that the typical soil over this area will be within 1 or 2 parts per billion thorium. Nothing like having a little fun with these things. The interesting question will be whether that is mare material mixed in, especially western mare material mixed in, which I'm inclined to guess from what I'm hearing about dark rocks and dark rays, or whether there might be an actual variation in the highland component. I would bet on the former at this stage. In other words, whatever you can recover as a highland component just might well turn out to be very similar to what you would find on the back-side highlands.

QUERY Is it true that ...?

HOWE Oh, no. No, when the RTG is present on the spacecraft, you gave us very nice calibration peaks, and, hence, while you were sweating out the waveoff, we were sweating it out, too, continuing to read these high gamma-ray peaks that were contributed from the RTG, even 30 feet away. But when the RTG goes, it stops. You might worry about a little delayed activation by neutrons or so on, but even the first spectra immediately after that are quite - -

QUERY Even when the LM's on the lunar surface?

HOWE Oh, you mean when the LM was on the lunar surface? No, 60 nautical miles is enough; 30 feet is not enough, but when you're that far away from the RTG, you don't sweat it.

QUERY I have one question on your 15 track, your back-side cal rate ...

HOWE Well, you've got to worry a great deal about whether I picked up old slides out of a box. One is directed for life and the other isn't. There's nothing fundamental there at all. There is a slight difference in the response of the 15 and 16 instruments. The 16 instrument is a few percent more efficient. You can't make two that are absolutely identical. But this is calibrated in the lab. If there aren't any other questions, I wanted to do, just once, a sort of NASA-type thing here at the end. You

HOWE (CONT'D) have presented us with various mementos of the mission, including I think it was the fifth day, when somebody sent around the Apollo 16 shoulder patch signed by Young, Duke, and Mattingly. The SIM Bay Group has a Barbara Trombka shoulder patch. Some of you remember Barbara from Apollo 15; she didn't make it on 16. She's Jack Trombka's daughter and she works the computer. She made us a bunch of shoulder patches for SIM science. I've only got one of them, but I think that Ken Mattingly is the guy that should get it, and if he'd like to walk over here.

MATTINGLY On behalf of all of us, thanks.

X-RAY SPECTROMETER (S-161) AND ALPHA PARTICLE SPECTROMETER (S-162)

CHAIRMAN Next in line is the X-ray Spectrometer. Dr. Adler.

ADLER I want to start by thanking the crew. Right after the first EVA when you people called down and wanted to know what the X-ray experiment said about the site, my stocks really went up in room 210, I'll tell you. So I promptly rushed up and began to beat on these poor kids we had brought with us that were processing the data. By the next morning, we were ready to give you a preliminary result, based on the first two orbits. As you might have expected, it turned out not to be completely representative

ADLER (CONT'D) because the subsequent values for the aluminum-to-silicon ratios in the Descartes area were higher. You said I have to keep in mind that until we know what the Sun was doing throughout the mission, it's going to be a little difficult, and so we have to do this orbit by orbit. And when we processed the first two orbits, we were not yet able to make a proper assessment of the backgrounds. Subsequently, as the first and only slide will show you, we were able to get more reasonable values.

To begin, let me say that the X-ray experiment worked very, very well again on 16, as it had on 15. Before we went home, we brought somebody with us from Washington who had already plotted something like 1350 data points on the map along the groundtrack, which was a sort of virtuoso performance right there, I think. On the basis of this, we were able to make some estimates of what the aluminum and silicon looked like. This one went from east to west along the groundtrack. We've done this for the aluminum silicon, and we've done this for the magnesium silicon. A number of things come through, which again agree very, very well with 15. For one thing, where we had overlapping groundtracks, we agreed to about plus or minus 10 percent, which I think is really very encouraging. The other thing, just as on 15, the maria areas tend to be low in aluminum silicon. Our present estimate of the aluminum in the highlands, if one

ADLER (CONT'D)

keeps in mind this is an important signature element, is that they're very much like the eastern limb highlands, as we see. For example, on the scale, the eastern limb highlands are about 1.55 and the area around Descartes is like 1.45. Although we don't show it here, towards the end, we were actually able to get some additional values which were west of the Descartes area into Nubium. The values then again begin to drop off on the order of about 0.8 or 0.9, which is again consistent with 15. The other thing that seems to come through as it did on 15 is that the aluminum and the magnesium vary in an inverse way. On 15, we had observed some sites where actually the aluminum went up and the magnesium went up. We find similar indications on some of the 16 groundtrack. We have discussed this, and this may be an important thing to look at, with respect to the albedo. It may turn out that this is reflected in decreasing ion concentration, but this will require further analysis.

We're presently beginning to look at the data in considerably more detail. As I say, we must look at what the Sun was doing. I think this time around, it was perhaps a little bit more erratic than it was on 15. We've had a look at it, and we're now going over the data and beginning to plot in greater detail. Just making a more detailed plot of the Descartes area is going to be very, very interesting since

ADLER (CONT'D) the geologists tell us it's not a completely uniform area in a geologic sense.

Now let me say a little bit about the astronomy. I talked to Dr. Bjorkholm yesterday. The results here are very, very interesting. The observations of Sco X-1 showed as much as a 30-percent variation during the measurement period. This has not been observed before. In fact, they tell me that right after the first sighting, there was as much as a 20- to 25-percent variation in as little as the first few minutes in looking at Sco X-1. It's also interesting, but it needs additional confirmation. Dr. Bjorkholm tells me that the first report they got from one of the observatories that was observing this and the radio frequencies simultaneously showed that this was also reflected in increased radio frequency activity. So this is going to be a very, very exciting thing to look at.

Finally, just to keep this brief, I'd like to say something about the alphas. In 16, the experiment worked considerably better than it did on 15. I don't know whether you were aware of it but on 15, there was a little bit of polonium contamination on a couple of the detectors, which had decayed off considerably by mission time but it was still a problem. On 16, the detectors were clean. Furthermore,

ADLER (CONT'D)

they were not bothered with a noisy detector, which tends to load down the electronics, as it did on 15. On this basis, there are some first indications of some enhanced polonium-210 in the Sea of Fertility. This can be fairly exciting because, as Dr. Gorenstein had pointed out on 15, it's perhaps a reflection of some sort of emanation on a very recent time scale. I suspect they want to do a more careful job of looking at these numbers as well. So that, in brief, summarizes the experiments. We are very, very grateful. We think things went off with considerable precision. On the X-ray experiment we have now 80 hours of data. As I pointed out on 15, there's so much data that, to us, it represents job security, so we're particularly grateful.

QUERY

What kind of resolution do you anticipate being able to get on the groundtrack by using the repetitive ...

ADLER

Our initial data were done looking at 1-minute intervals. Ultimately, on 15, we were able to process three consecutive spectra, integrated. That's like twenty 4-seconds. The feeling as we're getting resolution, which is like 60 by 80 nautical miles, the resolution actually is a little better than we had anticipated. I suspect the

ADLER (CONT'D) contribution from things coming from the periphery of the solid angle really doesn't exert so much of an effect. So it's going to be fun trying to get as detailed a map as we can by processing where we have adequate statistics. Incidentally, if the Sun was behaving, we can really begin to add up repeated values since we repeat the groundtrack so well; then perhaps we can go to 8-second spectra, but that sort of remains to be seen.

QUERY You mentioned Sco X. Did you get any data on Cyg?

ADLER Yes, they got some data on Cyg, too, but just as in 15, Gorenstein is not really ready to quite stick his neck out until he's absolutely certain the variation was due to the source rather than the spacecraft. On 15, it turned out that it was actually the source. Here now, he's got some simultaneous observations in the radio frequency range. The Cyg, of the two sources, is considerably weaker than Sco so you have to be a little bit more careful about your statistics, but he's got data on it as well. In fact, he's got a great deal of data which again makes us very, very grateful.

QUERY Do you want to cover alpha?

ADLER Yes, I did. The alpha thing worked well, and it looks like they found one hot source.

QUERY And say again where you think that was.

ADLER According to Dr. Bjorkholm, he thinks it's somewhere in the Sea of Fertility.

CHAIRMAN I wanted to comment. I did not make any reference, being a little too hasty on the gamma-ray experiment, on the transearth stuff. There's a good deal of excitement locally, that I should have referred to, about the fact that in the PTC mode, even during the mission, we were able to pick up two of the sources which have been discovered during the last year. They aren't new discoveries but it's remarkable to be able to confirm something like that, by the use of the spacecraft as an occulting disk with the boom at 6 feet. These are the galactic centers in the craft. Now in the pointing at the galactic anti-center, which you did the last morning, the interesting thing about that is that it looks as if the galactic center which is blocked is not exactly the source. It looks as if the source is only partly occulted, and there's some very deep data reduction going on right now to try to see whether on the PTC mode as well, it might have been just a

CHAIRMAN (CONT'D) little bit away. So this is in the early stage, but it is a remarkable fact to me that it's only within the last year that anybody has seen definite gamma-ray sources in the sky. Even at the first look at the data, we already confirm these sources and seem to be finding new structure. There's also one other spot that looks like a source, but I'm not yet supposed to say, because people again aren't quite sure of exactly where it is. So there's a lot of stuff in the can in that field, too, on the gamma ray.

QUERY Two questions for Izzy. Would you say there was a 30-percent variation? Fifty would be most encouraging. Can you recall what the percentage of variation was?

ADLER I don't know the exact numbers, but in talking to Bjorkholm, I get the impression that they did see variations in Sco at the time. The observation time is much less than this at this point. I think that the changes were not comparable to the sorts of changes they see now since they were so enthusiastic.

QUERY Do you recall the numbers Paul put down on 15, which was much less than just a 30-percent variation. He said he was seeing what amounted to a variation or flare that was something like 600 times the X-ray output of our Sun.

ADLER Yes, this is really very significant, and especially since the first sighting is where they really saw such large changes, which means that this thing sort of flares up maybe on a short time scale. So I think it was a very fruitful sort of measurement, these pointing measurements.

QUERY I have a question. With all the research ... your data around there, with names and features. Did you find a feature named Isadorus?

ADLER Is there one? They spelled it wrong.

MASS SPECTROMETER (S-165)

LOVELL Next will be the Mass Spectrometer. Dr. Hoffman.

HOFFMAN The mass spectrometer experiment worked very well up to a certain point in the mission. Beyond that point, I can't really say much for it. We obtained about 84 hours worth of data while in lunar orbit, and about three-quarters of that was actually in the minus-X direction. This is the condition in which the instrument is in the ram situation such that the scoop is pointing in the direction of the velocity vector and we should be scooping in the lunar atmosphere, which should be essentially stationary with respect to the spacecraft itself. The other times

HOFFMAN (CONT'D) were spent when we were in the wake direction or in some oblique angle, which is essentially similar to the wake direction such that the scoop or the instrument's pointing in the opposite direction from the velocity vector. We should then be seeing no lunar atmosphere, an ambient lunar atmosphere. I think the problems that we had with the boom should probably be relegated to the sessions tomorrow or the next day. Other than the fact that we did lose the experiment just before TEI, we had always wanted to have a subsatellite in lunar orbit, and sure enough, my wishes came true. So the instrument is still up there. But up to that point, I think we could say that, in general, the data are very similar to that which we saw on Apollo 15. That is, we saw many, many peaks in the mass spectra. We did scan the same mass ranges from about mass 12 to 67 atomic mass units. The water vapor peak is the dominant peak.

I have one sample spectrum here I'd like to put on the machine. This is a spectrum taken at 180 hours, which was fairly late in the mission. It is in the minus-X orientation, and this happens to be during the night cycle of the orbit. The water vapor peak is the mass 18 peak, and it is essentially saturated, as it was essentially throughout

HOFFMAN (CONT'D) the whole mission. All other peaks are considerably less percentage of the total gases that we've seen. We did a number of tests while in lunar orbit, such as water and urine dumps during the time that the instrument was fully deployed on its boom mount. And in two of them particularly, at 119 hours and 131 hours, we didn't see any change in any of the constituents, which indicates that perhaps the dump ports are not directed in our direction.

We would like to ask the question and get some comments on the observations of these particles, ice crystals, water droplets, possibly fuel droplets, or anything else that you have seen come off the spacecraft. Anything that you have to tell us on that is certainly going to be helpful. We did an outgassing test of the scoop itself, which is the gas-collection mechanism for the instrument, and showed that after it had been outgassed initially during this period of time, further 10-minute heating of it did not show any increased gas loads coming from the instrument. This tends to indicate that the source of all these peaks is really exterior to the instrument; as we indicated from Apollo 15 data, it's probably due to a vaporization or sublimation of ice crystals or fuel droplets or what have you that are in orbit with the spacecraft.

HOFFMAN (CONT'D) Again, the boom retraction tests that we did in lunar orbit showed essentially the same result. We got no increase, even though when we came in in the nearest position, which was about 5-feet boom extension, we probably had seen that in the case of the end of the bell on the back of the spacecraft, but this being a nice clean metal surface now is really not a source of gas. Also, we've observed essentially no difference between the ram wake conditions, although we do see quite a variation between day and night. There's a large diurnal variation, which could well be due to the change in the rate of vaporization of these ice crystals or fuel droplets or whatever the source of all these gases is. This particular spectrum was of some interest because it's one of the lowest amplitudes of the various peaks, and particularly the mass 20 peak, which is in the bottom spectrum. Normally, the mass 20 peak is due to a water vapor; it's the O^{18} isotope of water, and it is about 1 part in 500 of the O^{16} isotope. So $H_2 O^{18}$ gives rise to a peak at mass 20. For if one takes the water vapor peak, which in this case is just below our saturation level. Of course, we can get a handle on it also from the mass 17 peak, which is the OH radical that is made in the mass spectrometer ion source from the water vapor. Anyway, HOFFMAN (CONT'D) subtracting out what the contribution to the mass 20 peak ought to be from the water vapor, we essentially can account for the whole peak, which is about 60 counts on the scale. It's a log scale. We have 10^1, 10^2, 10^3, 10^4, and 10^5 counts per 0.1 second. We can essentially account for that whole peak as due to water vapor. Since this is in the ram condition at nighttime, we're looking here for an excess peak which will be due to neon, which would be a gas in the lunar atmosphere. Probably, according to the theories of formations, accumulations, and losses of gases in the lunar atmosphere, it ought to be the most abundant constituent, at least coming from the Sun. Since we can essentially account for that whole peak, due to the noise and various other uncertainties, we've set an upper limit of 10 counts there for neon. This translates back down to the surface, considering the surface temperature conditions, to be in about 10^5 atoms of neon per cubic centimeter, and this is an upper limit because we can account for the whole peak. So we've set an upper limit of 10 counts, which is equivalent to an upper limit of 10^5 neons per cubic centimeter at the lunar surface at the nighttime conditions. The theories which have been developed to account for a lunar atmosphere due to solar-wind origin give a value of 10^6 at night, and so we were about a factor

HOFFMAN (CONT'D) of 10 below what the theories would indicate. The dashed line that you can see just above the peak would be the amplitude of the peak, which would be equivalent to 10^6 neon atoms at the lunar surface. So you can see clearly that, had there been that amount of neon, we would certainly have seen it. So I think what we are saying here is that we have an upper limit to the lunar atmosphere from solar origin, which is a good order of magnitude less than what was predicted from the theories. Certainly, we know the flux of neon to the lunar surface, and that the surface itself is saturated; that is, it's in equilibrium. The same number of molecules per atoms per second are coming in as are being emitted from the surface. If that is not true, which this might indicate, that would be one reason why the gas in the lunar atmosphere might be less that what was expected. We are trying to do the same thing for the mass 36 peak, to try to look for argon 36. So far, we haven't been able to set an upper limit that's a little bit higher than the neon. Of course, argon ought to be quite a bit less than the neon and so that is not a significant result yet, unless we can lower the upper limit there. So I think what we can say to date is that there certainly appears to be much less lunar atmosphere from the solar-wind origins that what was expected.

HOFFMAN (CONT'D) As far as that atmosphere due to volcanic activity, we haven't looked at the data sufficiently and plotted it out to be able to make any assessment of that as yet. So I'd like to conclude by again also adding my thanks to the crew, particularly in their concern for the problems we had with the boom and the work they did in attempting to solve the problems before we got left in lunar orbit.

CHAIRMAN Thank you very much. Any questions?

MATTINGLY Let me see if I understand. Essentially, you proved that your instrument is not sensitive to direction, and that when we dumped, that apparently the scoop was doing its thing. We're not seeing direct impingement of things from the water and urine dumps. Is that correct?

HOFFMAN The scoop is probably doing its thing in that we're not seeing anything coming directly off the spacecraft, because in order to see anything coming off the spacecraft, the gas molecules have to undergo collision and be deflected in the right direction to actually get into the scoop. So I think it's doing its thing in that regard. Now as far as a ram wake effect, that doesn't appear to be there either, which it would be if the gas molecules that we were scooping up were not moving with the spacecraft. Since the gas

HOFFMAN (CONT'D) molecules are coming from the spacecraft, it's moving along with the spacecraft, so in the frame of reference of the spacecraft itself, it has just its own thermal velocities which are small compared to the spacecraft orbital velocity. So that's why, since it's moving with us, we don't see a difference when we are looking forward and backward. Had there been a lunar atmosphere which was not moving with the spacecraft, then we ought to see quite a ram effect when flying in the minus-X direction versus in the plus-X direction, which was our ram versus our wake condition.

QUERY Do you see a ram wake affect neon?

HOFFMAN We haven't seen any neon; that's our problem. This slide is in the ram condition. And had there been neon to the extent that was predicted, like 10^6 at the lunar surface at night, then the little dashed line up above the mass 20 peak would have been the amplitude of the neon peak.

QUERY You all looked at the spectrum ...?

HOFFMAN The spectrum in the wake looks essentially the same as the spectrum in the ram.

QUERY With neon?

HOFFMAN Right.

MATTINGLY Did you see any definitive change in intensity of counts with time?

HOFFMAN It appears that we're gradually decreasing with time but we haven't actually put everything on a plot and looked at it in a short time scale, which we'd have to do in order to see that kind of difference.

MATTINGLY It's a small difference then, anyway?

HOFFMAN It's a small effect, though.

MATTINGLY There are apparently large numbers of particles that travel with you, and they are not stationary. You could always see these at sunset and sunrise by looking down towards the lunar surface still in the darkness of the spacecraft and coming in illumination and the surface would be dark. You could look down and then you could see particles that were illuminated by the Sun. You could tell these little objects were going with you. But they look like you're stationkeeping for that short period of time, but the same particle was not there. If it was there at sunset, it wouldn't be there at sunrise in the same position. In fact, I was never able to identify any particle that remained, MATTINGLY (CONT'D) so they do have some relative velocity even if they continue to be there. The first time I saw it, it was not at all related to a recent dump.

HOFFMAN Did the density of these particles seem to increase right after a dump, or couldn't you tell?

MATTINGLY I don't recall that it did. I really never got around to making a quantitative estimate of that, but the times I remember seeing it (and I can dig these times out of the tapes) was a good while after a dump.

YOUNG We watched the ground call a dump when we were stationkeeping in the lunar module after the first rendezvous when we joined up again. That just came out in a straight line. How far would you say from the vehicle, before it started to spread out and go diffuse? It just proceeded right away from the vehicle just as far as you could see until it disappeared. It was really amazing. I would have thought it would have been more diffuse and spread out more, but it just seemed to come right out.

MATTINGLY There is a big difference in the dump characteristics than what you see from the waste water and the things you dump from the cockpit. On the way home, we dumped a bag of water at 5 psi out the hole by the hatch where you could see it, MATTINGLY (CONT'D) and it did go out in pretty much of a jet for a considerable distance. So even at 5 psi, it's coming out and going a long way as a uniform jet before it starts having diffusions.

HOFFMAN So there's no significant spray that comes off from it then, you'd say?

MATTINGLY Sure doesn't seem to be.

YOUNG No, it really was surprising. You'd think that you'd see particles collide and go every which way, but they were just coming out and going in a straight line.

MATTINGLY On several occasions, we saw particles that were deflected from the LM; when the LM was on the nose, you could see particles which were not direct collisions but you get a change in the velocity vector of a particle that hit the LM, and maybe it would get slowed down and deflected out to the side and then it would subsequently hit another particle departing the spacecraft, so it made its third collision to that one. After we lost the LM, I never saw another particle change direction, show any sense of rotation, or in any form return towards the spacecraft, except for these little particles that were in orbit with us. But no direct mechanism that I ever noticed showed

MATTINGLY (CONT'D) any change in directions. When the LM was on board, you could watch most of these particles collide and that way, you could get one that was coming back.

QUERY Can you give us the average size of a particle?

MATTINGLY Our only hope to put any size estimates on these things is to look at the stereopictures on the way home. With no reference, I have the impression that they are relatively small, a quarter of an inch or so, but with no scale reference, they could be anything from micron size to an inch, because the Sun illuminates them so brightly. I think you're seeing intensity variations in reflected light rather than in size.

HOFFMAN They are just points of light?

MATTINGLY Not the very slow ones, right at the end, when things started to come off slowly. I suspect that a lot of that's boiloff from around the nozzle. You can see some size to those. The only comparison we had was the LM paint that was peeling. It was going off in chunks of material that were probably 0.5 inch in size; they too lost their size definition very quickly. You could tell that they were particles that were flat, because as they rotate, you could see them wink. I don't think you could make any estimates.

YOUNG Some of the ice crystals that came out of the vehicle clearly had a glow around them like a halo. They would get small and disappear. You could see, they were just going "poof." Sublimating right before your eyes, I guess. That was the feeling I had.

MATTINGLY On transearth coast was something I had never seen before. It may have been there, but we commented on it that all of a sudden, I saw these little particles with a little halo around them. All of them did not have that, just some of them. I didn't see it before and I didn't see it again. How much of that was caused by a peculiar Sun angle incidence, I don't know. The particles disappeared after a while. I tried tracing any individual particle on its track out, and they would ultimately reach a point where I lost contact with it.

HOFFMAN And there were none that hung around during transearth coast, like you saw in lunar orbit?

MATTINGLY I don't recall seeing one, but I didn't specifically look for it either.

QUERY I have a question regarding the clasts you saw on the back side in the vicinity of Mare Orientale.

MATTINGLY No, sir; it would be beyond that.

QUERY You said it was shortly after LOS?

MATTINGLY Yes. But I was looking at the far horizon; it would have been further around in the direction of orbital track.

SIM BAY PHOTOGRAPHY

DOYLE The SIM bay photography consists of three instruments: the mapping camera, the pan camera, and the laser altimeter. Unlike most of the other experiments in the SIM bay, we don't really have any answer as to what we actually got until the film is processed, and that has not yet taken place. However, I can show you from the data that we acquired during the mission exactly what we think we have obtained. May I have the first slide, please?

This slide shows the actual coverage obtained with the mapping camera in black up here. The first data pass started on rev 3 on the far side, and we got this little piece of data over here and that continues with the light-side pass through the rest of the mission. The second data pass was to be on rev 15 and 16, and, of course, that was delayed by the change in the Flight Plan due to the delay in the landing. With the real-time planning and the cooperation of

DOYLE (CONT'D) the flight planners, we were able to get in another photographic pass as soon as the landing had taken place and as a consequence lost very little data due to that delay. Where we did lose data was as a consequence of abandoning the second plane change. What we lost was this part in red up here at the eastern edge of the coverage and this part down here at the western end of the coverage. Also, because of coming home a day early, we lost the 13 degrees of longitude that we would have had at the western end of the site. The dotted line here gives the coverage which was obtained with the oblique photographs north and south of the flightpath. These indications on the slide are obviously plotted just from the camera on/off times. We had no indication during the flight that there was any abnormality in the operation of the mapping camera. We did have a little delay in the extend and retract times, but there was no reason to think that that caused any loss of photography. The magazine for the mapping camera was opened last week, and we found some metal chips in the magazine. This, of course, is a cause for considerable concern. We have a meeting this afternoon following this presentation at which we will try to evaluate that condition.

DOYLE (CONT'D) The pan camera coverage is indicated in the lower diagram down here. Again, the first coverage was to take place on rev 3. The camera was turned on, and Ken noticed a draw down on the bus voltage and turned the camera off immediately. So we did lose this little piece of coverage that we would have obtained the rev 3 and 4 pass. We did the same thing in the real-time planning with the pan camera and were able to pick up a photo pass as soon as the landing had taken place. Basically, we did fill in most of the area which we had planned for the pan camera. The areas in black are the total area covered which we obtained. The areas in red are the pieces which we missed due to the deletion of the plane change and the day-early return. This little piece down here was an oblique pass which we had planned with the pan camera. It would have taken place on rev 72 in order to get photographs of Gassendi which is an area of particular interest to the geologists. So you can see that we did lose some data. On the other hand, we did pick up some data that we would have not gotten, and I think, overall, the effectiveness of the coverage was about 90 percent of what we had hoped for premission. I really want to express the thanks of the photo team to the flight planners here and also to Ken for his operation of the cameras during the mission. I know that it was very confusing to be continually changing

DOYLE (CONT'D) the Flight Plan, and Ken probably wondered what we were doing with all these on/offs and so on. But really I think we did a very effective job of recovering almost all of the data that we had planned for the mission.

May I have the next slide please? This is a diagram showing what the Apollo 15 coverage was like. This is the planned coverage for the 16 mission, and then the dotted line is the planned coverage for the 17 mission. The only reason I show this is to indicate that the areas where we did lose data, unfortunately, are in the areas that were not covered by 15 and will not be covered by 17, so that the losses, although small, were real.

Ken, during the film recovery EVA, you reported that the stellar camera glare shield was hung up on the handrail. I'm not too clear as to exactly what that situation was. This is the stellar camera glare shield, and this is the little cover that comes down and covers that when the mapping camera is retracted. Now maybe you've discussed this with the other people here at the Center, but for my own information, I would like to know whether this is the cover that was hung up, or this cover?

MATTINGLY Both of them. The shield at the forward end, that one. The first one you looked at. Yes. That was sticking up. But the tip out here was mashed against the handrail. That's right, and this lip was up against it and bent back out of the way. That was not in the full extend position. It looked like it was a partial extension.

DOYLE Yes. Could you tell whether this extend rail was bent?

MATTINGLY No, but I think we have a photo that will probably tell you that. I don't remember. The end of the shield was bent. Now, whether that rail itself was bent, I didn't notice.

DOYLE The implication, of course, is that if this had happened early in the mission and this shield were bent down, we might get excessive light into the stellar camera, and our background density would be higher than we expected. We might not see as many stars. The other indication of malfunction that we had, of course, was in the exposure control on the pan camera, and that would indicate that the pictures away from the terminator may be overexposed. That situation is also being looked at, and, again, we're going to talk about it this afternoon before we actually process the film.

DOYLE (CONT'D) May I have the next slide? This is the information I have in regard to the laser altimeter operation. The first row across is what the nominal mission would have been, and the second row is the actual results from Apollo 16. We had planned a total of 20 hours and a few minutes of operating time. We got 14 hours and a half, so we are down to about 25 percent in the total operating time. This is the total revolutions in longitude that we had planned - 10.3 - and we got 7.5, so again we're down to about 25 percent in that regard. The total number of firings is 3283, and 2106 is the actual number that was recorded. So we are down nearly 30 percent in the total number of firings. Also, the number of valid elevation readings that we got was appreciably less than the total number of firings, so that the actual mission in terms of altimeter observations gave us a little bit less than half of what we had actually planned. That's really not as serious a short fall as it sounds just from the numbers, because the readings were quite well distributed throughout the mission. The general operation that we saw on the altimeter: the first several revs were completely nominal; all the elevation readings were valid. Then, it began to fall off about 75 percent, 65 percent, and down to about 60 percent, except on the last data pass on rev 62 where it was only about 10-percent effective. Generally, what we seemed to observe is that there would be one good

DOYLE (CONT'D) shot then one bad shot, then one good, one bad, and then maybe several good ones in a row. We can talk about the reason for that, but that's more appropriately covered in the systems review tomorrow. The effect that it will have on the data is simply to give us a larger spacing between data points. So far as its effect on the reduction of the photography is concerned, that's absolutely inconsequential. So far as its effect on the correlation between the tracking and gravity data and the profiles, it's of a little more concern. They do have a little more smoothing to do between the data points that they got, but, essentially, I don't think that it really hurts us so far as the scientific return from the mission is concerned.

I would like to say just a word or so about the utilization of the photography. It has been proceeding much more slowly than I had anticipated in reducing the pictures from Apollo 15. However, the work, which is being done by the ACIC in St. Louis, is indicating that the photography/ photographic reduction will provide positional coördinates of features on the lunar surface with the accuracy of 10 to 12 meters in position and in elevation. And that seems to coincide very well with what we had predicted premission. So we are quite confident that we're getting very good

DOYLE (CONT'D) information from the pictures in that regard. The tracking data, generally, is consistent within an orbital pass, but we do find discrepancies of up to a kilometer between adjacent photographic passes, so that the reduction of the photographs is giving us a much better tie between orbital passes then we get from the tracking data itself. Consequently, we do expect eventually to come up with an internally consistent coordinate system/reference system figure of the Moon, with an accuracy on the order of 12 to 15 meters. That's highly gratifying to me. So far as the pan camera utilization is concerned, there have been some map compilations done. The indicated precision of those is on the order of 3 meters, which again is about what we had expected, but that is a precision and not an accuracy number because of the geometric problems with the pan camera photography. However, so far as the resolution of the pan camera is concerned, that has held up to just about what we had expected, from 1-1/2 to 3 meters at the subvehicle point and decreasing to about 5 to 6 meters at the limits of the film. Our indications at the moment are that the results from Apollo 16 are quite satisfactory. We do have these problems that we have to resolve in the processing of the film, and once that is done, we will be able to say exactly what we did obtain. But, again, I want to express the photo team's thanks to

DOYLE (CONT'D) you particularly, Ken, for the time and attention that you gave to the camera operation and the results that we have obtained.

CHAIRMAN Thanks. Are there any questions from the floor?

MATTINGLY Do you have a plot of the altimetry? You know, last time, someone had drawn up a rough hand sketch of the altimetry. I wonder, did anyone do that this time?

DOYLE No, I don't, Ken. I think maybe Sjogren has one of those. He'll show that.

MATTINGLY These particles that you found in the mapping camera, was that handfuls or a couple of shavings?

DOYLE I don't know how extensive they were. They were enough to concern the people in the processing lab. That's about all that I know about it. We're supposed to have a review of that this afternoon. It could have very serious implications, obviously. One thing that I seem to recall from during the mission is that we had apparently more film left for post-TEI photography then we had anticipated. And what that could mean, of course, is that the camera was not passing film when we thought it was, and things were being chewed up pretty badly inside. So we're going to have to look that

DOYLE (CONT'D) over very carefully before we go ahead with the processing of the film. We expect the camera contractor to look at the shavings and tell us where he thinks they came from, and that may give us a better clue as to what they actually are.

MATTINGLY Do you have a processing schedule yet? I guess it's all in abeyance to what you do.

DOYLE Everything is in a hold until after the meeting this afternoon.

MATTINGLY Assuming you determined to go ahead and develop, what would it take you, 3 or 4 days to process?

DOYLE Yes. I think the anticipated schedule was to have the original film processed within this week, and all the duplicate copies out and distributed within 4 weeks.

QUERY What is the accuracy of the altimeter?

DOYLE The least count of the altimeter is 1 meter. The accuracy is dependent pretty much upon the slope in the area which is illuminated and a little bit on the albedo in the area which is illuminated. In general, I think it's fair to say that the reading that we get from the altimeter will be correct to within 3 to 5 meters.

MATTINGLY Have you figured out what to do with the pan camera for processing? Have you got a correction for the overexposure?

DOYLE I don't know what has been figured out. We had a group working on it last week, and they're going to give us a report right after this meeting. We'll decide then what to do.

SPEAKER I would like to made a recommendation here. ... that there will be some slight loss ... They can handle the overexposure but at some penalty ...

DOYLE The situation is really that near the terminator the pictures are underexposed anyhow, because we can't open the slit wide enough and so on. So the camera was wide open at the terminator, but it was open wider than it should have been when we were away from the terminator so that the terminator pictures are a little bit underexposed, then they become properly exposed, and after that, they will be overexposed. So what we would really like, of course, is a variable processing through each photo pass, but that's probably not a feasible thing to even contemplate.

SUBSATELLITE PARTICLES AND FIELDS (S-173)

CHAIRMAN Next subject will be the Subsatellite Particles and Fields.

McCOY Just quickly, the subsatellite was deployed successfully. Our spin rate was nominal, about 5 ... second spin period. We want 5 plus or minus a couple. The attitude was good. According to preliminary indications from the Sun sensor, we're at a couple of degrees tipoff from the ecliptic, which is well within the limits we needed. The operation of all the electrostatic analyzers and both solid-state telescopes is good, although we don't seem to have the noise problem that we had on Apollo 15 in a couple of the analyzers. And we got rid of our accumulator counting error that was characteristic of 15. It's giving us some problems in our data analysis now of that satellite. A quick look at the results of our first magnetotail pass indicates again the presence of these rather unexpected, rather high fluxes of low-energy protons which have also been seen now with the new IMP series I satellite experiment, too. And on this magnetotail pass, we appear to have the remnants of a small solar event of the form of some high-energy solar cosmic-ray electrons and protons around, which should prove interesting for our shadow interpretation, particularly on electric fields and the magnetotail. Beyond that, if they have those slides there, I'd like to briefly describe some of the results we got from Apollo 15, which we'd hoped to be similar to this one. Our primary experiment was to examine particle shadow

McCOY (CONT'D) configurations in the magnetotail. That'd be while we're passing through this region back here where we are in the Earth's magnetic field, and I'll use that to examine the question of openness and access through the magnetotail to the magnetic field lines and alternately to the Van Allen belts, where theory generally holds that Van Allen radiation does somehow come in and then be trapped and accelerated. The degree of connection back here with the interplanetary field has been in quite a bit of question. And, briefly stated, the Apollo 15 results indicate pretty clearly that at least most of the time at the latitudes where we passed through the tail - we only have a couple of passes - that these field lines are in fact open out here, connect directly into the interplanetary fields, and have direct access of solar cosmic-ray particles. We have also observed the existence of a plasma sheath which is known to form in closer to the Earth to extend out to the Moon distance. On at least a couple of occasions, we have observed across-tail electric fields, which are important to theoretical models accounting for aurora and for acceleration of particles into the radiation belts. An unexpected observation which we've found interesting was 30-keV or thereabouts protons in very large numbers, which we observe quite frequently back in the magnetotail and also have observed outside the magnetotail

McCOY (CONT'D) in very similar fluxes. Our first feeling was these must also be some component of the solar cosmic-ray proton spectrum. They're, of course, very low energy, which are stopped by even the thinnest piece of material, but the numbers of them and the consistency of their flux densities make them very attractive. That's probably quite indicative of the source and mechanism they come from. Further examination, however, of the locations where we see them and of the almost constant intensities that we see almost forces the conclusions that they must somehow be protons from the outer Van Allen belt region here, which are somehow coming loose from the Earth's field and moving outward and then getting out to the interplanetary fields and moving out to where we see them. And perhaps an inverse process of the postulated process where we bring solar cosmic-ray particles in from the Sun populate the Van Allen belts initially. We're anxiously awaiting getting our data, and now our computer programs at Berkeley are in shape where we can examine this in detail. There has unfortunately been a delay in that.

If I can have the next slide, I'll show you one orbit of data from the telescopes when these very steady fluxes of protons were present. All during this period and actually for a period of a couple of orbits earlier, these fluxes

McCOY (CONT'D) were almost constant. There are some changing here. There is a very slight shadowing of protons, not very much at all. This is mostly an electron phenomenon. But then these particles essentially turn off as if somebody closed a valve, and this is characteristic of the way they behave. When they appear, they're suddenly there; and when they disappear, they're gone just as fast. We're very hopeful that we'll be able to make some sense out of what turns these things on and off and determine where they're coming from and what the mechanism is, presumably now, in the magnetosphere.

QUERY What is the lifetime estimate of the subsatellite?

McCOY (Laughter) Wish you hadn't brought that up. The next speaker will get that for us. We've been very hopeful of getting another couple of dozen magnetotail passes out of this satellite, since it worked so beautifully.

CHAIRMAN I think we're going to cover that.

McCOY Chris said it was going to stay up there forever, so it better. (Laughter) If it doesn't crash, we hope it will.

CHAIRMAN Any questions about particles and fields?

MATTINGLY I assume that our satellite is sending out good data. Is it?

YOUNG Is the battery charging okay and everything? It's not running into the problem that the 15 guys had or is that normal?

SPEAKER Seems to be a little bit better in the battery charging.

CHAIRMAN The machine is working.

MATTINGLY And all the detectors are operating?

McCOY As far as we know, yes. It's just that it's got a short lifetime.

SUBSATELLITE MAGNETOMETER (S-174)

CHAIRMAN I think Larry Sharp is going to take Coleman's place. Magnetometer.

SHARP The objective of the magnetometer experiment is essentially threefold. First is to measure and map the remanent magnetism on the lunar surface. Second is to map the electrical conductivity of the lunar interior. Finally, we study the various aspects of the Moon's interaction with the fields and particles in its environment. The wide scope of these objectives is made possible by the geometry of the surface

SHARP (CONT'D) as it passes through three fundamentally different regions of space. For example, to get the spatial variations of remanent magnetism on the lunar surface, one must be in the geomagnetic tail, where the temporal variations are almost actions of the magnetic field, a very steady situation. The initial orbit of the subsatellite was somewhat lower than Apollo 15, and the inclination about 11 degrees instead of 28, and I'd like to show you the predictions of the orbit. We've plotted days past deployment versus the perilune altitude. We started out here in pretty good shape about 97 kilometers, or 97 by 123 I guess, and the prediction was a very rapid drop down to about 30. It's back up in good shape. No real trouble until about 200 days when the probability of a crash is 50:50. Here are the error bars, you can say the probability was maybe one chance in 10 of a crash here, and maybe one chance in five. We've updated this plot. The odds have changed considerably. This is the prediction. Notice the time scale has been expanded greatly. These are hours now instead of days. Here is the initial prediction, that 37 kilometers; and here are the actual data points. This one was taken this morning around in here, and if you can extrapolate by eye, it looks like we have about 4 days left. I guess Bill Sjogren's taking 50:50 odds that it'll crash if anyone's in a betting mood.

SHARP (CONT'D) It's possible it will skim the surface and come back out. Even if we make it through this one, the next dip is going to probably finish us off, which is most important.

CHAIRMAN Just out of curiosity, this might be out of your field, but how come our prediction didn't match the actuality?

SPEAKER We don't know the gravity field that well and the business of the subsatellite initially is to determine the gravity field. We're ... about these pads, that I don't go along like here at MSC. I've tried to drive that point home so many times.

MATTINGLY It just goes to show you can't redo 6 months of planning in 2 days.

SHARP In any event, we have 1 month's worth of good data, which we'll add to our Apollo 15 results. To get some idea of what we're doing with this data, I'd like to show the first slide, which represents an average of 17 different orbits taken when the Moon was in the Earth's magnetotail. We plotted the Moon's longitude on the horizontal and the magnetic field in gammas on the vertical. These numbers are representative of the Earth's tail field, and, of course, if you subtract out an average value, this residual would represent the lunar surface field. Of course, the big result

SHARP (CONT'D) is this really huge magnetic dip going over Van de Graaff Crater, or a region right near the Van de Graaff Crater. Also, the other initial result we got from the Apollo 15 data was that most of the dips in the magnetic fields seemed to be clearly associated with craters lying within a few degrees of the groundtrack defined by the orbit of the subsatellite. We've numbered the seven most obvious local minima and named five of them with the associated craters. Initial results from Apollo 16 show the same type of structure, although we don't see anything as big as Van de Graaff. We do go over Korolev again. That's where the orbits intersect when we're in the tail. We see a few other Hertzsprung- and Pavlov-type bumps. One is with a little tiny crater called Stein and one near Mendeleev. Now if you could repeat this procedure several times with different lunations, say, cover different tracks over the Moon, then this allows a contour map to be made. If I can have the next slide.

This shows the ground tracks from where we found the nine. Here's one over Hertzsprung, Korolev, here's Van de Graaff. You can see it went right close to the northern border, Pavlov, and one over Milne. So there's a pretty good one-to-one correspondence with large craters. The initial 11 orbit, of course, is much nearer the equator. The inclination goes up to plus or minus 11 so we saw one over

SHARP (CONT'D) Korolev, and Stein crater is located right about here. It's not too big. The next map shows a compilation of a lot of these linear profiles into a contour map. It's a little harder to read the contours. You can see this big black blotch here is a result of contours stacked on top of each other near this Van de Graaff anomaly. You can see the numbers are hard to make out. You can see the structure over Korolev, a hint of some structure over Hertzsprung, and the southern sea over here is actually an enhancement sticking out of the Moon, so to speak. You can see Milne shows a definite structure. The Apollo 16 results should enable us to expand this map. We'll get better resolutions throughout this area and be able to extend it along in here. We'll probably still have a gap down in this region.

QUERY Larry, can you give some idea of that gradient in which ...?

SHARP The numbers presented in the chart, if you can see them, are measured in tenths of gamma at an altitude of 100 kilometers. For example, a good representative value is about 30, and the zero we've arbitrarily chosen at the bottom is Van de Graaff dip, since this is the lowest value of magnetism we observed. We just called it zero and scaled everything relative to it. So if you're at 100 kilometers, you'll see a 3-gamma dip going over Van de Graaff on the average.

YOUNG Does the gravity profile have any correlation?

SHARP I've looked at a gravity profile, and it doesn't seem to correlate at all. We've got some laser data that correlates better with - Yes, there's no gravity in the back side, but on the front side -

YOUNG I understand.

SHARP It correlates pretty well with the laser data which show a great big hole here in the back side of the Moon at Van de Graaff, and that's where we get our big hole, but I can't see any scientific justification for connecting the two results.

SPEAKER I will add - remind you the gamma rays are - a secondary peak is also at Van de Graaff. I don't know what it means.

SHARP The next slide shows the front side of the Moon. The front side of the Moon is much, much smoother than the back side. The variations are on the order of a factor of 10 smooth. You don't see very much structure at all; it's very hard to even draw contours.

MATTINGLY Can you sort out the differences in that and the effects of running through the Earth's magnetic field effects since the front side is always closest to the Earth? Are we really

MATTINGLY (CONT'D) measuring magnetic variations on the back side caused by being on the back side, not due to our measuring environment?

SHARP The Earth's tail field is very, very constant when you're away from the neutral sheet [?], and that's where all these data are taken. (I guess we never got the front side complete.)

This is a blowup of the Van de Graaff region in an attempt to pinpoint the exact source of this large anomaly. We suspected it was over the crater itself, since we always put forth the theory that what we were actually observing in these dips was some sort of a meteorite impact that caused a rather uniformly magnetized crust to suddenly have holes in it, and we've seen the equivalent dipole of what was left over, and this kind of shoots holes in the theory. This is the B_X component which is the radial component, and a plus number indicates a value sticking into the Moon. So you see this rather large hole here magnetically in between two craters, and if you look at various models of double dipoles and stuff, it just doesn't quite fit. The other components B_Y and B_Z show that it can't be one of these angling dipoles in the crater. Presently, we're conducting the same sort of studies over the rest of the anomalies to see if it checks out. I just yesterday completed the one over Korolev, and

SHARP (CONT'D) that one is right in the middle of a crater, which is promising for this shocking magnetization theory.

QUERY How much belief do we have in the groundtrack data?

SHARP How much validity do we have in ephemerises? I understand that there's no chance that we could be off by a kilometer or two. (Laughter) We'll get maybe one more. As the subsatellite comes crashing down on the Moon, the last 10 kilometers should give us a good swath of data which will give us another high-resolution plot like this. Other than that, we're kind of out of luck. There's the front side of the Moon. This is zero degrees. Then we go from zero to 90 east to 90 west - the terminator. This is the Southern Sea region which is about the only distinctive feature on the front side per se, and even that's on the terminator. Again, these are relatively high values of magnetic fields sticking out of the Moon. So you might say that the Southern Sea is a highly magnetized region. Notice that the 16 site is out of our coverage, as were all the Apollo landing sites.

QUERY With that 3-gamma delta on the back side, what are the ... on the front side, as far as you know?

SHARP Well, most of these values are about 30, 28, 27, which means that they are all 3-gamma higher than that zero at Van de Graaff, so I would say the maximum plus or minus is about half a gamma, on the whole front side.

QUERY Why are your anomalies elongate parallel to the track?

SHARP This is a function of not having quite enough data. Instead of making physical-looking contours, we drew actual contours of the data and, obviously, if there's a little bit of offset from one orbit to the next, it's going to result in elongated contours. If we fixed this up and made some intelligent-looking guesses, it would be much more circular.

QUERY Here are data on the Apollo 12 and the 15. ...?

SHARP Yes, the 12, 14, 15. Well, you see, the surface data deal with much smaller scale-size phenomena, and so there's really no one-to-one correlation. If we could be sure over a region of 100 kilometers that the average field was 38 gamma, which no one would bet on, then we could make some interesting predictions about other places on the Moon, but I suspect if you went down to Apollo 12 and 100 meters away, you'd find a much different value of magnetic field. Another significant case, look at Apollo 15, they went 1 kilometer, SHARP (CONT'D) and they went from 43 gamma to 103 gamma, so who is to say what's a representative value for the magnetic field for a given area.

QUERY What about 15?

SHARP Apollo 15 has a steady field of 6 plus or minus 4 gamma which is essentially zero, but there again, behind the next boulder, it probably could be 100 gamma for all we know. The results in 16 I thought were tremendous, the tremendous gradients they got between one place and another. That 313-gamma field really surprised me as far as getting a value that large. It would seem to me that the younger a crater was the more chance it would have to produce a good cleancut signature on the magnetic field data. For example, Van de Graaff itself looked like a fairly young crater because there are very few secondary craters in the bottom of it, so it looks fairly young, whereas things like Hertzsprung seemed to be fairly old, well-blotched with secondary craters.

QUERY What was the difference from the center of Van de Graaff to the center of the anomalies?

SHARP It's 80 kilometers from the northern rim, so from the center of Van de Graaff, it's about 130, 140 kilometers.

CSM/LM SUBSATELLITE TRANSPONDER (S-164)

CHAIRMAN The last subject will be the Transponder. Bill Sjogren.

SJOGREN This is a gravity experiment, and we monitor the gravity by just monitoring the velocity of the spacecraft or the LM or the subsatellite. Of course, on this particular mission, we lost our LM data on impact because it started tumbling, and those data were lost. We do, however, have the CSM in the low-altitude orbit, which is very interesting data. Here is the orbital track. This profile just below it is the gravity anomaly that was detected. This line right here represents zero gravity essentially, isostatic equilibrium, and anything below it, of course, would be negative gravity deviations. Here we are at Ptolemaeus going essentially over the center with the track, and we can see the large negative anomaly again of almost 100 milligals. Here we see some highland material where we have a positive, then we drop down into a relative low in between these two old craters - Hipparchus and Albategnius - and then a high in here. At this point, this is Descartes, the landing site, we actually have about a 50-milligal negative anomaly in that region. There are some nice correlations here with the laser altimetery measurements, and I'll show them in the next profile. But this is continuous - I've just taken

SJOGREN (CONT'D) one small section here, we have this thing from limb to limb, so about 110 longitude plus 110 minus and essentially eight revs, rev 3 through rev 11, eight revs of data that are pretty good, and then they started their stationkeeping, and our data kind of got garbaged up for a while with all the maneuvering that was going on. I want to point out one kind of interesting thing here. Notice that Ptolemaeus here has an anomaly much lower than Nubium, although Nubium, which is this region right in here, is still at about minus 50 milligals. There is an altimeter profile and, going right to the Nubium-Ptolemaeus region, again, we see that Nubium elevation here is some - by the way, the scale here is 2 kilometers per heavy line, so we've dropped down here almost 2 kilometers from the floor of Ptolemaeus to the floor of Nubium, yet the gravity anomaly is just the opposite - that Ptolemaeus is some 50 milligals lower than the Nubium region. Another interesting point is, on these ACIC maps, Nubium is shown a kilometer higher than Ptolemaeus, and here we are two kilometers lower. So we're talking about a 3-kilometer discrepancy in just that little area right there, so I think some of these guys were talking about the right thing when they said we really didn't know the altitudes too well.

MATTINGLY I think that shows up quite dramatically when you look at the low-Sun-angle view as you approach the terminator. The things that were in Nubium didn't show up until significantly after we anticipated. We missed our times on those things a great deal, and that could only be caused by having a discrepancy in our relative heights.

SJOGREN This is rev 28. We've looked at five very good laser-tracking passes over this region, and we've looked at three of them, and all three show the same consistency of a drop there. Another interesting thing here at Procellarum, Tranquillitatis, and Fecunditatis - they all seem to be about on the same level. Smythii, again, being a low of about 4-1/2 kilometers - that was the same thing on Apollo 15 when we passed over it. It was some 4-1/2 kilometers below mean radius here, and we're still referencing this to a 1738.1 radius off the c.g. If you take this stuff and start fitting it to an optical center with a c.g. offset, you do indeed get the 2-kilometer shift again of the c.g. being closer to the Earth by some 2 kilometers.

QUERY Does that say that Tranquility is about 2-1/2 kilometers lower than Descartes?

SJOGREN That's right. Yeah. Here's the landing site right in here. This is where we had the negative gravity anomaly in here.

SJOGREN (CONT'D) According to this, maybe that's the situation because it looks like a topographic low. This the back side. These are the limbs where the data were missing. These are taken from that thrift [?] printout that we get in real time, and there's some gaps in the data. These will be filled in once they get the station tapes delivered. We can see the crater Hertzsprung here very evidently with the central peak or something in it.

MATTINGLY You don't have any data in that 120-degree region, where it's missing?

SJOGREN Not now. We should have next week or so, whenever they get the tapes here and reduced. On Apollo 15, there was a definite big trough in here centered about 180, which was down about some 4-1/2 kilometers. It was very jagged, but it was centered just about here. On Apollo 16, we do not really see that. We see this highland material here which we had on 15 - that started about in here and showed a marked high region in there and is holding on 16 also. So this is kind of consistent with 15. Here is another - this is orbit 38 on the back side. I'll just lay it there to show you some of the consistency. This is 10 orbits later, and the profile is almost there - Smythii. Here we have some data on

SJOGREN (CONT'D) that. So there's your data, Ken. Were you looking for some particular feature right in here?

MATTINGLY I was looking for something around 105.

SJOGREN 105.

MATTINGLY Nothing in particular.

SJOGREN It would probably be on here somewhere. Nothing.

MATTINGLY Okay.

SJOGREN I do have some data on the subsatellite for Apollo 15, but I don't know if that's the time to really show you. I think I've shown enough stuff on 16, huh?

CHAIRMAN Well, I'd like to stick to 16, any of the problems we have or any of the data that we got for the crew.

SJOGREN Well, I'm very happy with all the data we've gotten on 16. The subsatellite on 16, as Larry mentioned, looks pretty grim. Although, there was an OD solution just run out about an hour ago where the last state vector was used as the position for running out the lifetime program. And the lifetime program, if we believe that 15-8 [?] model that we used initially back there when we ejected that thing, says that this thing should come back up, still says it's not going to crash.

CHAIRMAN This will be a very interesting thing to watch in the next couple of days, I can see.

SJOGREN Right.

SPEAKER That's all right, Bill. All the seismometers are cocked and ready. (Laughter)

SJOGREN I asked Kovach about that, and I don't know whether he can speak for Latham or not, but I told him it was 50 pounds. Whether they can actually see an impact of 50 pounds - I think that's all it weighs.

QUERY Where will it hit?

CHAIRMAN The central station.

GROUP (Laughter)

SJOGREN It should hit some 20 or 30 degrees west longitude and 5 or 6 degrees north, something like that. About 5 or 6 north latitude and about 30 west. That's what it looks like in that linear extrapolation that we've got.

CHAIRMAN Thank you, Bill. Thank you, gentlemen, for attending. This concludes our scientific debriefing, and I'd like to thank the crew for giving their time up to come on over to give us some information and also learn some things that happened.

STATEMENTS BY APOLLO 16 CREW

YOUNG Let me say something for the crew. I want to commend Ken for the way he operated the SIM bay. I don't think you'll ever find a guy who's either more interested in it or more aware of what the operational problems were and who could do a better job. He really did an outstanding job, and I think he's done well. The other thing I'd like to say is, since I've been on this program, I've been continually impressed with the importance of what we're doing in our support of you guys. These experiments look way out right now, and they're difficult for the man in the street to relate to; but my feeling is - from some of the things I've seen here and from the thrust of science and technology in this country - that in our lifetime - and I don't want to say when, but I imagine it's going to be sooner than anybody even here in this room can imagine - there are going to be practical applications of some of these discoveries that have been made that'll affect every one of us. I think you're to be congratulated. The other thing that I get out of all this is that I think the United States ought to be spending two to three times the amount of money that we are spending on basic research and development and applied research and development; and I think that, if you really

YOUNG (CONT'D) look at the big picture of our energy requirements for clean energy in the future in order to improve the life of human beings on this planet, we ought to do that and get on with it. Thank you.

MATTINGLY During the next couple of months, we'll probably have an opportunity to talk to a lot of people that have some influence or shape the pattern of our program and related programs, and I would appreciate it very much - I'm sure we all could use the data - as you find out things - we realize that you have preliminary data today and a lot of the things are only hints and suggestions of things to come. I would personally appreciate it very much if you would give us a call periodically and tell us what you've found. It's too easy for us to go through here and, at the end of a week or so, walk off and never really know what happened, and never know what kind of data came out. I think that makes us very poor salesmen. I think you could help us do the job to kind of feed back the quality of the data that you've been getting. If you could keep us informed and - please don't ever feel like you're interfering with our operation and don't be inhibited to call us. If we can't come to the phone and take the data or talk to you about it, our secretary will take the number and we'll be glad to call you back. If

MATTINGLY (CONT'D) there's any time when you have some question about the data - and it doesn't matter how trivial it may seem to you - if you want to look at some data and you're curious to know whether we'd ever seen a particular observation, or whether the spacecraft was moving at the time your data was taken, or - I don't care how far out it might seem to you. Questions are pretty cheap, and not asking questions can be very expensive. So please feel like our job isn't finished until you guys have all the information you can use. I would like you to feel like you can call any time. Thank you very much.

DUKE I'd like to give you my thanks also. I was very pleased with the way all the gear worked, especially my part on the lunar surface and the part I saw of Ken in orbit. It was just a real pleasure to have emplaced all this stuff for all you people, and I'm glad it's working so well. And thanks for the opportunity of going. If you can gin up another one, I'd be glad to go along again. Thank you.

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