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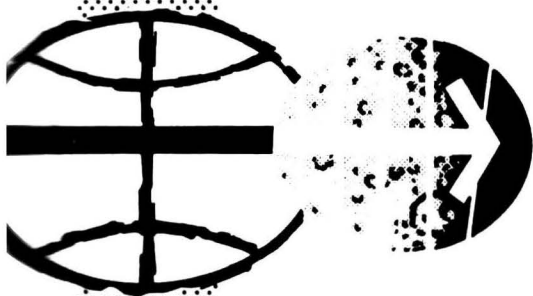


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FOREWORD

This is the transcription of the Apollo 17 Scientific Debriefing conducted at the Manned Spacecraft Center Building 30 Auditorium January 8, 1973. The Apollo 17 astronauts were Eugene A. Cernan, commander; Ronald E. Evans, command module pilot; and Harrison P. (Jack) Schmitt, 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 [?].

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OPENING REMARKS

CHAIRMAN Welcome to the last lunar science postflight debriefing that we've had, or will have for some time, at least until we get ginned up again to go back to the Moon. We'll have a review by the PIs of the experiments that we now have, the information that we've gotten so far. Let's start out with, first of all, the Heat Flow Experiment and Dr. Mark Langseth.

LUNAR SURFACE EXPERIMENTS

HEAT FLOW EXPERIMENT (S-037)

LANGSETH

I think I can start things out on a happy note. We're in the rather unaccustomed position of working with data from an experiment that is set up exactly as it was designed. The first slide is a photograph showing the heat flow setup as it is at Taurus-Littrow. The electronics box is sitting kind of top center. The probe that we call number 1 is the one in the foreground and directly west is the probe that we call probe number 2. The probes are about 30 feet apart and the best we could say in one word is that this setup is essentially perfect. I don't think there is any change that we could make to it.

The next slide shows the configuration of the probes in the subsurface. They are very nearly at the same depth. The probes that contain the thermometers are at a depth between about 1.3 and 2.3 meters. At that depth, they don't see any influence of the diurnal variation at all. There is a very small influence from the annual variation, but that is very small compared to the gradients we're trying to measure. Now we'll take a look at the temperature measurements that we've gotten.

LANGSETH
(CONT'D)

The next slide shows the temperature profiles that we've seen between 65 and 230 centimeters at probe number 1. Actually we have data here shown only up to day 357. The temperature curve that you see on the left actually changes very little up to the present time. We have plotted those temperatures and there is a change only of a few hundredths of a degree between day 357 and the present time. The probes have very nearly reached equilibrium with the regolith and will probably stay that way for a long time. The absolute temperature level is of interest. You can see it at about 2 meters. The temperature is about 256° K, or about minus 16° C. This mean temperature is about 4° higher than what we have observed at the Apollo 15 site. You would expect this since the mean surface temperature at this site is somewhat higher. We don't know whether it is exactly 4° , but that probably wouldn't be a bad guess. This was rather gratifying to us because, as you may recall, this 252° temperature that we noted at Apollo 15 was quite a surprise. Here again, we see this very high or very large increase in the mean temperature as you go down below the surface. Most of this increase occurs in the first few centimeters. The gradients that you see here actually change from the top. Between the thermocouple, which is at 65 centimeters, and the top of the probe, the gradient is about 1.77° C/m.

LANGSETH
(CONT'D)

That compares with 1.75° measured at the Apollo 15 site.

In the upper section of the probe, the gradient is about 1.5 and in the lower section about 1.1. There's a steady decrease in this gradient. We suspect that may correlate with an increase in the conductivity, so that the heat flow along there may be quite uniform.

The next slide shows the results at probe 2, which are quite different. Between 65 centimeters and the top of the probe, there is a very large gradient that is about $3\text{--}1/2^\circ/\text{m}$, whereas the gradient in the probe itself is quite uniform from top to bottom and its value is about $0.7^\circ/\text{m}$. This difference between the two probes is a little bit upsetting, but it really is the reason that we put two probes up there in the first place.

The best supposition about what's going on here is that we think there is a disturbance in the heat flow at this site. The possibility is that we put the probe very close to a large rock or something of this type which would cause the very low gradient that we observed in the lower meter of the hole. We have made two conductivity measurements at this time. We have the capability of making eight measurements. They take about 36 hours each, so they are rather slow-going processes. The two measurements we've made are

LANGSETH
(CONT'D)

at the top of each probe. The conductivity that we've measured at 1 is about 2.5×10^{-4} . That actually was the value we measured at the bottom of the probe at Apollo 15. The conductivity at the top of probe 2 is 2.1×10^{-4} . These units are watts per centimeter per degree K. We don't see any real difference in conductivity between the two probes, and we know that it is not drilled down in rock. However, if these conductivities are representative of the rest of the regolith, the preliminary indication is that the heat flow at this site will be very close to that measured at the Apollo 15 site, if indeed we can build a credible story about what's happening at probe 2.

I have three questions. This one I guess is basically for Gene. On the cable just above the bore stem, there's a black section that is actually tape wrapped around the probe cable. I wonder if you had a chance to see that and if you could report about the condition of that black surface on the cable. Did you notice any degradation to that surface?

CERNAN

Like tape peeling off?

LANGSETH

Right. We've had some experience with tape we've held here that that's a carbon black material on there that's been flaking off.

CERNAN No, I didn't see any of that at all. I was working in it and near it when I was putting the insulators down through the probe and tube and I didn't see anything different than I'd seen on anything I'd ever been familiar with before. I didn't see any flaking; I didn't see any peeling; it was intact just as I'd seem before.

LANGSETH It has a Mylar backing or it has some kind of plastic tape and there will be shiny streaks running through it. The black areas seem to be in pretty good shape.

CERNAN To the best of my recollection I didn't see anything unusual there at all. Of course, I didn't pay specific attention to it but it was in my work area and my line of sight all the time when I was working with the probe and I don't recall anything unusual about that part at all.

LANGSETH We've watched the drilling of the heat flow holes several times on replays, but I wonder if in general you could compare the subsurface as you felt it through the drill at hole 1 compared to hole 2.

CERNAN I was thinking about that just as you were showing these differences up here and I was trying to recall whether there was any gross difference between the two. I would probably have to watch the process and listen to the words as described what the surface felt like. I think here, again,

CERNAN
(CONT'D)

there was really no difference between the two. In other words, I didn't hit a hard rock to start with on probe 2 or vice versa. I think the tendency was to break through fragmentally joined pieces of rock is the best way I can describe it. I broke through those and had relative ease for 6 or 8 inches and then tended to chew away at something that didn't seem to me to be solid. It seemed to me to be fragmental, but I still had to break up those fragments and break through them. That tendency appeared, as I recall, generally in the lower sections of both bores. I would say that to the best of knowledge at this point there was no gross difference between the two at all.

LANGSETH

Both of you might comment on this last question. There is a small (about 3 meters) crater just south of the electronics box. The question is something about the nature of these blocks. Specifically, are they the instant type rock? There's some blockiness there.

SCHMITT

Those larger blocks would probably be subfloor gabbro type material, more than likely. The instant rock in craters of that size is much smaller, on the order of a few centimeters in average dimension and very angular. I never saw instant rock of that size.

CERNAN

Not in that area.

LANGSETH You don't specifically recall noting those rocks or looking at them?

SCHMITT No, not as specific rocks, but the crater was one that has a pit bottom crater and did have granularity of the surface. That's the instant rock in the crater. Both types of rocks, I'm sure, are there. The other photographs ought to show that pretty clearly. There are the pans, for example, that should show that crater. One of the pans was taken from just south of the craters so that could cross some picture there. Just generally, though, the blocks near the cable there would almost certainly not be instant rock.

LANGSETH Just one more comment, and the reason I brought this is up is that crater is about 3 meters across and it is rather surprising that it brings up blocks.

SCHMITT Well, I don't think it did. There are blocks in the vicinity and all it has to do is hit one and break it apart and it'll look that way. I don't think you hit bedrock.

LANGSETH No, I don't either. I just wondered if it reflected on the population of large rocks in the subsurface.

SCHMITT Probably, to a certain extent I would say in comparison with that to other craters you might develop some statistics to the depth of that crater anyway.

CHAIRMAN Couldn't it also be secondaries or something?

SCHMITT That conceivably could be part of the block. We saw one example on the traverse to station 2 where it looked fairly clear that the secondary fragment was at the rim of the crater, but ordinarily it was not obvious. Particularly for these larger pit bottom craters, it was not obvious where the fragment was that formed the crater. I think that when you get to the point where you get the glass-lined pits you probably have completely disintegrated any fragment. They're probably primaries or close to primary velocities.

CERNAN I've got a question (which you tended to go over fairly rapidly) about the difference in the gradient at probe 2 versus probe 1. You said it might be due to some large block in the area, but what do you plan to do with that difference? It sounds to me like it's pretty interesting.

LANGSETH Yes, it is. There's one feature of the data that I didn't point out. The thing that is suggested is the change in gradient in the hole itself, the fact that you have a very large gradient at the top and a much lower gradient at the bottom. The change there is about a factor of 5 in the gradient. However, the temperature at the top is 65 centimeters and down at the bottom of the hole is about the same

LANGSETH
(CONT'D)

as we observe at hole 1. Now, it may not be a reasonable assumption at this point, but if you assume that hole 1 were the more normal hole, then this could be explained as a local distortion in the heat flow pattern in this hole and, as I say, it's just pure supposition at this point. We think we might be close to something that's very highly conductive, which would cause the gradient to be low. The thing that is counted kind of against that is the fact that the gradient is so uniform along the probe. It's hard to imagine a geometry that would give you such a uniform gradient.

CHAIRMAN

Could your compressor just be bad?

LANGSETH

That top sensor is a thermocouple, as opposed to platinum resistance thermometers on the bottom, and its accuracy is not as great. The accuracy is on the order of about $1/4^{\circ}$; however, we have checked back on these particular probes very carefully and we find that the accuracy is very good if you use the temperature difference measurement. The thermocouples actually make a temperature difference measurement between the top of the probe and that first thermocouple. We think that's good within about $1/4^{\circ}$. I don't think you could explain the change in gradient by accuracy alone, although it certainly is not a solid.

LUNAR SURFACE GRAVIMETER (S-207)

CHAIRMAN The next experiment is the Lunar Surface Gravimeter, which we had a little difficulty with in initial stages. Dr. Weber is here to tell us what the latest information is about our experiment.

WEBER The lunar surface gravimeter employs a beam that is balanced in lunar g by adding masses. When the experiment was first deployed, all mass was added and the beam appeared to go down. One mass was removed and the beam appeared to go up. Since then, we have not been able to get the beam down by mass addition. The experiment was correctly deployed, and we thank Dr. Schmitt for doing a splendid job for us during all three EVAs. Many different reasons were advanced by the builder of the sensor, who is Dr. L. J. B. LaCoste of Austin, Texas. He is the world's authority and the sole supplier. At his insistence, the sensor was built without drawings, quality control, or quality assurance, as a hand-made job. This makes diagnosis a little difficult.

The present status is as follows. We have succeeded in balancing the beam, using forces in addition to the gravitational forces on the masses. At the moment, the instrument appears to behave normally. However, we must computer

WEBER
(CONT'D)

analyze the tapes since the results of visual examination may be incorrect. The visual examination suggests that we are getting data in the entire frequency band of interest, which is from dc to 16 hertz. I would say that at the worst, we will get 50 percent of the expected science from this experiment, assuming that the sensor was incorrectly designed and assuming that it has to be operated in a different mode from the way it's being operated right now. Now the beam is centered between the stops. Another mode is to - just don't let it rest against the top stop. At the worst, we will get the expected sensitivity science in the band from 1 to 16 hertz. I'm rather more optimistic than that. At best, we'll get all of the science we expected. The LSG is not dead and the principal investigator isn't dead either.

We had quite a bit of discussion with the crew during the EVAs, and I think we've pretty well exhausted the things that we might get other than by analyses of the tapes and study of the instruments we have that are like the LSG.

SCHMITT

Let me add one thing that will be in the pilot's report and I don't think has been mentioned; maybe I mentioned it in real time. I'm not sure. On the second time that we tapped or hit the gravimeter, there was a very small amount of

SCHMITT
(CONT'D)

dust that fell into the gimbal housing area. I can't believe it will cause a problem, but I thought you might want to be aware of that. It was on the UHT, and I saw it fall off. The first time, I had cleaned off the UHT. The second time, I forgot to clean it off.

QUERY

Dr. Weber, are you looking forward then to the LSG working nominally in the future and providing us with the information we're looking for?

WEBER

The LSG is working now. If you go up to the third floor and look at those drums, you'll see a normal-looking output in the free modes band and some evidence of seismic activity in the seismic band. Just why it's working as well as it appears to be is a matter of some discussion. I think I understand why it's working the way it is, but I won't say that all my colleagues agree with me. I'm frankly optimistic about getting most of the science. When I say most of the science, if we get science in a given range, it will be at the expected quality.

QUERY

I get the impression from what you said that you're not quite sure why it's working. You think you know, but the sequence of events that led up to its working now isn't particularly clear. Is that right?

WEBER The sequence of events is quite clear. When I say "sequence of events," I mean from the time of deployment to the present. The sequence that occurred in the builder's shop is rather unclear. What we have is a long-period accelerometer, which was intended to work as the result of a certain combination of forces. It's now working with a slightly different combination of forces than was intended. To be specific, one has about 130 grams of mass and all but 1 milligram is now gravitational. We have an extra 1 milligram of force being exerted by a caging mechanism. So the question is, does that 1 milligram of force that isn't exerted by gravity cause the instrument at extremely small displacements, on the order of angstroms, to behave very differently from a standard long-period instrument. This is debatable, but it's a question which can be resolved by tests.

QUERY Could you be a little more specific about what you're expecting to get in three modes: the seismic data, the free mode data, and the tidal wave?

WEBER Right. It's clear from the visual records that one is getting data in the free modes band, about 1 cycle every 20 minutes to 1 cycle per minute. I think it's clear that one is getting data in the seismic band, which is 1 to 16 cycles. Also, the strengths of the signals that one

WEBER
(CONT'D)

sees on the record are consistent with what you would get assuming that the sensitivity is governed by thermal noise from the mechanical sensor itself, which is a very important consideration. Now, to know whether or not we're getting tidal data will require a month's observation. To be sure that my statements based on visual examination are correct, we'll have to do a power spectral analysis of what's on the tapes. That is going to take some time. I'd again like to caution that my own conclusions are based on visual examination, which might turn out to be quite wrong.

QUERY

Your free mode channel looks pretty noisy. Is it?

WEBER

That's about what I'd expect.

QUERY

You expected it to be that noisy?

WEBER

Yes. One has a gain of over a million there. The instrument isn't being operated like a standard closed-loop servo. There was a tremendous amount of redundancy built into it: being able to short the integrator and operate open loop, and being able to operate under these conditions of gain. This redundancy enables us to make up for this kind of catastrophe.

QUERY Dr. Weber, do you have any surface gravity values in centimeters per square second.

WEBER I think Dr. Talwani has very precise information about that. That was one of the objectives of our experiment. From the way that we have been able to get the beam between the stops, one can't get a precise measurement of that, at least not in the present mode. Now, there is some possibility that the beam could be balanced by gravitational forces alone, going to lower temperatures. We're reluctant to do that while the experiment seems to be producing data because operating at very low temperatures risks damage to the electronics. But if we do succeed in doing that, we might be able to get a lunar g value.

QUERY I heard that you wanted 162.67. How many centimeters per square second did you come out with?

WEBER That was not our data. I think that was Dr. Talwani's data.

QUERY Is MSC processing the data tapes now for this experiment? Could you verify what you need to know?

SPEAKER No, I have not yet verified. Glen, have you verified?

SPEAKER Yes. It's now in the body of tapes.

WEBER We'll receive two, and those two had mainly our sequence of operations during the first two sleepless nights rather than scientific data. We looked at them last weekend.

LUNAR SEISMIC PROFILING EXPERIMENT (S-203)

CHAIRMAN Thank you, Dr. Weber. Our next PI was for the Lunar Seismic Profiling Experiment, Dr. Kovach, who didn't make it. Dr. Strangway will fill in for him.

STRANGWAY I'm just going to read what Bob wrote. He said, "Congratulations to the crew and to the program. The experiment was an unqualified success, and all charges were recorded. We can now answer some perplexing questions concerning the shallow lunar interior. The closest LSPE charge was at 60 meters distance, so there is no data on the regolith thickness. A quick look at the data reveals there's a constant velocity material of about 250 to 300 m/sec extending down to a depth of about 300 to 400 meters. At this depth, the velocity jumps abruptly to about 2 km/sec. This velocity was then constant out to the most distant charge at 2.7 kilometers, suggesting a minimum thickness of 1 kilometer of this high-velocity material. The high-velocity material is compatible with that measured on competent lava flows on the Earth. The upper material is

STRANGWAY
(CONT'D)

probably fractured and comminuted to give the lower velocities." He then cautions us not to take the numbers rigorously since these are preliminary results and since he has not yet received all of his data and all of his tapes to do these really careful analyses.

"The LM crash, 9 kilometers to the southwest, was clearly recorded on the LSPE. The recorded traveltime reveals that the shallow lunar velocity variation in the top 5 kilometers cannot be a steep continuous increase, such as first believed to be compatible with self-compression of granular material. Instead, the crustal structure or the very shallow crustal structure looks like it has stepwise increases." He then suggests that it looks rather like a minicrust in this location. I guess that will be with the minimascons we'll hear about shortly. I asked him if he had any specific questions of the crew, and he said that he felt that he had most of the information that he needed and didn't have any specific questions.

QUERY

He said that the first charge was at 60 meters and therefore didn't give any information on the regolith. What does he mean by that?

STRANGWAY I think he simply means that this very shallow regolith of a few meters at this site - -

QUERY He did not see any change in velocity until 300 to 400 meters?

STRANGWAY That's right.

QUERY The surface velocity was identical to that?

STRANGWAY No. He didn't get the absolute surficial velocity, I think is what he is saying. On the previous missions, you remember, he had his thumpers and they gave him data much closer than 60 meters. In those cases, it went down to about 100 m/sec in velocity. I think he's suggesting that if he had closer data, he might have got lower velocities in this case.

QUERY Would you repeat that paragraph where he talked about the LM impact?

STRANGWAY Okay. That was a long sentence, wasn't it? "The recorded traveltime reveals that the shallow lunar velocity variation in the top 5 kilometers" - that's about what he thinks he can sound with, with that distance range - "is not a simple steep continuous increase." In other words, what he is saying is that the velocity he got, if you take 9 kilometers as the distance, is essentially the same as he got

STRANGWAY
(CONT'D)

from the close-in distance. So he is implying that he's got a uniform velocity and not a velocity increase, down to a depth of 5 kilometers.

QUERY

Does impacting, and I can only assume 9 kilometers southwest is probably somewhere in the summit area of the massif - is that pinpointed yet, Jim? South Massif?

SPEAKER

I don't know the final word on that. Guy, do you have that?

SPEAKER

Yes, I think it was pinpointed about 9-1/2 kilometers south, just over the crest.

QUERY

My question is, then, does something the size of the South Massif bias the profiling?

STRANGWAY

I'm sure that it must bias it to some extent. And you'll notice he didn't give us any very specific velocity numbers based on that.

QUERY

The question as I recall, Dave, was what happens between the surface and where Latham was getting a lot of his data? How does the velocity curve look? That was where we were missing the data. You could draw it any way you wanted it between there. One of the proposals was it's just a straight tail-off from a very low velocity to the first velocities that Latham sees. What he is saying, as I interpret this now,

- QUERY
(CONT'D) is that you're probably got several velocities which account for this change from a very low to a high velocity. It's not just a straight-line extrapolation.
- STRANGWAY Right. That's just exactly what he is saying. It's not just simple self-compression.
- SCHMITT I think it'd be unwise to start too quickly, as he says, because when you're dealing with unknown structure between the massif and the subfloor in that area, how you interpret your velocity profiles through that structure and through the interface is going to be a difficult one.
- STRANGWAY Yes. He emphasizes that. He said several times do not take the numbers rigorously. These are preliminary results.
- SCHMITT As a matter of fact, I'm sure that he and Gene Simmons will be talking to each other. Some of the conversations we had the other day, Gene, suggest that, between the two of you, you may be able to define that structure.
- STRANGWAY Yes, I think shortly Gene will be making a presentation on some of the other results.

LUNAR MASS SPECTROMETER (S-205)

- CHAIRMAN Lunar Mass Spectrometer - Dr. Hoffman.

SCHMITT Are we supposed to call this the LACE now? You're detecting arsenic, is that right?

HOFFMAN There has been some confusion about the name of this experiment. Throughout the hardware phase, it was always called the LMS, but in-house, we had chosen to call it the LACE. I think Headquarters has officially switched over to LACE; so anytime you want to add arsenic, that's fine.

SPEAKER We've never switched over. That's always been our name.

HOFFMAN The deployment went very smoothly. However, we were a little bit disappointed when Jack picked up the instrument and started to walk away with it, the TV camera panned off onto some other scene; and we never did actually get to see the deployment. But from the words that you said that came back, we assume that it went very smoothly, and we certainly want to thank you for doing a good job. There is one question I did want to ask. From the panning of the television camera, there appears to be a boulder not too far from the instrument; do you recall about how far away it is?

SCHMITT I think I said about 3 or 4 meters in the transcript. I'll look that up for you. There is a crater to the east-southeast also, about 2 meters.

HOFFMAN How level was level on the bubble level? Was it in the center?

SCHMITT Right in the middle. Yes.

HOFFMAN Right in the middle. Very good.

QUERY Did you photograph that one, Jack?

SCHMITT Oh, yes. There are photographs of all the ALSEP equipment, I think, although not exactly in the nominal sequence. I think before we were through, we probably got most of the photographs everybody wanted plus the pans. I don't think there is a closeup, though, of the bubble. I don't think there are any 3-footers. They are all 7s.

HOFFMAN We can get an idea of the tipping of it, anyway. We proceeded to check it out in the low-voltage mode almost immediately after deployment, and everything looked okay. We kept the dust cover closed until after the last explosive package, which occurred on the night of December 17. At this time, the temperature of our ... had risen to 154° F. We had a cutoff of 160°, so we were okay. We blew the dust cover at that time, and the temperature dropped very rapidly, indicating that it had worked okay. The next day, we out-gassed the ion source for about 9 hours and reached a

HOFFMAN
(CONT'D)

temperature of about 250° C, which was a little more than we had anticipated and was very good.

The first turnon of the instrument occurred 2 days after sunset at the site. Now, sunset was the morning of Christmas day. We waited 2 days after that because on previous sites the cold cathode gage had showed fairly large excursions of pressure or bursts of pressure for some couple of days after sunset. Since we had gone up there to measure the lunar atmosphere and not artifacts that could be produced by man's having been there, we decided to wait for that time and get a good clean lunar atmosphere. We'll catch it on the second sunset. So we did turn on the experiment on December 27.

The first slide is a picture of the stripchart as it appears, and we are getting miles of it up in room 314. This shows a number of the different peaks in the spectrum being identified by their atomic mass number. If you start out on the bottom range, we have covered the mass range 1 through 4. The 4 peak, we believe, is really a helium peak that is bona fide lunar helium, native lunar helium; if we multiply the number there, which is given in counts per step per telemetry main frame, by a factor of approximately 1000 or 1200, we get something around $2 \text{ or } 3 \times 10^4$ as the helium concentration at night on the Moon.

SCHMITT That's atoms per cubic centimeter?

HOFFMAN That's atoms per cubic centimeter. Right, 10^4 . The 2 peak is molecular hydrogen; the 1 peak would be atomic hydrogen. We don't believe the 1 peak as being real, as the mass spectrometer produces hydrogen ions from many, many substances. Many hydrocarbons, water vapor, and other substances will give a mass 1 ion in the mass spectrometer. However, it's difficult to get a mass 2 ion, the H_2 molecule, from the dissociation of other molecules. However, that peak amplitude translates to something like 10^6 atoms/cm³, which is somewhat higher than has been predicted in the past for the hydrogen content of the Moon's atmosphere, so we are not certain about that one. That could still be due to outgassing of the instrument itself.

SCHMITT Can you make predictions based on the UV experiment in orbit, what it ought to be?

HOFFMAN We have not yet, and we haven't really talked to Dr. Fastie about this question, although I don't want to steal your show. You tell your story. You did see some H_1 ?

FASTIE Yes. If that's a peak, what do you call it?

HOFFMAN It is a peak in the spectrum. It is due to hydrogen ions, but we don't believe they're ambient ions. We believe that

HOFFMAN
(CONT'D)

it's coming from the dissociation of organic molecules about $10^3 \times$ - maybe 500, so 5×10^5 or something in that range - a few times 10^5 . It's too high for that. Also, these peaks have been decaying with time, which I will show you in a few minutes. But going on up the spectrum there, we see a number of other fairly good-sized peaks. Now, I emphasize that this slide is data taken very, very shortly, just a few minutes after turnon, and so we did expect that there was quite a bit of outgassing of the instrument at this time. There is a curious one at mass 93-1/2. Now, you can get a half-massed ion by a doubly charged peak. In other words, if you take twice that mass number, you get 187; and that is rhenium. In fact, that smear of ions between 93-1/2 and 91 appears to be rhenium and tungsten coming from the filament in the source of the mass spectrometer. It has remained very, very stable, and we think it's going to be a good calibration peak to ensure that our sensitivity remains constant with time.

SCHMITT

Did you expect that?

HOFFMAN

We didn't expect that. Of course, we've never operated a mass spectrometer for this length of time in this good a vacuum; we've never had residuals down this far that we've been able to see a peak like that. The estimates are that

HOFFMAN
(CONT'D)

if that is really boiling tungsten and rhenium boiling off the filament, we've got somewhere between 10 and 30 years lifetime on the filament that would produce that much material. The 78 peak has been and still remains somewhat of a mystery. It's probably some sort of a hydrocarbon outgassing from the instrument. Benzene is mass 78, but we wouldn't know where benzene would come from unless we used something in the cleaning solution. Now, that wasn't part of our data. Anyway, there are a number of other interesting peaks in the spectrum. The cluster at mass 50, again, we are not quite sure of. We thought for a while the 36 peak may be argon-36, but it has been decaying too much with time. I think you're getting the impression right now that we have a lot of work to do to sort out which of these peaks in the mass spectrum is really due to the lunar atmospheric gases, which are due to the instrument, from the site, from the LM, from the other experiments that are also present right in the immediate vicinity. Any peak that will decrease as we get into daylight will be almost certainly a lunar ambient mass peak because we'd expect that in the daytime, due to the increased temperature and increased scale heights, the ambient surface concentration of those species will decrease. So we're anxiously awaiting sunrise, which will occur about midnight tonight, to see which peaks do decrease.

HOFFMAN
(CONT'D)

Because outgassing rates will increase rapidly and should be a good tool to sort things out.

As we look at the next slide, I've taken a number of the major peaks and plotted them as a function of time from the first turn on until last Friday afternoon, which is the last glimpse we've had of it. That dashed period in the middle of the slide represents the time period when we had the instrument off because, during part of that time, there was no real-time support in room 314, and we were just playing it cautious to keep things off. But it didn't seem that we lost very much because most peaks seem to go right straight through that time period. There was a 24-hour period where there was no real-time support, over New Year's Eve and New Year's Day, but that's all right. I don't think we suffered much by choosing to turn off during that time. As you can see, the mass 4 peak is increasing very slightly, which could possibly be due to a decrease in temperature as we went on into the night further. The mass 32 peak has a very strange-looking function there. When we first turned on, on the second, we find that the instrument was a little cooler, and it could be that the outgassing rate had decreased and had increased then during that next 12-hour period. The other peaks are remaining fairly constant after

HOFFMAN
(CONT'D)

that initial drop, which we are sure is due to outgassing of the instrument itself. As I say, we are anxiously awaiting sunrise to see which of these peaks will increase and decrease. We expect, of course, most of them will increase because the outgassing rates will increase very markedly as the temperature increases, both at the instrument itself and at the site. One more day, we may have a lot more information.

QUERY

... the 28 to 44 peak. Do you identify the CO and CO₂?

HOFFMAN

I believe that the 28 is most likely CO. The 44 is certainly CO₂, but one could possibly have N₂ at the 28 mass position; however, after that initial outgassing, it appears that the mass 16 and 12 peaks, which would be made in the ion source from CO, are larger than the 14 peak, which would come from N₂. So the evidence points to the fact that it's probably CO, and the 28 peak is the second largest amplitude peak in the spectrum.

QUERY

... 44? What is the relative concentration that you assume there?

HOFFMAN

It's a factor of 5 to 10 or something like that.

QUERY

CO is that much more?

HOFFMAN CO is quite a bit larger than CO₂. On the top slide, we've got a factor of 2 or 3.

QUERY The point that I wanted to make is that we calculate that the CO₂ should be dissociated, come into equilibrium, but in equal amounts between CO and CO₂. If you were seeing outgassing of CO₂ from just local stuff, that would not be an ambient condition, and you would not get the CO. It may well be that what you're seeing there is the true ambient CO₂.

HOFFMAN That's possible; however, CO or mass 28 peak is a rather prevalent peak in vacuum systems, even those which have been well baked like this one now is, having gone through part of the first lunar day. It's a well-baked-out system. I might emphasize that these peaks are really very, very small. We have a high-sensitivity instrument. And while it looks like we've got a big mess of peaks there in spectrum, each of those peaks doesn't represent very many molecules of gas; therefore, the total pressure is probably something on the order of 10^6 atoms/cm³, which is equivalent to the 10^{-11} torr range. To those of you who have ever worked with vacuum systems, 10^{-11} torr is really getting down there quite a ways. So the partial pressure of many of the other gases may be in the 10^{-13} or 10^{-14} -torr range,

HOFFMAN
(CONT'D)

so we are really talking about a very, very clean system in terms of Earth-type laboratory vacuums. And CO is quite often a prevalent peak in those systems. Since we use a lot of stainless steel in the instrument, CO can be outgassing from it.

QUERY

But the solar ultraviolet will produce it in those quantities on the Moon, if you've got that much CO₂?

HOFFMAN

Right.

SCHMITT

Didn't we get a little bit of soil analysis that has been done so far, gas analysis? Don't we have a CO? We wouldn't know what the species was, but isn't CO₂ higher than normal? No answer? No soil people, huh? I heard that, anyway.

QUERY

John, do you expect your experiment to ... the voltages ...?

HOFFMAN

Yes, we have used this cyclic mode, as we call it, in which we change the electron energy in the ion source through four different voltages. By that process, we have essentially eliminated, at least initially, that that mass 36 peak was really argon. We don't quite understand what it is because there is a large 35 peak associated with it, and it could be that we have some HCl. Again, we don't know the source of it, but it would give us the 36 and 35 peaks

HOFFMAN
(CONT'D)

like you see there. In addition, we have also a fairly large mass 20 peak but accompanying that is a 19 peak. The water vapor peak at mass 18 is quite a bit less; therefore, the 19 peak cannot be coming from water vapor like you would find in a laboratory system where you had a very small 19 peak. You can get an H_3O ion having been formed. But that 19 peak's being very large and almost comparable to the 20 peak leads one to wonder whether or not there are some HF and fluoroine there; therefore, we can't say anything yet about the neon content, which the 20 peak also would be - or could be, I should say.

QUERY

What would you say was your limit for argon in the lunar atmosphere?

HOFFMAN

We have a peak there that's around 20 or 30 counts $\times 100$, between 10^3 and 10^4 right now would be an upper limit, which is quite a bit less. Neon, on the other hand, would be something similar, and that's somewhat less than what was predicted. But, on the other hand, the cold cathode gage having seen at nighttime about 2×10^5 , we don't know what that is. It's assumed that it probably was neon. But it may not be; it may be CO, for all we know, or it may be hydrogen.

SPEAKER Dr. Hoffman, let me clarify the statement here that Jack seemed to be concerned about. The support here at the Center was shut down, Jack, but the data at the range station were recorded. So as soon as that range tape comes in, Dr. Hoffman, we can look at the data.

LUNAR EJECTA AND METEOROID EXPERIMENT (S-202)

CHAIRMAN The next experiment is the Lunar Ejecta and Meteoroid Experiment, the LEAM; Dr. Berg.

BERG It's somewhat unfortunate that this debriefing was not scheduled a week later, because our experiment thus far has only seen lunar night. Most of the data come from particles that come generally from the direction of the Sun and tomorrow, of course, is lunar dawn. We expect to start seeing better data. Because of the lower data rate, I want to start by giving you anticipated data results so you won't think I'm disappointed in the very few events we have seen and you won't be disappointed in the very few events that I tell you we have seen. The anticipated results that I'm giving are based on two similar experiments which were flown on Pioneer 8 and 9, which have been up since 1967 and 1968 in heliocentric orbits. From these data, we expect to intercept an interstellar grain, the particle that comes

BERG
(CONT'D)

from outside the solar system, approximately once every 6 months. From those data, we expect to intercept cosmic dust particles, other cosmic dust particles that we believe are from comets, at a rate of several per day. For lunar ejecta particles, those particles that are born of impacts by larger meteorites on the lunar surface, we would anticipate from one to 1000 events per day. There have been no previous dynamic measurements of the lunar ejecta particles, but our data here are based on hypervelocity studies and on microcraters in lunar soil. The LEAM experiment sensor covers were left on until December 29, at which time they were removed by command. The reason for leaving them on for such a long time was that, because of our very low data rate, it became essential to know the background noise rate for the instrument. The background noise rate has been obtained for about 60 hours of lunar day and about 50 hours of lunar night. The noise background is very low, beautifully low, essentially zero except for one of the microphones that tends to give a small pulse every 450th frame or every 90th read-out. However, since we know when it comes and how big it is, we can easily accommodate it. The instrument is beautifully quiet.

BERG
(CONT'D)

Since December 24, we have seen 23 events that we associate with lunar ejecta particles. These are particles that have a momentum greater than 2×10^{-5} dyne/sec and that have velocities less than 1 km/sec. If they had a velocity greater than 1 km/sec, they would have recorded on the plasma sensors which begin to respond to velocities above 1 km/sec and which are about 100 times the sensitivity of the microphones. So we think these are lunar ejecta particles. To date, we have seen no primary cosmic dust particles, and the reason we say we don't expect to see them is again from Pioneer data. Essentially, all of the events come from particles near the Sun. Other than three particles out of 300, the rest came from directions near the Sun. The three are interstellar grains. There is a mechanism by which these particles come generally from the direction of the Sun. As the comet passes through its perihelion, it releases the microparticles. As the Sun evaporates them, these microparticles are released and they go into temporary orbits around the Sun or into hyperbolic orbits, in which case they are ejected in hyperbolic fashion. They would then go out from the Sun, or if they go into temporary orbits around the Sun, they would gradually spiral in towards the Sun. When they come to within a few solar radii, they partially evaporate. Somewhere along the line, there

BERG
(CONT'D)

are two forces on the microparticles. One is solar radiation pressure, and the other is gravity. So as the particles approach the Sun, they partially evaporate and the gravity drops off quicker as the radius cubed. The radiation pressure has a greater force and the particles are driven out radially from the Sun, in which case we would also see them going out from the Sun. This is why we see particles going out from the Sun, and we're sheltered from these particles by being on the shaded side of the Moon.

QUERY

That's only one species. What about all the other things that are floating around?

BERG

I mentioned the three species. One is the interstellar particles, and we expect to see one every 6 months. The other is the lunar ejecta. We have seen the lunar ejecta from the three events. We believe essentially all cosmic dust that we see in the vicinity of Earth is from comets. Of course, we would only see the cometary particles that come within 1 AU.

QUERY

Meteoritic particles climbing into the Earth are cometary?

BERG

No. I'm talking about microparticles. The big meteor particles are not affected by radiation pressure. They're

BERG (CONT'D) affected by gravity and can come barreling in through our atmosphere from outside the solar system.

QUERY Do you expect to see them on the Moon, too?

BERG Yes; from that, we get lunar ejecta particles.

SCHMITT Their frequency is small enough, even in the small micro-meteorite particles, that he's not going to see them at all. Is that right?

BERG Yes.

SCHMITT He'll only see their secondary effects. He won't see one of those unless it's extremely fortuitous.

BERG We're talking about particles that are 100 microns or smaller in diameter. Tomorrow, dawn comes to the LEAM experiment and we hope to see better data.

SCHMITT When you see your secondaries, do you expect to see them in swarms so that you can calculate the primary rate?

BERG Yes. That's the reason for the estimates.

SCHMITT You can eventually come up with a present day primary rate?

BERG Yes.

SCHMITT Will you get any idea of the energy of the primaries from the energy of the secondaries?

BERG Yes.

SCHMITT I meant the primary meteoritic particles that are giving you secondaries. For example, if they're able to date the boulder at station 6 and do some primary microcrater counting there, we'll be able to make a comparison with the apparent rate at that time versus the present secondary rate.

SURFACE ELECTRICAL PROPERTIES EXPERIMENT (S-204)

CHAIRMAN Surface Electrical Properties Experiment. Dr. Simmons.

SIMMONS The first slide that I brought along shows in a very quick way the concept of the experiment in which we have a transmitter laid on the surface of the Moon and the receiver that was attached to the Rover. The field strengths were measured as a function of distance from the transmitter. I used this slide because we have not yet seen the photographs taken on the surface of the Moon. We did get data from the SEP site along the traverse out to station 2. It is becoming increasingly clear that we do not have data for any of the other traverses on the Moon. It's not quite clear

SIMMONS
(CONT'D) to me why we did not get data because you guys turned them on and the instrument was cool enough that we should have had data.

SCHMITT Are you talking about 4 to 5?

SIMMONS I'm talking about everywhere else, other than from SEP to station 2.

QUERY Were the tapes blank?

SIMMONS That's not clear to me. I'm told the tapes have been searched and there are no data on them.

CERNAN I thought you had data on 4 to 5.

SIMMONS I thought so too.

SPEAKER There are some data from 4 to 5 but it's degraded.

SIMMONS From the SEP site to station 6, we had hoped to have data and apparently there aren't any there.

SCHMITT Gene said that the instrument was in STANDBY when we got to station 6.

CERNAN I'm almost sure that when we got to station 6 and were asked to turn the SEP off, I found it in STANDBY rather than ON, which would explain the lack of data.

SIMMONS That would explain why there's no data on the tape.

SCHMITT It surprises me that you were getting degraded data on
4 to 5. It was also on from 5 to the LM, wasn't it?

SIMMONS Yes.

SCHMITT There's apparently no data there. I don't understand that
either. It may well have been in STANDBY. That was a bad
switch to read.

SIMMONS I think the switch itself was very bad.

SCHMITT I'm surprised that you don't have data in the other places.

CERNAN It was within temperature limits during that period of time?

SIMMONS Yes.

SCHMITT Was it for 4 to 5 and 5 to the LM?

SIMMONS Yes.

SCHMITT There must be some other problems even if the switch worked.

SIMMONS That's right. The hardware guys are looking at the prob-
lems, and I'm sure there will be discussions tomorrow or
the next day on those aspects. Let's look at the data we
did get, which are beautiful. I don't want you to get the

SIMMONS
(CONT'D)

feeling by the discussion of the negative aspects that positive side is not really great, because indeed it is.

This is the 1-megahertz data for the Z-component, the east-west transmitting antenna that's placed horizontally. The distance is plotted in wavelengths, with each cross representing about 300 meters. We have data out to 2-1/2 to 3 kilometers. When we overlay the theoretical curves for a semi-infinite halfspace with a dielectric constant of about 5.6 to possibly 5.8, we match perfectly the wiggles at this frequency. We are convinced that at depths from a few tens of meters to at least 1-1/2 kilometers, there are rocks with a dielectric constant of about 5.6 to 5.8. We think we could see through 1-1/2 kilometers of this material; if there were good reflectors as shallow as 1-1/2 to 2 kilometers, we would have seen them in this plot. The long wavelengths sample deeply, the shorter wavelengths sample less deeply.

SCHMITT

That also implies at the scale of 200 to 300 meters in that range that the medium is uniform. Is that right? That there are no local boundary conditions that would cause noise?

SIMMONS There are no large blocks comparable to a wavelength in scale that your profile passed near. We would have seen extra bumps in the plot that we just looked at. On this plot for the 16-megahertz frequency, you can see there are many peaks and troughs. They are not quite as regular in spacing as the ones that we saw on the 1-megahertz data. As seen from the spacing of the peaks and troughs, a dielectric constant for a shallow layer of something in the range of $3\frac{1}{2}$ to about 4 fits reasonably well. We infer from that observation that there is a rather shallow layer that does in fact have the dielectric properties that would have been expected and that we did expect for the outer lunar material, the soils. We are having a difficult time, however, in reconciling the models that we can fit to the 16-megahertz data and fit to the 1-megahertz data.

SCHMITT What's your wavelength there?

SIMMONS At 16 megahertz, it would be 20 meters, roughly. So 20 meters times 30 to get you out. This is about a 600-meter profile. The data go on farther, but we chose to quite plotting at this point. We have data that look quite usable out to some 3 kilometers from the SEP site toward station 2. The next slide shows a theoretical plot for a dielectric constant of 5.8 and a loss tangent of 0.06.

SIMMONS
(CONT'D)

The loss tangent is somewhat higher than we had expected for the Moon. On the other hand, it's not at all unreasonable on the basis of some of the previous measurements made on lunar samples and on terrestrial basalts.

The next slide shows the effect of putting a thin layer over the material of 5.8 dielectric constant. This thin layer has the dielectric constant of 3.2. It's only a tenth of a wavelength thick, and you notice that the wiggles disappear. It was to this phenomenon that I was alluding when I said we were having trouble reconciling the two models. The plot here would be for corresponding to the 1-megahertz data with a very thin layer of a tenth of a wavelength (30 meters thick). Our wiggles disappear. But they did not disappear in the real data. They disappear in our models.

That's essentially where we are at the present time. We do feel that the data we have along the first 3 or 4 kilometers are, in fact, usable, and they are beautiful. We are sorry that you had so much trouble tending to the temperature problems on the instrument. But, nonetheless, we fully appreciate data that we got, and we're really excited about it.

SIMMONS
(CONT'D)

When you started from the SEP towards station 2, I believe you drove fairly slowly and in a straight line. Do you recall about how long you continued in this straight line?

CERNAN

Past the end of the antenna, you mean?

SIMMONS

Yes.

SCHMITT

I'm trying to think of where we went when we went by the LM. I don't remember which side of the LM we went on.

CERNAN

My feeling is it had to be relatively straight, at least to where the LM was.

SCHMITT

I think I took some pictures. I think we went left of the LM. I think that was one of the few times we went left of the LM. You wanted to get over on the other side of the ALSEP.

CERNAN

We did go to the south of the ALSEP. I don't know whether you've looked at those pictures or not, but I suspect that there will be two or three traverse pictures that might give you the direction of where we headed. I hope I started taking them.

SIMMONS

I'm asking whether you were generally going in a straight line and at what point did you deviate.

CERNAN Generally, we were going in a straight line, but we did, obviously, deviate. I don't remember whether we went left or right of the LM, but I know we went left of the ALSEP.

SCHMITT You can get it out of the ascent photography, because it shows your antenna cross. If we did go left of the LM, it's going to be generally a straight line. It would just be a curved line, and it'll show up on that. I think that'll be the only time we ever went left of the LM, if we did. Otherwise, we went right.

SIMMONS That's a good point. When we compared odometer readings with geologists' estimates of where you were, there was something like a 10-percent difference. That may be caused by any of a number of things. The geologists could have been mistaken.

CERNAN These are tape odometer readings?

SIMMONS Yes.

SCHMITT Are you getting range or distance off the tape?

SIMMONS Odometer at the moment, mainly.

SCHMITT You mean nav data?

SIMMONS Yes. But my question relates to how long you went at a fairly modest speed from the SEP site toward station 2 until you went balls out. Perhaps the slippage may not have been occurring during the early part of the traverse in close to the SEP but may have occurred after you speeded up.

CERNAN We started going full throttle probably after we got by the LM ALSEP combination. Full throttle was anywhere down the slopes from 8 to 12 clicks. I would say we were approaching that even in the area going by the LM and the ALSEP. I'd say fairly rapidly after we departed SEP. We were going at that time from 10 to 12 clicks, and later on, slowing down because the slopes were around, and that's all we got out of the Rover. You're talking about wheel slippage though.

SCHMITT You're also aware that the geologists' numbers would be based either on range or distance in the nav system which have different ways. I keep forgetting whether one turns over at every half kilometer and the other turns over at a tenth before the kilometer, or something like that.

CERNAN My feeling of slippage on the Rover is that the only time we had real wheel slippage was when we only had three wheels on the ground, which was a good part of the time. As far

CERNAN
(CONT'D)

as total wheel slippage when you've got firm contact with the ground, I personally felt we did not have a great deal of that.

SIMMONS

We're only talking about 10 percent.

QUERY

How do we stand on the Goddard tracking, Gene? Has that been checked out?

SIMMONS

I don't know.

QUERY

You don't know whether it has been checked out, or that we don't have it?

SIMMONS

We don't know either one.

QUERY

Do you think they've got some good track, Don?

SPEAKER

Yes, as far as I can tell.

SPEAKER

It won't be here until the end of January.

QUERY

Gene, I've noticed that you showed that you were getting an east-west antenna transmission. I was under the impression that the traverse tended to annul the east-west antenna.

SIMMONS

Both antennas worked. Both antennas give data, and I simply picked two out of 36 components to show you. Half of them

SIMMONS
(CONT'D)

are just as good and spectacular as the ones I showed. The other half aren't quite as good, but, nonetheless, every one of them is usable.

QUERY

Gene, the other day you said you thought you saw a reflective horizon about 200 meters deep. You didn't say anything about that now. Is it still the same?

SIMMONS

Yes, I did say something about it in the sense that we have a layer of about 3.4 dielectric constant over something below 5.8. That's essentially a reflecting horizon.

QUERY

You are still working the depth?

SIMMONS

I'm still working the depth, and I'm still working the reconciliation of the model to fit all of the constraints.

TRAVERSE GRAVIMETER EXPERIMENT (S-199)

CHAIRMAN

Next is the Traverse Gravimeter and Dr. Talwani.

TALWANI

We're very happy with the traverse gravimeter experiment, and I'd like to thank the crew and express my appreciation. We're really very happy with the things that were going on with the experiments. The first measurement was, of course, the Earth-Moon tide. We have a value relative of the absolute value determined at Cambridge. The value on the Moon was 162 694. It might change just a little bit by

TALWANI
(CONT'D)

1 or 2 milligals, but we believe this value is good to a maximum error of 5 milligals. We call that a big success. Using value of radius and a value of GM that's well known, we predicted a value that was about 20 or 30 milligals off this. I guess that radius happened to be a good one; this told us that, getting to within 20 milligals of what we expected, everything was working right, and the bioshift was just 2 or 3 milligals. Therefore, when the instrument landed on the Moon, it was fine. All through the mission, everything was all right.

The only problem we've had is a distance of about 5 milligals between readings on the Rover and readings on the ground. We don't understand this, and there are some tests that we'll still do on the engineering models. Since this definitely was consistent, what we have done is to simply added 5, and this seems to bring everything into shape.

CERNAN

That's collected for the difference in height, of course.

TALWANI

Yes, the difference in height. We do not believe it's the motion of the Rover. I was going to ask you of any motion on the ground of the instrument that you noted, but I thought it would be very difficult for you to note any.

CERNAN

Motion on the ground?

TALWANI Yes, just a sinking or anything of that sort?

CERNAN No, other than the fact that those ground measurements are on the LM where there was a lot of activity, of course, of us walking in and around. The only other place specifically on the ground that we had to take it off was up at station 6 because of the potentials of the slopes. I forced it into the ground to keep it from falling over because the slopes were that great, right there. I think it was pretty stable. I don't see how it could have settled any more. We were gone from that area, so I can't think of any different perturbation.

TALWANI The difference is that it's lower on the ground. It's very surprising.

CERNAN Are these based on measurements near the LM?

TALWANI Yes. You also made one at another station, specifically to check this.

CERNAN Was that station 8?

TALWANI I believe it was station 8.

SCHMITT We took one off one or two other places.

CERNAN Yes, but we didn't make a measurement on the Rover. I'm talking about a comparison.

SCHMITT Was it station 8?

TALWANI I believe it was 8. You made one off and one on. Again, it was 5 milligals off. You have this problem, but it seems to bring everything into line, so we feel that for the present time, anyway, we're going to just add 5 milligals to ground values, which were just a few, and use normal values.

CERNAN Ground is lower than you expected?

TALWANI There's no reason that we could think of. Your first picture just simply shows how we set about interpreting these things. We have just taken a line from the South Massif towards station 8A and projected all the values onto this line of cross section to make a simple two-dimensional interpretation.

CERNAN How do you know which is the correct value, if you bias it?

TALWANI The whole thing is relative, so it doesn't make any difference. Our interpretation is just to the value of the spacecraft. Either you add 5 from those or subtract 5 from the others. The first picture is just a map showing where the

TALWANI
(CONT'D)

values are, and so what I say next, I just projected all these values onto this line. We haven't used 6 here because it's very close to the Massif, but it comes to a funny position on the line. A more thorough analysis was used there. The location is funny. You cannot use a two-dimensional approximation since it's close to the Massif, but if you projected it onto this line, it would appear to become distant soil. It's just geometry, nothing else. We simply say South Massif and the measurements and then the North Massif, and we've used this line of topography that you see to make our corrections. Then you go back to figure 2, which simply shows the measurement of station 2 on the left to station 8A. The observed anomaly is, without making any corrections, the measurement that you made. You can see quite clearly that, as you approach the North Massif, you have 50 milligals lower than the LM site. At South Massif, you have about 30 milligals lower than the LM site. This 50 milligals lower has got us quite interested and excited, and that's why we asked you for an extra measurement at 2A. As you'll see after all the corrections are made, that's going to be important in telling us where the edge of the high-density body is. I'm sorry; it should be 50 milligals on the South Massif. Left is to the south. Now the first correction we applied is the free anomaly,

TALWANI
(CONT'D)

which is simply the effect of being farther away from the center of the Moon. If you make that correction, you are minus 30 milligals at station 2 and about minus 20 milligals at station 8A at the eastern end, or this northern end.

Please skip to figure 4 where we make a few other corrections, and there are some dotted curves. One dotted curve is the effect of the South Massif. Even though no measurements are made on it, it's so close and it's so big and massive that it makes a large correction of about 15 milligals. At the same time, if you look at the top dotted curve, this is the effect of the valley floor. That's simply the elevation of the station and the excess mass between the station and IM elevation; you'll see that's up to 10 milligals. The third dotted curve is the effect of the North Massif. When you put all three together, it turns out that the total correction is very small. The last curve was just above the free air curve, and this is the final curve that we will use to make our interpretations. It's about 25 milligals down on the South Massif and about 20 milligals down on the North Massif. This is a two-dimensional calculation, and they're very rough; thus, it has become very clear to us that the thing will change by at least 4 or 4 milligals when we make the three-dimensional correction.

SCHMITT I was going to say that I'd expect the North Massif effect to go up when you go to three dimensions. Won't it?

TALWANI No, it'll actually go down because we now assume it's infinite in this direction, but it's really limited, so it will go down. The value will go lower. I'm really not sure which way it'll go. We'll also be able to use station 6. Station 4 has an unusually high value, though it's one station, so I don't pay any attention to it.

SCHMITT You heard about the core-tube data?

TALWANI It's just simply one value. For whatever it's worth, it's a high value.

SCHMITT Well, the lower core tube apparently had high density - anomalously high - 2.5?

TALWANI No, I didn't hear about that.

SCHMITT Well, whatever the number is, it's higher than you expect for core tubes.

TALWANI I didn't know about that.

SCHMITT So there may be some dense materials around station 4 then.

QUERY It's hard to say just because of one station. We have so few stations anyway. Why do you say you're ignoring that one?

TALWANI I am not ignoring that. I wouldn't want to make a big deal out of that, but that other data set is the one you're talking about. Once you get different kinds of data, you see a lot more. The main thing that we see is the systematic thing going down to either side. Now, that certainly means that station 4 is high; it's a good value, and we'll use it. Station 5 is very close to the crater and that will have some effect. Again, the curve might also change when we make these corrections.

We have to use some densities to make some models, and we've looked at older density determination in terms of three kinds of units. The values we used were at 11, 14, and 15. And the breccias had density values between 2 and 2.5. The basalts had values averaging about 3.2; and the anorthositic gabbros had values, I believe, about 3.0. The contrast between breccia and basalt is 0.8, which we have used. That's a very high contrast. I think when we get the samples and make some density measurements, we might be able to make an estimate of what density contrast to use. In any event, given a certain density contrast, we are able to predict

TALWANI
(CONT'D)

the thickness of this material. And if the density contrast is 0.8, we have a 1-kilometer-thick layer of high-density basalt. If, as is perhaps more likely, the contrast is 0.5 or 0.4, then we have 2 kilometers or more. This certainly says then that the Taurus-Littrow Valley floor is underlain by high-density basalt. This is what made up the valley. Then it becomes a matter of interest and speculation whether these basalts were implaced at the same time as the Serenitatis mascon. Certainly, this is to me direct evidence that this mass filling by basalts has taken place.

SCHMITT

I think you've gotten 3.4 on the subfloor gabbro? Since the breccias we sampled are really fine-grained crystalline rocks, I think that the density there is probably up closer to 2.6. So that's going to lower the density and will have a contrast of about 0.6, something like that. So you're probably on the right track.

TALWANI

Now, if you look more carefully at the western edge, we had to put it not at station 2, but just west of station 3. We're going to define these models as we make three-dimensional calculations. It might turn out that the scarp at station 3 is really the more important structural feature than the edge of the massif at station 2. This model is just one we quickly calculated, and we are happy to find a

TALWANI
(CONT'D)

little more that gives you the sense of why having more stations there will be useful to us in the last and now the final analysis of the stations.

CERNAN

It performed on the Rover, I must confess, better than I personally had ever hoped it would. Even at some of the slopes that we were parked at in the station 2 and station 3 areas. Apparently, it compensated well and gave you the information you wanted.

TALWANI

Was it very difficult to get it off at station 6 where you parked at about 20 degrees?

CERNAN

Everything at station 6 was difficult because of the slope, everything we did. It was just difficult standing up and doing anything around the Rover. But as far as ever taking the instrument on and off, it was as simple as we anticipated. However, had we had to take the instrument off at every stop, the difficult thing would have been to press the buttons and read it because that type of operation on the lunar surface is difficult.

TALWANI

On the last reading, I believe, you lifted it up and read it. Did you punch the buttons; did you read it every time? Did you read it on the ground, or did you read it every time holding it up?

CERNAN

We didn't take that many ground readings. I think most of the time the ground readings were read on the ground. The last time was the last time, and READ was punched on the surface, and then I picked it up to read it. For two reasons: number 1, to bring it to where I could shadow it from the Sun so I could read it; and, number 2, it's much easier to pick it up than it is to bend down and read it. Just bending down is a process you have to learn on the surface. Unless you've got a crutch of some sort, it's a difficult process. So, any time you lean down to punch something as low to the surface as the gravimeter was when it was on the surface, it's a difficult thing to do. So I'm really glad it worked on the Rover.

QUERY

You're not going to get your final station ... for quite some time, but did you change any of your values by more than 1 or 2 milligals?

TALWANI

Part of the problem is there is a slope to the whole plan. Simply from the fact that I get nearly the same values at the south and the north end, I'd certainly be surprised that they'd change very much. But you may get a tilting of the whole thing. That might be more important really than the elevation is.

QUERY How far did the instrument go on that last ...?

CERNAN How far did it go? Not nearly as far as I anticipated it might. I was more concerned with the ascent stage of the LM.

SCHMITT As I recall, when I was over there the other day, you were starting to see (at long numbers of wavelengths), some noise or potential echoes from other structures. Do you still consider that as a possibility? That could relate to the structure that Dr. Talwani has seen with gravity. It could relate to echoes off the South Massif subfloor contact. Is that possible?

SPEAKER I think it's possible. But when we get out to the end of those traverses, we've got a lot of data processing to do.

SCHMITT But there is a potential of that kind of information in your data?

SPEAKER There is potential.

SCHMITT I'm just trying to think of all the ways that we may end up being able to pin down the structure of the valley.

TALWANI I believe that's what Bob Kovach got ... nicely with this thing. He had a surface layer and a high-velocity layer. This high-velocity layer was probably the top part of the high-density layer.

LUNAR NEUTRON PROBE EXPERIMENT (S-229)

CHAIRMAN Next on our agenda is the Lunar Neutron Probe Experiment, which was successfully emplaced and extracted. Dr. Burnett.

BURNETT The data processing phase of our experiment is just getting underway, so I don't have a lot of startling conclusions to report. The deployment and retrieval of the neutron probe was entirely nominal, if not indeed perfect. The probe was inserted to the full depth, so it means we will have data down to a depth of about 2.1 meters. From the preliminary examination of the material in this deep-drill stem, it's known that the densities are running very high; typically, numbers like 1.8 g/cm^3 , which means that we should have data down to 375 g/cm^2 beneath the lunar surface. This means that we should be able to define the profile neutron flux very, very well from that. We've had the probe back in our hands for about 2 weeks now. This time has been spent primarily in disassembly and detailed documentation of the detector materials that are in the probe. We document these things very thoroughly, so there is a permanent scientific record of every little piece that came out of the probe that posterity will have at their disposal when we get done with it.

BURNETT
(CONT'D)

Everything on the inside of the probe appears to be in very good order. None of the temperature indicators, of which we have four at different positions along the probe, were tripped. The lowest one was set to go at 140° F. It had not turned, so the temperature in all parts of the probe, as best we could measure, was less than 140° F, and we are very happy about that. All our target materials, particularly the boron targets, survived perfectly. There is no indication of cracking and peeling. This is an item that we were somewhat concerned about, but these have come through very, very well. The neutron probe has two parallel target detection systems. One of these uses boron-10 with a cellulose triacetate plastic as the detector material. The second system uses uranium-235 as the target material and mica as the detector material. Our initial data processing has been in terms of the mica detectors because these are simpler to handle. At the present time, we've processed two pieces of mica from depths of 125 and 185 centimeters below the lunar surface. We have tracks. The cosmic rays weren't shut off during Apollo 17. We can verify that. The track densities are close to what we anticipated for these depths. We need a good postflight calibration ... and put these in terms of absolute capture rates, but they appear to be just right in line with what we expected.

BURNETT
(CONT'D)

We've also looked at some of the areas of the cellulose triacetate that were not exposed to our targets on the Moon but were carried along and saw the fast neutron background from the RTG during the flight to the Moon. The fast neutrons can interact directly with the plastic and leave a residual background and tracks. That background appears to be even lower than what we had anticipated, so we look in fairly good shape there. What we can do at the present time, as far as data analysis goes, is compare the relative values of the track densities that we've seen in the mica, for the two different depths at the deeper parts of the probe. The dropoff we observed between 125 and 185 centimeters is like a factor of 1.7. Theoretically, we would have predicted the dropoff would be about a factor of 1.5. That's quite close. Perhaps the cosmic rays have been accumulating somewhat faster in lunar material than what we might have predicted theoretically. That may mean that, sometime in the future, we won't have to dig in quite as far for our lunar base as we might have expected. Nevertheless, this is quite close to what we were anticipating. In conclusion, I think things are looking very, very good at the present time. I think the neutron probe will deliver all the data conclusions that we anticipated from it.

- QUERY What were the track densities that we expected to get?
- BURNETT We were expecting 900 at the one depth, 900 to 1000 per square centimeter. We have, it looks like, 880.
- BURNETT You felt it necessary to insert the probe through the treadle in the hole. Did that mean that there was sort of a cavity around the top of the hole of some sort?
- CERNAN I didn't feel it necessary because I didn't think of it, but after I was reminded of it, I thought it was an excellent idea. I think we might very well have lost the neutron probe. The area that we were working on was very well tramped down, obviously; and we had to go to some extent to save the hole. Once we got the core retracted, the hole stayed intact, although it did tend to flare out. I think that the handle of the upper section of the probe probably wouldn't have gone more than 6 inches to a foot, but I think it would have gone.
- BURNETT How much was this flaring?
- CERNAN A couple of inches maybe. You see, the material is just nothing but ... mantle. It's just really beat up, that we were working on.
- SCHMITT That upper 5 centimeters is so very, very soft.

CERNAN In keeping track of the hole, the hole got into a short depression because of our footsteps and everything around there, it got in shadow. I had to put a mark on the ground so I knew where the hole was to look for it. The hole, though, in spite of this flaring on the top of it, looked to me like a very well intact hole. The neutron probe went in about a third of the way and then I did contact just a sukosh of resistance, just a sukosh. Just a little force, and then it seemed to break on through, and from there it was just letting it down.

BURNETT So this flaring was on the order of inches and not feet?

CERNAN Oh, no, no. It just flared on the top of the hole. Just enough where you'd say, "I'm not sure whether the handle would have gone or not," but my guess is it probably would have gone to several inches.

BURNETT In retrospect, it might be worth noting that the X-ray examination of the deep ... have indicated that, in the second section down, it is almost entirely composed of rock fragments up to centimeter size. Terrestrially, when you drill this kind of material, it is very, very difficult to keep it from backfilling. Even allowing for the fact of 1/6 g, it was a very, very skillful job of hole drilling.

QUERY When will we see the ... results, Don?

BURNETT We don't have the capsules yet to Marty. Those will probably go down this week sometime, and he'll need I would guess a month or so to process them.

COSMIC RAY DETECTOR EXPERIMENT (S-152)

CHAIRMAN Next on the agenda is the Cosmic Ray Detector Experiment, one which was put on late, but I think we have some good results. Dr. Walker.

WALKER This experiment has the distinction of being the last experiment to be accepted, and it was accepted after the last deadline. It consisted of a small box that could be separated into two parts: one to be hung in the Sun, and one to be hung in the shade. In the boxes were various detectors including mica, glass, plastic, and platinum foils. These detectors are meant to do different things, and they will in fact be studied by different groups. The platinum foil will be studied by Geiss, the glass by Fleisher at GE, the plastics by Price at Berkeley, and the mica by Washington University. The different parts of the experiment were, first, to try to measure the abundance of a heavy solar wind ion. With a Geiss-type aluminum foil experiment, you can go up to argon; in satellite measurements, you can go

WALKER
(CONT'D)

up to ion. Then the abundance of elements drops precipitously, and the upper two-thirds of the periodic table is essentially untouchable by these techniques. An attempt to fill in the upper two-thirds of the periodic table was the major thrust of this experiment, and this consists of looking at extremely shallow tracks in the mica detectors. Another goal of the experiment was to try to decipher the mysterious region of energies in the cosmic rays, a region below 20 MeV/nucleon, during times of quiet Sun. All these objectives required a quiet Sun, and I'm happy to report that the Sun did in fact cooperate, though it started blurping a little bit at the end. I got very nervous, and the experiment was brought in at the beginning of the last EVA instead of at the end. I would not change that decision now. We've spent most of the time since we got the detector sawing it apart and documenting the various detectors. Because of an unfortunate experience on Apollo 16, on Apollo 17, we had all the screws epoxied in and everything so solid that we had to saw it apart to get the detectors. We were also encouraged to put it together solidly because of the unusual nature of the experiment being added at the last minute. We do, in fact, have some data. We just took out a test strip of mica and ran it in the few

WALKER
(CONT'D)

hours just before I got on the airplane yesterday. You won't be able to see these slides very well.

In the thing labeled phase contrast, there is a fine background of shallow pits that show up beautifully in the microscope, though not in that particular photograph. They essentially completely filled the field of view. These are the predicted solar wind heavy ions. The prediction was made some 6 years ago that you should see such things on mica exposed on the Moon. That was expected. We fully now believe that we can get the gross abundances of elements in the periodic table.

What was unexpected are what you see. They're labeled, "New particles," which are bright diamonds on those photographs or somewhat deeper pits or longer tracks or more energetic particles. During times of quiet Sun, if you had projected the best estimates through 2 orders of magnitude down to the energies that we see here, you would have predicted a much lower density of these kinds of deep pits. These are a mystery. We're looking at an energy range and a mass range that has never been looked at before. Whether these represent some kind of a very residual activity of the Sun that's always there or was associated with the activity that there was an active spot on the Sun

WALKER
(CONT'D)

or whether they represent a suprathreshold component to the solar wind, we don't know. However, those bright diamonds are unexpected, and we feel we have two for the price of one. We've got the solar wind, we believe, and we have now this other phenomenon to study.

There were a lot of things that could have gone wrong with this experiment. In particular, the astronaut could have put his grubby glove on the mica detector. That did not happen. The detectors are absolutely beautiful. The lack of dust is absolutely astonishing, just no dust on the thing at all.

SCHMITT Did you get the photographs of the orientation? There are photographs of both pieces.

WALKER I appreciated those photographs, Jack, very much. I have two questions. As far as you know, did the position of the detector in the Sun stay essentially constant?

SCHMITT Yes, I'm sure it did. It was perpendicular.

WALKER The one other question I'm embarrassed to ask, but I feel impelled to ask just to set my mind at ease. The shaded part never really got in the Sun at all, did it?

SCHMITT No, sir. Never once.

QUERY Was there a control detector onboard, or if not, did that create it?

WALKER No, absolutely not at all. There were the detectors, of course, in the shade and the detectors in the Sun. Therefore, you'll be able to distinguish between anything that would act like the solar wind that wasn't. Such a thing can be radon. In fact, another subsidiary goal of the experiment is to use the mica in the shade as a radon detector, because the radon decays would give similar pits. But I think there's plenty of controls, and we understand the problem well enough so we're not worried.

QUERY Could you give us any idea of what the energy and mass range of these little strange particles are?

WALKER Yes. I know the mass range. They certainly must be heavier than neon, and they're probably no heavier than iron because of the abundances of the elements. They're mostly iron particles. The energy ranges we're seeing are somewhere between 20 and 200 kilovolts as probably the maximum energy as so far seen in the quick scan under the microscope, with the bulk of the particles in the 20- to 50 kilovolt region. That's per nucleon.

LUNAR GEOLOGY INVESTIGATION (S-059)

CHAIRMAN Next on the agenda is the Lunar Geology Investigation;
Dr. Muehlberger.

MUEHLBERGER I'd like to point out that we have a complete set of the photographs, and most of your experiments are in those pictures somewhere. There are about 2389 pictures, which is a new world's record. As you are aware, the traverses went almost nominally. Therefore, there's a wealth of data returned geologically. I think most of you have copies of the report that we produced by splashdown, which represented our stage of knowledge at that time. It's the most nearly "scientific" report that we'd been able to produce by that moment, primarily because of the accurate descriptions of the materials that were being collected and observed. We've already spent a week or so with the crew and the tapes, and listening to their comments again, and asking questions on debriefing kinds of things. The units that were mapped there premission seem to hold pretty well. There are still some questions as to how we interpret some of the dark mantle and subfloor relationships. And the massifs are still there, and the scarp was still there, and bright mantle was still there, so I suspect that in those kinds of details, you can see our report as our level of ignorance at that time.

MUEHLBERGER (CONT'D) Initially, when you were in the LM/ALSEP area, you started talking about mantling. Would you want to add any comments as to how continuous was the mantling? Were all of the rocks filleted? I checked around the obvious, very young, fresh craters, which you did comment on in real time.

SCHMITT My impression was that filleting was there. There was a little bit of a ramp up to the edge of most of the larger rocks, but it wasn't a major feature of the rock. Where there was a sloping face of less than 30 or 40 degrees, then that fillet tended to climb up the side of the rock, so that contact was a gradational one. For the most part, there weren't many of those slopes like that. There were some low slopes of the block. The blocks generally were angular and blocky and had fairly steep faces at the contact with the mantle. However, let me also add that where there was a flat surface that was only a few centimeters above the ground, it tended to be relatively clean. Only the little depressions on that surface and cracks had obvious fragmental debris in them. Is that about your impression?

CERNAN I guess I'd always been a little bit impressed with the draping, as I call it, of the general mantling over the entire area than Jack was. I'm talking about individual blocks on a gross valleywide scale. Like Jack says, unless

CERNAN
(CONT'D)

there was particularly a down slope, you could see definite buildup of filleting and direction to it. I probably could not put a directional attitude on the valley floor.

MUEHLBERGER No directional things anywhere on the floor?

CERNAN

I got an impression that the whole floor had been dusted or draped (except the obvious places we've pointed out) to some degree or other, depending upon the size of the rock or boulder that was exposed. Everything was somewhat draped, mantled, dusted over to some degree throughout the whole valley floor. That was the impression that stuck with me. There are very few, if any, places that I can remember (except around a couple of those craters we talked about) where you get a feeling that any of the fragments that were on the surface were literally on the surface and not somewhat submerged within that surface. I don't think Jack feels quite as strongly about it as I do.

SCHMITT

We're talking about different scales, and I agree with you. If you talk about a crater scale, we've commented continually about the appearance of the crater rims that were draped. You had streaks of material that partially buried portions of the block fields on the wall. But when you came to an individual fragment or boulder, most of them were clean unless they were low to the ground or had a low

SCHMITT
(CONT'D) slope. Then they started to get dirty. But I think most of the fragments are clean. That doesn't mean that they didn't look as if they had an innerblock cover between them.

CERNAN I didn't have the impression that the fragments were dirty except at Van Serg.

SCHMITT Were you talking about a covering on top of them?

CERNAN Yes.

SCHMITT I might temper what you say and say that I wasn't quite as sure as you were. I felt that I would have to walk a long way across that valley floor to go over to an average-size rock and pick it off the surface and say it was literally laying on a covering that was on the valley floor.

CERNAN I agree with that. All I'm saying is that if there ever was anything on the rocks that eventually ended up on the floor that partially buried them, that was gone for the most part.

MUEHLBERGER I think probably in most areas there were no piles of the material in the crevices or convex areas of the rocks.

SCHMITT There was the feeling of partial burial of the rocks. I agree with Gene completely. I think we're saying the same

SCHMITT
(CONT'D) thing. Very definitely you had that impression. The blocks were projecting out of something.

CERNAN And that was not local. That was valleywide.

SCHMITT At Van Serg, I did have the impression that the blocks were dusted. That was the one place that there was something on the blocks. It was hard to see the texture of the rocks. It was the kind of rocks we're dealing with, the fine fragmental breccias or soil breccias. But there still was this feeling that they hadn't been completely cleaned off like other blocks in the area had.

CERNAN My other impression to go along with that is that I think most of those rocks I'm talking about that were generally buried were rounded to subrounded rocks, whereas around Van Serg and a couple of other unique places, they became dominantly by comparison more angular to subangular. Wouldn't you say that?

SCHMITT I think the terms you're using are relative. The blocks at station 5 are not conglomeratic rounded boulders. In the large scale, they're quite angular.

MUEHLBERGER But the corners are all rounded?

SCHMITT The corners are round. I think when you get to station 5, you begin to get more sharp corners.

CERNAN You begin to get specific about Van Serg and Camelot and some of these areas. I was just talking about in general across the valley.

MUEHLBERGER How about dark mantling materials on the massif walls or in the valleys? Was there anything visible to you there? Any blocks or anything? On the cleft and a little bit above the scarp on the North Massif, there are some very dark areas on the premission photos. I was wondering if there are any observations you could remember.

CERNAN I never saw anything but gradational change from dark to light at the contact with the massifs and the valley.

MUEHLBERGER That's the valley floor?

CERNAN Yes.

MUEHLBERGER And the cleft?

CERNAN Yes, it looked a little darker, and we commented about sunlight. But the wrinkle-face texture on the Sculptured Hills was due to an obviously darker albedo or darker reflective surface within the crests of those wrinkles. But on the massifs, the answer to your question I'd have to say was no.

CERNAN
(CONT'D)

The change in the albedo (which made it change from a generally light color to sometimes a very bright reflective white in areas) very intuitively can't be proven. Maybe your 500s will give you a better idea of it. I have the feeling it was just ready to burst out with an outcrop; however, there was no evidence of an outcrop.

MUEHLBERGER The brightest areas would be extensive outcrops?

CERNAN

It's like you took a piece of sandpaper and sanded the South Massif down, and you started to begin to hit hard spots. Where you hit hard spots, it generally had a lightened pattern. But it didn't quite expose any outcrop. That is just an intuitive descriptive feeling of what I think that change in albedo was.

SCHMITT

My feeling was that you were dealing with both differences in the topography along the slope that changed light to dark plus areas that may have had movement on them more recently and were lighter colored. That may go along with what Gene is saying. They're a little closer to fresh outcrop.

MUEHLBERGER I think you've answered my question then on station 8.

Permission maps have that still in dark mantling material; and, obviously, the albedo things were so subtle you wouldn't have spotted it.

SCHMITT That change was gradational. There is one thing I think that is illustrative of that. There is a medium gray layer on the light mantle, once you get below this. We did that primarily at 2A. I noticed it in the transcript; I'd forgotten all about it. Gene kicked up light stuff very briefly at station 2 as we were getting a rake sample, but we didn't pursue it. At station 3, there was a light gray over the marbled material. It's just that there seems there's a layer of variable thickness, let's say 5 to 10 centimeters in the intercrater areas, on the light mantle. That coloration was gradational right up onto the talus slope at station 2. That may be the local regolith development through patination and impact glass and this sort of thing, or it may just be related to other dark mantle dusting over the whole area. I don't know the answer to that, and only the samples, I think, will give the answer.

MUEHLBERGER Is that upper 5 centimeters lighter than the marbled material below, on the average, or would the upper 5 centimeters average out to the mixed marble?

SCHMITT No. No, it was about the same gray as the gray in the marbling. It's a medium gray, in contrast to a little darker gray for the dark mantle and a very light gray for the light mantle.

MUEHLBERGER As you drilled at Shorty, you observed it was darker and thus could easily spot it. As you drilled away from Shorty toward Victory, you should have been fairly close, if not crossing, one of the dark rays out in there. Did you observe that at all? You were going sort of up-Sun so it would be bad seeing, but you didn't, huh?

SCHMITT I never noticed a major change or any change in albedo from Shorty towards 5.

CERNAN You're too close to them, unless you can stand off like we did one time approaching the light mantle from the LM toward station 2. As we got closer to it, we were on a rise and we sort of stood off, and we were relatively close to it. I was surprised we could see the change that close, but we could. Then we drove down into it, and it disappeared basically, I think.

SCHMITT My main criterion for singular and light mantle was the walls of the craters greater than 5 meters in diameter or so, which were distinctly lighter colored than any walls of craters out in the dark mantle.

QUERY It seems to me that the Sun angle and the angle between you and the site you're looking at and the Sun would be pretty important there.

- CERNAN No, at the Sun angle at which you drive and observe, you could think you're looking at half a dozen different things.
- SCHMITT There's no question that you have to temper almost all albedo observations with looking directly down-Sun. As soon as you get any significant cross-Sun component, you start getting topographic, photometric effects that'll do you in. Unless the contrast is exceedingly high, you can't be sure you're seeing albedo change.
- MUEHLBERGER One of our potential targets for 500s, which there was no time for, was Pigeon Pete. You remember every looking at it, either of you, and seeing whether you could identify it? It was way up-Sun in the Sculptured Hills.
- SCHMITT I did and I should have said something about it. As we came over the Scarp leaving station 2, between 2 and 3, I looked in that direction and never saw it. I looked for it.
- MUEHLBERGER We think we found it on your surface photos, in your pans, but it's dark, if we've located it right. That's why I want to get your impressions.
- SCHMITT No, I never noticed it. I tried to see some of those over there, but we had bad lighting for that point.
- MUEHLBERGER You did. That's true.

SCHMITT We were looking into the Sun and south slope.

QUERY I'm very interested in this question of the albedos. Is this a true statement that wherever you are there and you picked it up, the underlying dust is darker?

SCHMITT That's true, out in the dark mantle.

MUEHLBERGER It's not true on 16.

SCHMITT We crossed our tracks, and I didn't notice that on the light mantle.

CERNAN It's very interesting. Let me back off. We observed the Apollo 15 landing site as well as our own from orbit, and you can't see the LM or anything else, but you can tell exactly where the LM landed because there is a whole area of disturbed soil that gives you a lighter reflection. We could pick out the 15 site as well as we could our own in that valley, but when you are there, the disturbance is darker.

QUERY You can see it in the ascent photographs, too, can't you?

SCHMITT Yes, and it's dark. So I think from orbit somehow or other we must be getting an anomaly from a different scale feature than the detailed disturbance, because the detailed disturbance is distinctly darker. There's no question that it's

SCHMITT
(CONT'D)

darker when you are looking at a footprint. But when you look at the area from orbit, it's distinctly lighter.

QUERY

Is it possible that the footprints just put some shadows in.

SCHMITT

No, sir. It's just the dust. As soon as you turn over, the spray is also darker. I think it's clearly a darker albedo. This is known from Apollo 11 that this is the normal thing for these sites.

QUERY

Yes, but it is a fact that it looks bright from orbit and looks dark when you're there?

SCHMITT

Of course, it's 60 miles versus a meter.

CERNAN

But that's not unusual because so many things that you can see from orbit, like the white mantle, things you could see from a stand-off distance in terms of the light mantle and other things, when you approach them, you just lose sight. You just cannot see the difference in albedo. I'm sure if you approached them under different Sun angle conditions, that would even change it for you also. There's no question that we were at Shorty, and you could tell you were coming into a darker area. But Shorty is so well defined from orbit, black versus a light mantle that it's sitting right in the middle of. But my impression is that if I had to go

CERNAN
(CONT'D)

back and do it again, probably it would be very difficult to put a contact line around that black-white contrast on the surface when you are 6 feet from it.

SCHMITT

You might think about what would happen to albedo of a surface if you swept it in a planar, omnidirectional way with a descent engine. Maybe the filleting you get will cause, for some reason, a lightening of the surface rather than in the normal way where it's just a random kicking.

QUERY

That would be radial though, and the Sun's coming in from one side.

SCHMITT

No, we're talking about when we were viewing these things. I think we were at a sufficiently high Sun angle. Maybe you've got to consider that. Certainly, at our site, we were just about at zero phase when we were seeing this.

EVANS

But I saw it.

SCHMITT

You saw it earlier.

EVANS

It's very obvious; you have a light area surrounding it.

SCHMITT

That's even better evidence of why it's disturbed. It's disturbed from the descent engine. It could be compaction or directional filleting, radial filleting, giving you some kind of optical surface that's different from what you would normally get.

QUERY Could it be your optics of your field of viewing, your windows? When you are on the surface, you're viewing through a rather sharp angle.

MUEHLBERGER The surface panoramas show it an any Sun angle. You can see where they walked.

QUERY You're talking also about photography on the ground, is that right?

MUEHLBERGER Yes. Yes.

SCHMITT Yes, the visual and the photographic evidence were consistent.

SOIL MECHANICS (S-200)

CARRIER There are plenty of photographs with boulder tracks and bootprints and so on, which we'll get into. I guess one of the critical points on the mission was dropping the neutron flux probe into the hole. We were very pleased that the prediction that it would stay open was true and that later when you pulled it out, there was no squeezing of the hole. Is that correct?

SCHMITT That's right, not as far as I know.

CARRIER I thought I would talk a little bit about the cores. This is from the drill stem. You can obviously see right away

CARRIER
(CONT'D)

that the absolute bulk densities from the 17 drill stem are, in general, higher than either the 15 stem, except right down here for this section, and higher than the absolute bulk densities from the 16 drill stem. The plug that you inserted into the top of the stem did not function. I think you sensed that it wouldn't on the surface because it fell in so easily. We found it at the top of the drill stem.

SCHMITT

Was the core intact on it?

CARRIER

There was some disturbance. Fortunately, you put the rammer jammer down so we could take your estimate as well as review the kinescopes and get an estimate on how deep the sample was on the surface and compare that with how we get it back in the Lunar Receiving Lab. We figure from your rammer jammer that it was down about 30 centimeters. In the LRL, it's down about 15 centimeters. We assume that all the disturbance occurred in the top three stems during the return to Earth and that the thing moved roughly 15 centimeters.

QUERY

Didn't you have a break in the core? Wasn't there a space?

CARRIER

Yes. Just looking at the X-radiographs, we can see 8 plus or minus 4 centimeters of void. So we almost see as much void as would be predicted if the thing moved in sections, but not quite.

QUERY Where did the break occur? Do you remember if the base of the break was below the surface?

CARRIER There were two breaks: one was the top 6 centimeters on the third stem, and there were 2 centimeters at the top of the second stem missing or void. But based on that kind of information, this is the average bulk density that is quite high for the top 3. This dotted line is my estimate of what it was originally. We end up with a gravelly layer that Don Burnett mentioned, which has a very high density.

QUERY What's the depth of the first break in your curve?

CARRIER This is 10 centimeters.

SCHMITT At the LM, the main change in the properties as the function of pushing things into the ground was probably deeper than that, whatever two-thirds of a core tube might be, something on that order.

CARRIER Yes, you mentioned that to me. That's one possibility, that this denser coarse layer corresponds to the resistance that you got pushing in that core at the LM. This is a little bit difficult, to do some of these things. Based on the drilling rates, the relative density (that is, how tightly the soil is packed together) suggests that the

CARRIER
(CONT'D)

15 site was the same or maybe slightly denser, relatively speaking, than your site. In other words, your drill rates were a little faster or more or less the same as the 15 site.

CERNAN

Does that take into account the different drill design?

CARRIER

It is the same drill core. The core didn't change at all.

CERNAN

The core didn't change, huh? This is a little bit nebulous because ... in techniques and learning processes and a lot of things.

CARRIER

Yes, I realize that. Actually, the drill rates are based on the bore stem on a prediction based on new equipment at the 15 site. That's why it's a little "iffy." Don't press me for details, but the suggestion is that if the relative density of the two is similar, then the density of the particles is considerably different, just based on densities. This leads me into the other viewgraph.

This is data summarized for double core tubes. Now we've only had four cores so far from this mission out of the bags. We've not X-rayed them, so we don't know the lengths, but we have weighed them. This is where the uppers from 15 and 16 fall for bulk density, and this is where the lowers for 15 and 16 fall. This line is my guess for the core sample that went into the CSVC. Since it was a lower, I assume it was full length.

CARRIER
(CONT'D)

Actually, I have a question for you later on your procedures exactly with doing that. Anyhow, we don't know what the length of the upper would be so that this curve may be shifted up or down slightly, but the density in there is typical of what we expect. This was at station 3. Then at station 4, Jack mentioned the high densities for the double that was taken there. You can see that the upper half, assuming that it is 100 percent full, which is not normally the case for an upper.

CERNAN

But, you remember, I couldn't get that plunger to move at all. I think they are pretty full.

SCHMITT

That one went all the way down that double core above the ... soil.

CARRIER

The average density of 2 g/cm^3 and then the average for the lower half is 2.35. These are truly extraordinary densities. I plotted them on this kind of a curve to show you that comparison. If we assume the porosity of this core is typical of the porosities that we found in these cores, which is about 42 percent (and there's no reason to believe that it would be much less than that or you could not have driven it in the first place), you end up with a specific gravity, the density of the individual grains, of slightly more than 4. That's extraordinary.

SCHMITT I think your porosity could be lower. Looking at the little beads of glass and things in the microscope, they might have packed pretty well. It's sorted; it's sized, practically. Correct me when I go wrong here, but my impression was that it was a much better sorted soil than a normal regolith, and it may well be packed better.

CARRIER Actually, the less sorted it is, the better it packs.

SCHMITT You are right, the angular fragments.

CARRIER I think we have a very different material in this double core tube. I suspect that the density is not the orange soil but the black soil that you found underneath, and we're very anxious to get into that.

One other comment on the orange soil itself. Just a qualitative impression, but besides all the other interesting things about it, it appears to be one of the most cohesive soils that have been returned.

QUERY Cohesive?

CARRIER Cohesive, sticking together.

SCHMITT It was that way in the field. It broke into fragments actually.

CARRIER Would you verify the exact procedure you used with the CSVC, then?

CERNAN I used the exact procedure as we planned to use, as we were trained.

CARRIER I understand. Go ahead and repeat that, would you, just to be sure.

CERNAN (Laughter) No, I won't; I won't repeat it. I did it as planned. Do you have a specific question as to what you doubt on it or something? Where's the doubt?

CARRIER I received what you did second and third hand, so I'd like to verify exactly what it was.

CERNAN I just did it as we had trained and planned. Bob Parker will tell you what I did.

CARRIER The thing I'm after is that you pushed the keeper out of the adapter, right?

CERNAN It was a full core. I let the keeper lay on top the core and threw the top away and then put it in and sealed it. The keeper was on there, but, as I recall, the core was so full that I couldn't compress the keeper. The keeper was not wedged within the circumference of the tube. It was just

CERNAN
(CONT'D)

laying on top when it went into the can. Then I dumped a little out and put our patch in there just so you'd know who collected it.

SCHMITT

Which fine-grained material? All of it?

SPEAKER

In general.

SCHMITT

In general, my impression was that the dark mantle materials were sorted, that they were biased into the fine-grained fraction, at least the surface stuff, that you did not see much granule and greater sized materials until you got up to the fragment population which were scattered around, up to big blocks. That's just an impression; I don't know how that's going to turn out. I expected to see more of what would visually appear to be a seriate fragment population; that is, an exponential increase of fragments as you get smaller, but it seemed to me more biased toward the smaller size range. However, in the light mantle, although it was very fine grained, it seemed to be more of a seriate population of fragments. You saw little grains, little fragments in that, whereas I almost never saw them except on tops of rocks like the one we took there at station 5. You could see little grains, little fragments of rock in the sample we took. I suspect those did come off the granulated rock rather than the dark mantle.

QUERY You didn't observe any mission sizes?

SCHMITT That's what I'm saying. In a seriate population, what was missing was the granule to 1-centimeter size, let's say, because I just didn't notice that.

GAMMA RAY CRYSTAL EXPERIMENT (S-160)

CHAIRMAN Let's get on to the Gamma Ray Crystal Experiment. Dr. Jack Trombka.

TROMBKA This experiment has come out extremely successful. Much of the activity that we had measured is much higher than we had expected. On 15 and 16, we were performing geological surveys from orbit, looking at the gamma ray spectra emission, then during transearth, we were looking for a component which may be of cosmological interest in gamma ray flux in the region from about 300 kilovolts to about 30 MeV. One of the problems involved in an interpretation of this has been the problem of the activation of the crystal due to proton and secondary neutrons produced in the mass around the crystal, and, secondly, the protons in the cosmic ray flux. This then was the experiment, to determine the magnitude and extent of this activation. The experiment is very passive until you get back to the Earth. That is, the crystal was placed in the command module, and then it was returned

TROMBKA
(CONT'D)

extremely swiftly to the aircraft carrier. I think we've made about the earliest measure that's been made on a body that's been returned to Earth after some extensive exposure to cosmic ray flux. Measurements were made from 1-1/2 hours after reentry and have been continuing. The activation has been a very long-lived activation. That was unexpected. The electron bremsstrahlung which we thought would dominate the spectrum did not. There are very discrete lines, and the identification of the nuclear species produced is quite easy. Let me just show you the reason for our major interest from the results of Apollo 15 and 16.

In the first slide, the bottom curve is an extrapolation of what we expect the galactic component (this is on a log-log plot) of the gamma ray flux should be. The actual measurement is indicated under the Apollo 15 corrected curve. If you notice that in the region above 3 MeV or even above 1 MeV, there's a 3.2 order of magnitude higher flux than what is expected from the galaxy indicated there. The question had come: "Is this all due to induced activity in the crystal." I think now we can say with quite a bit of certainty that it is not, that it is indeed a meaningful flux above the galactic flux. From measurements made at about 30 MeV, we find that it's extremely isotropic. That's the very stimulating, idea that this might be the first

TROMBKA
(CONT'D)

major signal of cosmological importance that has been detected. This just indicates the type of thing. The measurements do indicate that our theoretical calculations seem to be correct, and that the magnitude is of that which we feel. I don't have slides of the results as of yet. We have identified components with half-lives anywhere from 18 minutes up to 60 days, right now. We've detected iodine-123, iodine-124, iodine-125, iodine-126, sodium-24, which we hope will give us a hang on the thermal neutron flux in the spacecraft. We also think we're seeing antimony-124, which would give us a hang on the fast neutron component produced by the secondaries from the cosmic ray flux. These are all significant measurements. Our measurements on the aircraft carrier produced intensities which were at least an order of magnitude higher than dactron [?] and about five times higher than we thought. But they are all concentrated in discrete lines; that's the fascinating thing. The continuum which would have been the problem in interfering with this is extremely low. The lines are very low energy, and, in fact, some of the lines are those that have been published in the literature having to do with galactic center, discrete lines which get us a little worried about the identification of these particular lines. If we see them here, I don't think they are coming from

TROMBKA
(CONT'D)

the galactic center. So this very briefly summarizes it; I have quite a bit of detailed data for anyone who would like to see it. There's about 2 weeks of measurements completed now, and we're continuing for a month. The only question I had was, where was the crystal with respect to the CSM? Do you remember?

SCHMITT

Where was it, in the waste compartment?

EVANS

It was in R-5.

TROMBKA

Okay, fine. Thank you very much.

SPEAKER

Right-hand LEB.

CERNAN

In the wall on the right-hand side down in the LEB. Just above the waste stowage.

INFLIGHT EXPERIMENTS

SM ORBITAL PHOTOGRAPHY

SPEAKER One of the big projects we had on this particular flight like the rest of them was the photography. We've got Fred Doyle to talk about the panoramic and mapping cameras and the laser altimeter.

DOYLE The principal things that you bring back from the Moon are, of course first of all, your good selves and after that, the surface samples and then the photographic film. As Jim has said, this is the third mission that we've flown the SIM bay cameras. I think I can run over them very rapidly.

The first camera is, of course, the panoramic camera. It covers a swath 300 kilometers wide underneath the spacecraft and resolves 1 to 2 meters on the lunar surface. Just for comparison, that's equivalent to photographing a Volkswagen, with the camera here at MSC and the Volkswagen up at the Intercontinental Airport. So it really is a very wonderful instrument for collecting information.

The other camera system is what we refer to as the mapping camera. The big black lens in the middle is the terrain

DOYLE
(CONT'D)

camera that photographs the lunar surface. The left side up there is the shield that covers the stellar camera photographing the star field, and the two gold-colored lenses in the lower left-hand corner are the transmission and the receiving optics for the laser altimeter. The terrain camera resolves about 20 meters on the lunar surface. The stellar camera has a fixed exposure time of 1-1/2 seconds, and it records about 25 stars in each frame. The laser altimeter measures the distance from the spacecraft to the surface with a range resolution of 1 meter.

In terms of coverage on this mission, we got almost exactly what we had planned to get. I'll talk about the pan camera first. We started the camera in the first rev in the elliptical orbit and took two short sections of coverage in that. Next block of coverage is on rev 13 and 14, and that was exactly as per Flight Plan. On rev 15, which was the next operation of the camera extending out over to the western terminator, we noticed that the V/H signal was erratic, and we decided that all subsequent operations would be performed with the V/H setting in the nominal position. And that was handled just perfectly throughout the rest of the mission. We did put the camera in that MANUAL OVERRIDE position and operated it that way from then on. On revs 28 and 49, the operation was exactly nominal. However, we did note that

DOYLE
(CONT'D)

the film usage was a little bit high, probably caused by the fact that the orbit was not circularizing as rapidly as we had anticipated. And, consequently, we shortened the first part of rev 62 to save a few frames. You note, right about in the middle of the diagram, a dotted section. We have to operate the camera at least once every 24 hours to keep the film from sticking. And so on rev 36, we put in this little block of oblique photography, which will be very interesting to look at in the pan camera.

QUERY

What's it of?

DOYLE

It's looking out over south of Mare Crisium.

SPEAKER

It looks like it might have been Luna 16.

DOYLE

Luna 16, yes.

QUERY

Is that the plan?

DOYLE

No, there was no plan. We just had to operate the camera and that was a convenient time to fit it in the time line. On rev 74, the first part of it was nominal. Then the second part, which is the westernmost coverage that we got and the last operation of the pan camera that we got in lunar orbit, the stereo drive motor failed 8 minutes into that pass and so the last half of that, the end of 74, will

DOYLE
(CONT'D)

be in mono rather than in stereo. We had originally planned for about 30 frames of post-TEI photography. Due to the excessive use of the film, we cut that back and we actually did obtain 16 frames of post-TEI photography. The film has been developed, but I have not yet had an opportunity to review it. There have been no copies made so far; the photo lab people tell me that the exposure quality is excellent all the way through. They did not look at the few items that I would have wanted to see. That's the oblique photography on 36 and particularly what the end of 74 looks like. As far as we could tell from the telemetry, the lens was a little bit aft of vertical when the stereo drive failed and that will probably mean that the aiming of the post-TEI photography will be off, and I'm not exactly not sure how that will look. The mapping camera coverage is in the upper diagram. Again, that was almost exactly as planned. Revs 1 and 2 were according to Flight Plan in the elliptical orbit.

On the next operation on revs 13 and 14, the extension of the camera took 3 minutes and 19 seconds as opposed to the nominal 72 seconds that it should have taken. So we decided that we would not move the camera in and out, and left it extended through the operation on rev 38. We had to retract it at that time for the plane change. That retraction time

DOYLE
(CONT'D)

took 3 minutes and 51 seconds, again far beyond the nominal. We decided then that we would operate it in the retracted position on rev 49. The consequence of that is that we lose stellar photography on rev 49 and also the lens covers are in the frame of the picture so that we lose about an inch of coverage along the forward edge of the pictures. You do, however, have 78 percent forward overlap so that we have not actually lost any terrain coverage. We did extend the camera again for rev 62. I have had the chance this morning to go through all of the mapping camera photography and do find an anomaly at the beginning of rev 62. Ron, maybe you can explain what actually happened there. The first half a dozen frames on rev 62 are a westward-looking oblique so that we actually see the lunar limb and see the the earth above the lunar horizon. That operation was supposed to start just east of Tsiolkovsky. By the time we got to Tsiolkovsky, we were in the vertical attitude. The rest of that pass was nominal. Somehow or other, we were out of the attitude that I expected at the beginning of rev 62. We must have been pitched forward quite severely.

EVANS

I don't remember by rev numbers, but I was not too careful with 5 extra hours of film or something like that as to when I turned the thing on. If it looked like I had time and

EVANS
(CONT'D)

my timer went ding-ding and it wasn't time yet, I turned it on anyhow. And that may have been a point in question.

DOYLE

It's certainly not critical. We have nearly 100-percent redundant coverage down there anyhow, but I think these pictures are interesting. They are the only mapping camera pictures we have with the Earth disk in them, so they are interesting from that point of view.

EVANS

I was really trying to get the Earth in there, too.

DOYLE

You were? (Laughter)

EVANS

I'll have to check the Flight Plan. It's on rev 62?

DOYLE

Yes, the beginning of rev 62. We did take both north obliques and south obliques. On rev 36, the camera was started late. We noticed that and talked to you about it as soon as we had AOS and you had seen that you didn't get the barber pole and so you didn't turn the camera on. But when you tried it again, it did come on. That means we lost a little bit of that north oblique photography, again not a critical item since we had north obliques on the earlier rev also.

EVANS

Could you tell, was the temperature too low at that point in time?

DOYLE I could not tell. The conclusion that was reached here by the engineering people was that the temperature was low, and that's why you didn't get the barber pole. When you did try it again, it worked and we got pictures.

EVANS I just left it in STANDBY until the barber pole disappeared, and as soon as the barber pole disappeared, I turned it ON.

DOYLE That's what happened. We did have that one interesting maneuver where you started in the north oblique attitude and then rolled through the vertical and finished up the pass in the south oblique, which looks spectacular on the film. The pictures are really outstanding.

The only problem that we noticed, apart from that late turnon in the one case and the westward-looking obliques, was that the telemetry showed the shutter-open pulses missing whenever we were in the low light levels throughout the mission. We were a little bit concerned that this might be resulting from improper exposure of the film. So we had them cut into the roll and take out a section of rev 62, which was the least critical photography, and process that first to see if there were any exposure problems and indeed there were not. The rest of the film was processed nominally according to plan. During rev 74, the last

DOYLE
CONT'D)

operation of the camera was exactly according to Flight Plan, and that gave us the most western coverage that we have in that part of the Moon.

The aim point of post-TEI photography was apparently a little bit off because the center of the disk is not in the center of the frame. It gradually drifts out of the frame entirely. By the time it does that, the diameter of the disk is down to about 2 centimeters, so again that is no serious loss. We got a total of 3554 frames with the mapping camera, an absolutely spectacular accomplishment.

Now I want to say a few words about the altimeter. On the past missions, we have had all kinds of trouble with the altimeter, as you know. This time it operated just like a dream throughout the mission. We did lose 4 minutes of data on rev 24, because you hit the switch and as soon as you saw it, you turned it back on. We got so much altimetry on this mission that we can afford that 4-minute loss. On rev 62, we gave up 30 minutes of altimetry on the dark side so that the UV spectrometer people could conduct an exercise out of SIM bay attitude. Then because the altimeter was operating so beautifully, we left it on through the sleep period from 220 to 230 GET. We got 10 hours of continuous altimeter data, and that's going to make the altimeter boys extremely happy.

SPEAKER On rev 62, we hadn't got back into attitude after we got through doing the UV guys' stuff.

DOYLE That could very well be. We didn't get back into attitude as soon as we had thought. We had a total of 3769 laser observations, and Bill Wollenhaupt is going to talk a little bit about the significance of some of those later on.

Just to give you an indication of what the relationship of the 17 coverage is to the 15 and 16, the 17 coverage is shown on the dotted lines on this slide. It does overlap extensively with the 15 coverage. This chart was made up premission, and actually we do have a little bit more of new ground covered at the western end of the 17 coverage and a little bit more at the eastern end, because we were able to start photography on rev 21 rather than 13 and 14 as had been originally planned. In the pan camera coverage diagram, we had a gap in the middle and that's where it crossed over the coverage from the 16 mission, so that we made the maximum use of the film load that we had. I'd like to say a few words about what we're doing with this photography now that we have it back.

The pan camera produces a very peculiar geometry which is not immediately recognizable. I've shown here at the top

DOYLE
(CONT'D)

of this slide a picture of New York City made with the pan camera. You see how the parallel street pattern converges as you go out toward the horizon, so you have a continuously changing scale. In order to use that photography for interpretation and cartographic treatment, we want to change it back into a normal geometric pattern like is shown in the bottom picture, in which the distortions have been straightened out.

We worked with the military and got this piece of equipment that came from a different program entirely, and it was modified to accommodate the Apollo pan camera. It does this transformation from the pan geometry to an equivalent frame geometry. Essentially, the film is wrapped over the cylindrical part. There's scanning light that traverses the film just as the slit does in the pan camera, and the image is reflected onto the easel in the front. This little array of hand wheels is used to adjust the curvature of that easel to match the curvature of the lunar surface from the nominal altitude of the spacecraft. The big hand wheel in the front can be used to tip the whole easel to accommodate for the forward and aft look that gives us the stereo.

This next slide is what the output of that looks like. The upper print is from Apollo 15. There is a 108° sweep of the

DOYLE
(CONT'D)

lens during the photography. The picture is 115 millimeters long and 115 millimeters wide. When it comes out of the transforming printer, it is nearly 6 feet long and about 9 inches wide. We are only able to transform the inner 74° . This is one of the critical factors in the flight planning. We try to arrange the overlap of the pan photography so that the entire ground cover will be within that 74° sweep whenever possible.

This next slide is a print from the mapping camera from Apollo 15 over the landing site there. It's equivalent to what we have in the vertical photography for this mission. The exposures are absolutely perfect throughout the photography.

This slide is one of the obliques. The attitude is rolled off so that the lunar horizon does appear at the upper limits of the frame, and we get these very attractive pictures which are extremely useful not only from the PIO point of view but also from the geologic interpretation point of view.

Just a few words about what we are doing with the photography. The data package consists of the tracking data, the terrain photography, the stellar photography, and the altimetry. Fundamentally, the tracking data relate the spacecraft to a

DOYLE
(CONT'D)

coordinate system on the Earth, and the terrain photography gives us the relationship of the lunar surface to the spacecraft. Adding those two things together, we get the relationship of the lunar surface to the lunar coordinate system to the Earth coordinate system. That gives us refined information about the Moon's ephemeris with respect to the Earth's coordinate system.

The second thing is with the terrain photography itself, we are able to do an operation called triangulation, in which we are able to tie together the geometry of all the photographs in the mission — indeed in the three missions — into a single reference system. The internal consistency of that network of points that we establish is about 20 meters in position and elevation. Anything that is photographed by the mapping cameras, we can put together into a unified coordinate system with that kind of precision, about 20 meters in all three coordinates.

The next thing is the stellar camera, photographing the star field. That is rigidly tied to the terrain camera photographing the surface. So this gives us the relationship between the lunar coordinate system and the celestial coordinate system, and that gives us refined information about the rotation rates of the Moon, the orientation of its

DOYLE
(CONT'D)

axis with respect to the celestial coordinate system, and the physical librations of the Moon. It also gives us an independent determination of the attitude of each one of the terrain camera exposures, so that we can get higher precision in tying the pictures together to cover the lunar surface.

Finally, we have the altimeter which gives us a profile from the orbit track to the subtrack on the lunar surface. It also provides us a measured distance in each one of the stereo models, so that we tie the photography together very rigidly.

There are problems that we have encountered in this data reduction. The measurements of the photography have gone very well, and they do tie together with the precision of 15 to 20 meters, which is just about what we expected. However, we have run into problems with the tracking data. We find if we compare the positions of one orbital pass with an adjacent orbital pass, they vary from 600 meters to 3 kilometers, considerably wider variation than we had expected in those numbers. So we are making some revision to the data reduction plan to do the photogrammetry entirely independently of the tracking data and to get a completely coordinated system before we try to incorporate the tracking data into it to give us the final tiedown.

LUNAR SOUNDER (S-209)

CHAIRMAN We have one of the new experiments, Lunar Sounder. Dr. Phillips.

PHILLIPS First of all, I'd like to thank the crew, particularly Ron, for doing an outstanding job of operating our experiment. Thank you for staying up late one day to get our rev 55 pass and rev 56. We essentially flew the nominal mission, and the hardware appears to have operated nominally also. Most of our data are on film. Before I get to that, let me briefly review some of the telemetry that we received during the mission.

We monitor the average power reflected from the lunar surface, and we found a few surprises. The area of the Moon that is smoothest to the eye is roughest in the HF and VHF frequency range. The highlands on the Moon that are roughest to the eyes are smoothest in our frequency range. We had a converse relationship between what you see visually and what we saw, and the most consistent explanation that we have is that the mare regions appear rough because of subsurface structure. You can see how much rougher the signal gets over the Serenitatis region as opposed to the highlands surrounding Serenitatis. On the average power and on VHF,

PHILLIPS
(CONT'D)

we saw a high correlation between the shape of the time series that we see average power in the topography. You see the Crater Hevelius, where there is a drop in power on the crater rim, probably due to the fact that the spacecraft is out of the specular direction. We see a high amount of correlation between the ... topography and the average power return, particularly on VHF.

Our prime data are on film, and we have about 620 feet of it. We have so far developed 2 feet. This is simply to verify the development process. The signal film showed a very strong signal on all three frequencies. This was encouraging. The one thing we didn't know after the mission until we developed the film was whether the CRT, the cathode ray, actually wrote on the film. All the telemetry received showed that everything else was operating nominally. We looked at the film in Ann Arbor, and we did take a peak in the image plane. We saw in VHF a very sharp image. I hear of photographic quality; I haven't seen it myself. The particular area where this was taken was over the Copernicus ejecta blanket. There is a very sharp image, very little specular return on the VHF. In the HF which was taken a few minutes later (this was the first half of the EMI test) was out of Procellarum. We do see a lot of specular targets. We see the obvious specular target from nadir. We

PHILLIPS
(CONT'D)

see other targets that are down range. Some are obviously off vertical surface features; others may or may not be subsurface features. Today, we hope to have a go ahead and develop the rest of the signal film. I think in another week, perhaps we'll have the signal film at Michigan in the processor. We'll be ready to go to work, and we're very excited both from what we saw in the specular power, indicating that there apparently is a lot of subsurface structure in the mare. Just the 2 feet that we have developed looks very good. So we are looking forward to that. I hope by the time of the Lunar Science Conference, we'll have something more definite to say on the imagery.

QUERY

Is there any doubt that's what you're seeing?

PHILLIPS

We ... the specular power to measure what we call apparent ... constant, anywhere from 3 to 8 or 9 or 10, even 14. You should not interpret anything like 14 as a real dielectric constant.

QUERY

What scales are you talking about when you talk about subsurface roughness?

PHILLIPS

We are talking about the lowest HF frequency as seen down perhaps a little more than a kilometer. The HF-2 frequency, 15 megahertz, is seen down 600 or 700 meters. The VHF is

PHILLIPS
(CONT'D) seen down 200 or 300 meters. We see in this roughness, in this specular power, the lower the frequency, the rougher the signal appears, the more noiselike the signal appears. Again, we feel fairly confident that most of the structure in the mare is subsurface features. That's interpretation at this point. We will really know when we see the film.

QUERY What range do you think you have for your dielectric constant?

PHILLIPS It seems apparent, like the constants from 3 to 14.

QUERY Does that fit your data?

PHILLIPS I underline the word, "apparent." You know what I mean. We don't measure the real ... dielectric constant.

FAR UV SPECTROMETER (S-169)

CHAIRMAN Next, the Far UV Spectrometer. Dr. Fastie.

FASTIE The operation of the spectrometer was almost identically in accordance with the Flight Plan. There were just a few changes. We got every observation that we asked for, and all the data were good. The instrument performance was nominal, except for a higher background count rate that we had anticipated, which apparently is due to cosmic rays,

FASTIE
(CONT'D)

and that has had the effect of giving us a lower limit of detection by about a factor of 3. We lost two temperature monitors internally in the instrument late in the mission. This loss had no effect at all on the performance of the scientific data output.

The major purpose of the experiment was to look for the lunar atmosphere. On a quick-look basis, we have identified, so far, only one constituent of the lunar atmosphere and that one at only 1 percent of the anticipated level. That's atomic hydrogen, and our number is somewhere between 10 and 100 atoms/cm³. We expected 10 000/cm³. Dr. Hoffman showed a rather large H₂ signal, and the expectation of H was that we would see the protons in the solar wind charge exchanging at the surface and coming off as hydrogen atoms. If Dr. Hoffman's H₂ measurement holds up, that means that they are charge exchanging and combining into H₂ and coming off as H₂. If it's not H₂, then the hydrogen is held in the surface, and that would be a little bit surprising. It's not necessarily surprising that you don't get very much H.

I would like to guarantee you that the numbers I'm giving you are real numbers. The measurement that we're making is not one that requires a great deal of assumption. It's a hard number based on well-established constants of the

FASTIE
(CONT'D)

instrument. We have seen nothing else at all in the lunar atmosphere, and our sensitivity is pretty good for some of the things. We could see 100 atoms of carbon per cubic centimeter, and we don't. We could see 1000 atoms of atomic oxygen, and we could see about 1000 atoms of atomic nitrogen. When it comes to molecules, we have to do a little bit less hard numbering, but we still think that there is no more than 10^4 atoms of carbon dioxide in the lunar atmosphere. All these numbers are spectacularly low.

In our analysis, the Moon has stopped degassing. It may have some gas left in it, but it's holding onto it very tightly, and it's not letting it out. One of the things we looked for was to see if Aristarchus was degassing. Since you looked on Saturday afternoon, we decided that Aristarchus doesn't work on weekends because we saw absolutely nothing. I would like to ask a question about this later.

The other lunar measurement that we made was the lunar albedo. We got precisely the signal off the Moon that we expected on the basis of the lunar dust samples that we had measured in the laboratory from previous Apollo missions. By "precisely," I mean right in the right ballpark. We also saw lunar albedo variations in the far ultraviolet

FASTIE
(CONT'D)

that were very interesting to us, and we'd like very much to correlate them with some of the mineral observations and to measure the Apollo 17 samples. We made a number of observations of the solar system, of the Earth, of the galaxy, and of the extragalactic targets. Dr. Henry, who had the major responsibility for that part of the measurement, is here, and I'm sure he would like to comment on that and to ask you some questions.

The only question that I would like to ask is the following. You did go over Aristarchus when it was close to the terminator, and you went pretty far over it, and it was in earthshine. You could see it reasonably well? I know that you took some pictures, but could you visually see the crater?

EVANS

Yes, you could definitely see Aristarchus.

FASTIE

This is in earthshine, and it looked pretty bright in terms of the albedo at that point?

EVANS

Yes.

FASTIE

Did the surface structure indicate anything that might show a fissure?

CERNAN

No, you can't see that. It's too far.

FASTIE You did most of the work for Dick Henry, not for me, because when you were in lunar orbit, it was a simple operation. When you got out into transearth coast, he made you jump all over the place. I didn't have anything to do with that.

QUERY At one time, you thought you had an emergency of the photo-multiplier tube. You now say that that was really galactic information?

FASTIE We're depending on Dr. Trombka to tell us what the wavelength of that cosmic radiation is. It looks like it might be gamma rays. It was a big signal, and we have yet to quantitate how we could have seen that big a signal with the kind of levels that people are talking about. It looks very much as if it is cosmic, not solar. At least, if it's solar, it's solar particles that have become omnidirectional by the time they get to the Moon.

QUERY I understand that it did not measure any neon?

FASTIE We could not measure neon. The only gases that are likely to be there that we did not look at were neon, argon, and helium in terms of the atoms. That's why we're very much interested in Dr. Hoffman's numbers for those three gases. His number for argon-40, for example, is also spectacularly low in my opinion. I don't know how he feels about it.

EVANS Is there any possibility of these gases being swept away last summer during that solar activity?

FASTIE The model for the lunar atmosphere, Frank Johnson's model, says that the method of loss from this thin atmosphere is ionization first, and then the solar wind sweeps the ion away in the electromagnetic field of the solar wind. You couldn't possibly have this level of density if there weren't some mechanism of that nature, because just the argon coming out the potassium-40 decay would be enough to give you a very sensible atmosphere in terms of the level of measurements that Dr. Hoffman can make. There is a huge loss mechanism of that nature.

EVANS Could it be a momentary loss related to the activity of the summer?

FASTIE The time constant for recovery would be relatively short. One of the things that we also looked for was an atmosphere that you created when you landed, and it looks like there might be a little bit there. The hydrogen signal might have been a little bit bigger during that period. It may have decayed later, but we haven't gotten into the details enough to answer this. At this moment, we can barely give numbers; we can't give numbers as a function of time. Dick, did you have any comments?

HENRY No, not a specific thing. Would you like me to talk about the transearth coast?

FASTIE Yes.

HENRY If you have an astronomical comment to make, you should have an astronomer make it. On transearth coast, primarily, and also to a certain extent in lunar orbit, the astronauts did look away from the Moon and out at the stars, and we acquired a very large body of data. I've only had a chance to look at about 1 percent of it, and so anything that I say today could well be wrong. I have to temper my remarks to that statement. The astronauts had to open and shut the door, and they had to turn the instrument on and off. More important than that, they had to maneuver the spacecraft so that it was pointed at all of these targets. We looked at a large number of different types of targets, and I'll just run through quickly what I think is going to be the result on each of these. I could be wrong.

First of all, we did look at the absolute brightness of the brightest stars, and we seemed to get a figure that's near a factor of 2 of what people have observed in the past. The people in the past have claimed that their observations were more accurate than that, and we may have a comment to make on that subject. The stars seem to be a little fainter perhaps.

HENRY
(CONT'D)

Secondly, there's zodiacal light, interplanetary dust, and sunlight scattering off this dust. This is what Ron Evans will recognize as the mode 4 operation, and this is the one where we got the extra operation. We were so surprised at our result on the first one. The second one confirmed it. The Orbiting Astronomical Observatory had led us to believe that the interplanetary dust was producing a lot of UV radiation. What we did was essentially a solar eclipse. As the Moon just covered the Sun, that is to say, as the spacecraft just disappears into the dark, you look very close at the Sun and the sky. We looked and we saw nothing. Mr. Dubin [?] at Headquarters has produced an interpretation of this. He believes the OAO results and suggests that perhaps this observation that the OAO has made is of dust much nearer the Earth. This is something else that we can tackle with the data that we have from Apollo. We haven't been able to look at that yet, but we're looking forward to it.

The third point was the so-called Coma cluster of galaxies, a large group of galaxies at very high galactic latitudes, and there seems to be a very dynamic problem with them. It seems to be flying apart, and yet the galaxies themselves are apparently very old. So the question is whether there might be additional matter present in the cluster of galaxies

HENRY
(CONT'D)

gravitationally holding it together. We thought it might be in the form of ionized hydrogen. We looked for Lyman-alpha radiation, red shifted from the ionized hydrogen, and we didn't see any. We set a lower limit, which certainly excludes the possibility that the Coma cluster is held together by this ionized hydrogen. I think that may leave a real mystery as to what is holding the thing together.

The fourth point may turn out to be the most interesting thing of all. When you look in the Milky Way, you see a lot of UV coming from the stars, but the question is, what do you see when you look up to the North Galactic Pole or down to the South Galactic Pole. One of the most exciting results of X-ray astronomy was the fact that an X-ray background was observed over the sky that nobody had expected, and part of this is the gamma-ray background that Dr. Trombka talked about. In the UV, nobody knows, but you never know until you look. You do have to deal with this background of stars that we know is there. So we did look at a large number of different points at high galactic latitudes, both north and south. The spectrum that we see is above this dark count. In other words, this abnormally high dark current did not, in fact, interfere with that experiment. The spectrum that we see looks like the spectrum of the hot

HENRY
(CONT'D)

star; however, we know that there were no hot stars within our field of view. Therefore, the most conservative interpretation, I think, is that what we're seeing is light from hot stars in the galactic plane going up out of the plane and reflecting off interstellar dust. There are certain characteristics of the spectrum, though, that don't fit that theory, and it's at least possible that this is extragalactic radiation. I'm looking forward very much to the detailed computer study of this, but it's going to take a long time.

Fifth point: Lyman-alpha hydrogen radiation is a completely separate problem, and Gary Thomas at the University of Colorado and Charles Barthum [?] observed this from OGO-5. We obtained just an enormous amount of data on the Apollo that's going to straighten out this picture and clarify it considerably. This is hydrogen that is inside our solar system. It's sunlight reflecting off this. The hydrogen, Gary Thomas thinks, is hydrogen from interstellar space streaming through the solar system, and he is looking forward with great anticipation to getting detailed analyses of that.

One more thing: the spectrum of the Earth. I keep saying "we," but these were the guys that were there. We looked at the Earth from outside. A lot of people have observed

HENRY
(CONT'D)

it essentially from inside the flying rockets. The rocket goes up this high; the hydrogen is up here like this, and you observe it from the inside. You guys were observing it from the outside here, and the spectrum is expected to be different. We observe it to be different, and the analysis of that is going to be very important. Also, that coordinates beautifully with Apollo 16, where the astronauts used George Carruthers' camera to photograph the Earth. Thus, those are going to tie together very well.

Just one last point that Bill was going to mention but forgot is that we do get from the lunar albedo study a measurement of the solar spectrum, the Sun. There have been two previous observations, one by the Naval Research Lab and one by Harvard. They are different by about that much, and our measurement comes right there in the middle, so that's going to make everyone happy.

IR SCANNING RADIOMETER (S-171)

CHAIRMAN The IR Scanning Radiometer. Dr. Mendell.

MENDELL The infrared scanning radiometer is basically a line scanner of fairly standard design. The objective was to thermally map the lunar surface, and our primary scientific data were during the lunar nighttime. That's where the instrument

MENDELL
(CONT'D)

design was oriented, toward making this kind of measurement to get the maximum sensitivity from basically a room-temperature detector, which was tricky. We managed to get a pretty good instrument up there in orbit and to make measurements that improve the Earth-based information by an order of magnitude, both radiometrically and spacially in their resolution. The operation of the instrument was very good, we got the expected sensitivity, and the data looks fine.

The kind of information that we did receive during the mission was as follows. The data rates out of our instruments were too high to be piped straight into the Mission Control Center. What was arranged by Frank Brizzolara and some of the people here in-house was to take 3 seconds' worth of data at the range station and to buffer it, and then pipe it to us over 2 minutes. Basically, what we got was a scan or two about every 6° of longitude, and this let us see the quality of the data and any kind of anomalies that should appear, so that we could evaluate them during the mission. Otherwise, we would have been totally unable to do this. What slides I brought with me are directly off some of the charts that we got brought down to us in room 210 and were looking at during the mission. They

MENDELL
(CONT'D)

represent more or less random selections of data to give you an idea of what sort of things that we did see.

This first slide is just a schematic of the instrument that shows basically a Cassegrain optical configuration with a folding mirror. The 45° mirror rotates, and this causes the scan line. The orbital motion of the spacecraft spaces the scan line, so that we can build up an imagelike picture, much like a TV set or raster fashion.

This next slide is simply a photograph of the instrument, about 2 feet long and 8 inches square, in the other dimension. In the aperture in the housing, you see a scan mirror which rotates. What we see in a scan is, basically, as the mirror comes out of the housing, we'll see space, where zero is set by the instrument as it looks in space. Then we scan across the Moon and see space again on the other side, and then we go back into the housing. This sequence is repeated cyclically.

This slide is a lunar nighttime scan here. You see the three channels of information, which are identical data but at three different gains, to cover the dynamic range of temperatures on the Moon. The bottom channel is channel 1, our most sensitive high-gain channel. What you see here is a nighttime scan, somewhere near Kepler, in that region.

MENDELL
(CONT'D)

I haven't identified the thermal anomalies that you see on the general soil background, but this is the kind of thing that we do see. There's one point that I want to bring up. If you'll notice, on the right-hand side, we have the space hook or the space clamp. We climb onto the Moon, see the structure, come across the Moon, and come back. The scan should drop back to zero as it is on the right-hand side, but it doesn't. It's about 10 PCM counts above zero, and this is an anomaly that occurred in the operation of the instrument. My guess as to what this is evolves daily. It's either electronic or something in our field of view, and I think that the predominance of the evidence is that there is something in the field of view on the trailing end of the scan. We did quite a bit of looking at sky, which should be totally flat. When we look at sky, we see, about in the middle of the scan, a small ramp starts to build up to where it builds up to about 10 PCM counts to the left-hand side. Much of my activity in the past week or so looking at this real-time data has been studying this ramp function. It seems to be a very nice linear feature, which can be taken out basically by getting the amplitude up from the trailing end of the scan and extrapolating back. It should have no effect on the data other than to increase our noise slightly on the trailing edges

MENDELL
(CONT'D)

of the scans. You notice that even on the cold Moon, our sensitivity is quite good. You see a good healthy LM deflection and this is all very encouraging. The instrument was performing quite well.

This slide is a presunrise nighttime scan. You'll note a couple of things about it; one, it's very featureless. The other thing about it is the low deflection over here on the right side, where our presunrise temperatures got down to about as low as 85° Kelvin in our preliminary look. These numbers will be juggled as we look at our calibration curve in more detail. Toward the left-hand side of the scan, you'll see a little dip which can be classified as a cold spot. We're very interested in this kind of thing. These areas must be areas where the physical properties of the surface are different from the general soil background where the soil is more porous near the surface. Therefore, it cannot be an impact feature because that tends to densify the surface. It is not like a general soil background, which leads me to believe that it is very likely a younger feature than the ordinary lunar soil. We have done a very tentative identification on one such feature of this type near the northern part of the central basin, Mare Orientale. A look at the orbital photography of this feature tends to

MENDELL
(CONT'D)

show that there's a lower crater density, and it seems to be somewhat geologically younger than the surrounding material.

The next slide was taken 2 minutes earlier in the time line. The slide was not made exactly right, but you see that suddenly we are at the morning terminator and the signal gets very jagged and irregular as we see sunlight glinting off peaks. We see very cold areas next to very hot areas, which are indications of slope distributions and warming up as the Sun hits the area. It happens very rapidly.

This next slide is a daytime scan, and I show it for interest because you notice how very flat it is. This happens to be a scan of the western edge of Mare Crisium. Notice the dip on one side and the bump on the other, which is the sunward-facing slope on the south and north ends, respectively. You get a shadow at one scarp and direct sunlight off the other scarp. The next scan is the next slide.

These are typical highlands. In 2 minutes, you've changed from a very flat feature to this, where the slopes and albedo changes dominate and we get a lot of thermal structure.

MENDELL
(CONT'D)

This slide is one of the more spectacular features we saw in the early revs. You see a trapezoidal-type feature with a spike in the middle. I've tentatively identified this as Kepler A. At this particular time, we are right down on the deck at 21 nautical miles. I interpret that as an ejecta blanket around the central crater. Otherwise, note once again, the mare areas are very featureless except for these isolated thermal anomalies.

Although these are daylight data here in the center frame of this last slide that are not of great scientific interest, you do see that when we line up all the scans, we do get something that looks like an image. In particular, the slopes facing the Sun are hotter and those away from the Sun are cooler, so you get the same kind of effect as you would from just visual light reflection. This gives us some confidence that when we put everything together we will see a nice continuous picture.

The other anomaly in the operation of the instrument was saturation at the subsolar point, which surprised us. I think I'm just gradually beginning to understand how that occurred. It may be that for the very hot Moon, we had skirts on our field of view that we did not anticipate having and didn't really test well for. We're going to

MENDELL
(CONT'D)

be undergoing a test program at Rice in the next few weeks which will better define a lot of these instrument parameters.

S-BAND TRANSPONDER (S-164)

CHAIRMAN

The last experiment on the agenda is the S-Band Transponder and Bill Wollenhaupt.

WOLLENHAUPT

The S-band transponder experiment derives gravity information from the Earth-based Doppler frequency measurements obtained by tracking either the orbiting command module or the lunar module. We've been involved in this type of experiment throughout the entire Apollo project, following the mascon discovery by Paul Muller and Bill Sjogren of JPL. Since the real estate that we covered on this mission was very close to that of Apollo 15, I will frequently make comparisons between the two sets of data that we have obtained from the gravity experiment and the laser altimeter.

Essentially, on the front side, we were south of the 15 ground track; on the far side, we were north. This particular plot shows the preliminary gravity results where we're showing an acceleration profile in milligals for the region, plus or minus 50° longitude. The major features that we see here are Serenitatis, Aestuum, and the Littrow landing site.

WOLLENHAUPT This is, by comparison, a slide obtained from Apollo 15,
(CONT'D) where you again see the sharp positive acceleration at Serenitatis, a negative there in the Littrow region, and of course Crisium shows up as a strong positive. Now go back to the previous slide, please.

More or less to summarize what our preliminary results indicate now, we see a slight gravity low or mass deficiency at Copernicus. So far, all the small craters that we have overflowed appear to show a negative or gravity low. Sinus Aestuum is a distinct mass excess or mass concentration, and we're getting a refined estimate on this. The previous data did not give us good coverage. Serenitatis, as I previously mentioned, is a distinct gravity high. Littrow is a negative. Incidentally, on the landing site, the values that we obtained from this experiment agree very well with the traverse gravimeter. Dr. Talwani mentioned 162.69 cm/sec^2 . From these data, we're seeing something like 162.75 to 162.8 . We still have some further adjustments because where the traverse gravimeter is on the surface, we're overflying at something like 12 nautical miles for this particular orbit. The Procellarum region appears to be an uninteresting gravity region. We don't find too many distinctive features in this region.

WOLLENHAUPT I'm going to describe something that's a little more visual;
(CONT'D)

I'd like to go into the laser altimeter results. These plots show the deviations in kilometers from an assumed spherical Moon having a radius of 1738 kilometers. From this, you can get an idea of the deviation from such a sphere and also look at the altitude profile. Note how we get what appear to be very sharp spikes throughout this, such as Neper on the right-hand side. You can see the ringed basins or the ringed maria come in, like in Serenitatis, Crisium, and we go over the Apennine Mountains into Imbrium, then over the Carpathian Mountains out into Procellarum. Again, we see another sharp spike there which correlates with the crater Reiner. Off to your left is where we're transiting over the outer rings of Orientale. For comparison, I can show you the near-side altimeter profile from Apollo 15 on the next slide.

Here we're seeing essentially the same type of thing, across the Apennines just about in the middle of the plot with Serenitatis, Crisium off to the right, and the other flat region to the far right is Smythii. The little peak at about minus 60° is the Marius Hill region.

WOLLENHAUPT
(CONT'D)

This next slide is the same type of profile but for the lunar far side. I have shown some overlap there. There's a data gap where that sharp ramp is. These represent data that we have not recovered as yet. The data are available; they will show up in the station tapes, but we did not get them during the mission playbacks. Perhaps the most significant thing here is that we again see a very large depressed region of something like a 50° diameter, and again it appears to be centered roughly at the 180° longitude point. That is in a very gross sense, and it shows up as very rough or perhaps mountainous but it does appear to be depressed. Our depth is something on the order of 2 kilometers. Now here's the same type thing on Apollo 15 in the next slide.

Here we were about 5° to 6° south of the Apollo 17 ground track. Again, we get a very distinct impression of this depressed region, only now the depth from the mean sphere is something on the order of 4 to 5 kilometers. From the two types of slides on the near side and the far side, you can see how rough or mountainous the far side appears by comparison to the near side. The near side profiles have all indicated that the ringed maria are relatively flat regions and depressed with respect to the surrounding terrain.

WOLLENHAUPT (CONT'D) The next slide shows a view from Apollo 17 at a later revolution. We're now out toward the end of the data-taking period in revolution 71. Here again, there'll be a lot of gap periods. The section over to the far right does not represent data; that simply represents where the data are missing. This is a computer plot, and it will continue to draw lines. At around 40° east, you can see Tranquillitatis, then we drop off into Serenitatis, the sharp peak is in the Apennines, and it looks like we were almost right on top of the ridge. Then we drop off out into Imbrium and transit out through Procellarum.

This slide shows the far side again with many data gaps that we will fill in. I think I got that more for Jack on Tsiolkovsky. Do you see it, Jack? It's right at around 120° , and it appears that it is something on the order of 4 kilometers deep. That was one of the good passes we got over Tsiolkovsky.

QUERY How deep were the bad passes?

WOLLENHAUPT I haven't had a chance to find out, as far as the center being relatively well centered in this sense. As Fred Doyle mentioned previously, we have a wealth of data this time. In fact, we have more data on this mission than we

WOLLENHAUPT did in the other two missions combined. Of particular
(CONT'D) interest from the gravity standpoint are the five consecutive revolutions that were obtained toward the end of the mission. These should yield a much more refined altitude profile, and hopefully we can extract some gravity information from these data.

QUERY Did you get Gagarin at all from any of these runs?

WOLLENHAUPT No, not on this particular one but I think we've transited over part of it. I haven't seen too much of that yet.

SPEAKER It looks like at about 95° or 100° , there's a major shift in what you might call the trend of the mean level of mare. It would be interesting to see what other craters on the back side you got that have mare in them and see how their depths plot across there.

WOLLENHAUPT There are not too many.

QUERY Tsiolkovsky is a good mare-filled crater. As I recall from the other diagram, the mean depth of Smythii was about 4. Is that Smythii there?

WOLLENHAUPT No, we're probably up north. There might be one point toward one edge of it.

- SPEAKER At any rate, right in that region, it seems to jump up again at much less depth, a lower depth with respect to the mean radius.
- WOLLENHAUPT If you want to take another moment here, we can compare it with 16. You'll see this same type of contrast. This is the near-side pass obtained on the Apollo 16 mission.
- QUERY You're down to 4 or a little greater than 4 there at Smythii?
- WOLLENHAUPT A little greater than 4 at Smythii, yes.
- SPEAKER It's not so clear here, but on your previous slides, it looked as if there were a very general trend or slope of the mare surface from basin to basin, from about Smythii at 4 or 5 to Procellarum.
- WOLLENHAUPT Yes, the 17 near side showed that very clearly. There does appear to be an upward trend moving from east to west in the depth of the maria.
- SPEAKER Between Smythii and Tsiolkovsky, that jumps; there seems to be a break in that trend. That was the only thing I was pointing out.
- WOLLENHAUPT On the next slide we've got a ringed maria on the far side. What is that? Mendeleev.

QUERY That's not real mare; that's more light planed. That's not good, dark mare, is it?

WOLLENHAUPT No, it's not a good, dark mare. You notice how this one differs. We see no evidence of the depression, but here we are going at about 9° or 10° north latitude over the depressed region. I think you can see parts of the profiling from Hertzsprung in there.

QUERY I do have a question for Ron Evans. On that impact, ... contact, do you have any more details on that?

EVANS It just looked like a little pock mark right in the center of window number 3.

CERNAN As I recall, that was the only one I saw in any of the windows.

EVANS This pockmark may be a millimeter in diameter, something like that.

QUERY Any idea what the time was, GET?

CERNAN It'd be on the tapes. It was translunar, wasn't it?

EVANS I think it was translunar, yes.

STATEMENT BY APOLLO 17 CREW

CERNAN Before you all run, I'd just like to thank you. You've given us a lot of accolades for coming back with the data, but that's what we were sent there to do. We had a lot of help and a lot of good hard work doing it. I think some of the relationships and some of the meetings that were started very early in the game and some of the realizations of our problems and some of the understanding of yours are what really put this all together. We're obviously very proud we came back with so much data. That's made many of you apparently as happy as you seem to be. It was a job, but nevertheless, we're glad we were given the opportunity to do a good one. We hope we did, and we thank you for the part you played in it.

CHAIRMAN Thank you, Gene.