MODIS Team Meeting Minutes

Minutes of the MODIS Team Meeting held on Tuesday October 10, 1995.

Action Items:

113. Determine the best method to display a fixed pattern noise (herringbone, Spec 3.4.5.3.3). Assigned to Knight 8/15/95. Due 10/15/95.

114. Determine the extent of ghosting from the SMIR and LWIR polished cold shields. Assigned to Waluschka 8/29/95. Due 9/22/95.

Distribution:

√	Richard Weber	√	Bruce Guenther		Larissa Graziani
√	John Bauernschub		George Daelemans		Bob Martineau
√	Rosemary Vail		Mitch Davis		Bob Silva
	Lisa Shears	√	Ken Anderson		Robert Kiwak
√	Mike Roberto		Rick Sabatino		Harvey Safren
√	Gene Waluschka		Cherie Congedo	√	Ed Knight
√	Bill Barnes		Jose Florez	√	Harry Montgomery
√	Les Thompson	√	Gerry Godden	√	Marvin Maxwell
√	John Bolton	√	Sal Cicchelli		Bill Mocarsky

The following items were distributed:

1) Weekly Status Report #210
2) SBRC Memos submission from week #202
3) Minutes of the previous team meeting

MODIS Technical Weekly October 13, 1995

sent to MODIS.Review 10/17/95 at about 8:30 pm

1.0 Introduction

This report covers from October 9 through October 15.

On Monday, October 9, Tom Koch provided the PFM status at the systems integration and test coordination meeting at SBRC. View graphs from this presentation were also shown at the GSFC MODIS team meeting on October 10.

Excerpts are included from David Jones' weekly. Joe Banuch has moved on to other projects and Mary Ballard (Mary Dowler's new name) has taken over Joe's duties. David comments that for the PFM RC/OAO T/V test, the scene plate will now be 180K except for a short period when the PFM optics assembly are heated to +26 degrees C. At that time the scene plate temperature will be raised to + 20 degrees C. Telephone numbers are provided for Larissa Graziani and George Daelemans.

Eugene Waluschka discusses testing the MODIS filters for light leaks. Gene states that ideally the filters and detectors should be tested separately and independently for crosstalk. He mentions that modeling this effect in the filter and detector size domain is more difficult than modeling the behavior of the large optics because of the small dimensions at the focal plane. This makes testing important. If the hardware, time, facilities, and personnel required for independently testing the filters and detectors can not be obtained within the next few weeks, the alternative will be to wait for near field response testing. This testing will also include the effects of ghosting and scatter.

Ed Knight provides information in three separate areas:
1) Details are provided on a discrepancy in the MODIS EM packet data which involves a violation of the CCSDS protocol. This needs to be forwarded to SBRC so they can make the correction.

2) Ed describes proposed fixed pattern noise displays. SBRC plans two tests. One will scan the inside of the Earth aperture door. The second has the scan mirror fixed as it looks at the space view. With the mirror fixed, some pattern noise could be missed. Also, some individuals have questioned whether pattern noise might become visible at Ltypical that was not evident at the low radiance level planned for the test. Ed describes the MCST version of current TAC software that can be used to display 2 dimensional images of raw counts. Ed recommends four images that can be used to verify spec compliance.

3) Ed provides comments on the master curve approach applied to EM test data. He has also included comments from Gerry Godden and Larry Goldberg. Gerry believes it would be helpful to confirm the assumption at the detector level that the FPA response is not dependent on the temperature of the instrument. He lists some questions and concerns regarding this assumption. Larry Goldberg does not believe the EM test data was good enough to verify or justify the master curve approach.

Jose Florez and Mitch Davis reported that SBRC was able to get the Single Board Computer up and running. The problem of not being able to run at the proper speed was caused by the additional delay introduced by buffers added by a redesign from the EM.
Once corrected, the board was able to run at 15 MHz, even at low bus voltage.

Neil Therrien provided an email message on LWIR leaks and crosstalk. He reviewed scans taken during LWIR focus and alignment operations. He was unable to see the 10 percent leak observed on the EM between bands 33 and 27, nor the about one percent leak near band 31. The test signals were low for the PC bands, typically 50 to 100 counts. There may not have been enough signal to see the band 31 leak, but the absence of a leak between bands 27 and 33 is probably true. Neil describes a test that can be performed at the OBA level to confirm the absence of the band 31 leak. He also believes that the crosstalk anomaly noticed between PV LWIR bands is electrical since the 'receiver' band's error signal is dependent on the 'sender' band signal. When schedule allows, there may be some diagnostics SBRC can perform in this area.

2.0 David Jones (Excerpts from Weekly Report)

Author: David R Jones <davidjon@pop400.gsfc.nasa.gov> at Internet
Date: 10/12/95 8:05 PM
Subject: MODIS ONSITE RPT W/E 95/10/15

The following are excerpts from David Jones' report:

1.0 STAFF CHANGES: Mr. Joe Banach (FPA Project Manager) has moved on to other projects at the SBRC Detector Division, and Mary Ballard (Dowler) has taken over his responsibilities.

2.0 PFM RC/AOA T/V: The PFM Rad. Cooler and Aft Optics Assy. T/V started at 02:52 AM Saturday 8 Oct. Details of the tests planned (including a profile of the temperatures planned for critical elements) were emailed to the GSFC Project, on Monday 9 Oct.

It should be noted that the "Scene Plate" (which I described last week as an "Earth View Target") will now be maintained at -93 deg C (180K), throughout the T/V, except for a short period, when the PFM Optics Assembly are heated to +26 deg. C (at this time the Scene Plate temperature will be raised to +20 deg C).

3.0 GSFC Test Support: Larissa Graziani continues to support the T/V testing, and can be reached at SBRC on (805) 562-7251. George Daelemans arrived Monday (9 Oct) afternoon and can be reached at (805) 562-7287.

3.0 Eugene Waluschka (Testing MODIS Filters)

Author: Eugene Waluschka at 710
Date: 10/13/95 1:52 PM
Subject: testing modis filters

I have just spent the better part of a week making lots of telephone calls attempting to determining what tests can be performed on the MODIS LWIR focal plane filter assemblies to, experimentally, see if there are unwanted light paths which result in a broadened line spread function i.e. cross talk. Currently the cross talk becomes evident rather late in the game, that is, only when the filters are attached to the detectors and entire filter/detector assembly is being aligned (to the other focal planes) in the aft optics assembly. The alignment is accomplished by sliding slit images across the various focal planes. Synchronization of the various detector responses is achieved when the focal planes are aligned. These moving slit images also form the basis of the transient response
measurements. Again, any cross talk problems are detected only well into the aft optics integration stage of assembly.

Ideally the filters and detectors are tested separately and independently for cross talk. It is relatively simple to envision a experiment where only one detector element (or pixel) is illuminated before filters are attached. This would unambiguously show detector cross talk. A similar experiment on the focal plane filter assembly would illuminate a single spot on the entrance face and the light distribution would then be measured on the exit face. In the absence of the detector assembly immediately under the filters this would give a fairly good indication of any filter cross talk. At present the entire detector plane is illuminated prior to attachment of the filters. This test indicates which detectors are alive but gives very little information about detector cross talk. The focal filter assembly is also not tested independently for optical cross talk.

The current concerns about MODIS's transient response are, in large part, due to the engineering model measurements. Analysis of the results has produced a fairly good understanding of the fore and aft optics contribution to scattered light and ghosting portion of the transient response degradation. However our understanding of what happens when light enters the filter/detector assembly is not as well understood. We have models which predict, fairly well, the behavior of light in the (big) optics. We do not have models which work as well in the small, filter and detector size, domain. This in large part is due to the fact that keeping light away from edges is more difficult. That's why testing is so important.

Now, what can be done in the short term (a couple of weeks). A possible quick look approach to see if there is a problem with the flight model filters can (possibly) be made with the IR camera which is currently being used to examine the electronic circuit boards. There is a close-up lens available. A possible experiment setup could be viewing a "hot" source through the filters. Here the area between the filters should appear uniformly dark and only the filter area should be bright. It is not a perfect experiment but it could reveal light leaks. Unfortunately the camera works in the 3.2 - 5.6 micron wavelength range, but we could also look at the SWIR filter assembly. There is also another camera which works in the 8-12 micron range available at Goddard. But this is somewhat more difficult because it would require (in the near term) transporting it, software and, knowledgeable in its' use, person to use it.

That's it for one week of telephone calls. In the near term the camera examination is about the only thing we can do (if it were in the visible light we could do more. The results of a camera exam will be inconclusive. However is it better than nothing?

Of course we could do nothing and hope for the best. However, if we discover we have a cross talk problem then be forced to test with a substantially longer schedule hit. Another possibility is we perform the camera examination and we see no problem.

What choices and consequences are possible:

1 - Wait for near field response testing.

4.0 Ed Knight (Packet Format Discrepancy, Proposed Fixed Pattern Noise Display, Comments on Master Curve Memo)

Author: eknight@highwire.gsfc.nasa.gov (Ed Knight) at Internet
Date: 10/13/95 2:45 PM
Subject: Fwd: Discrepancy in SBRC Packet Formats

I believe that the following needs to be passed on to Joe Auchter and/or John Mehrten. Keith Degnan of SDST has discovered a discrepancy between actual EM data formats and the documented formats.

Ed Knight

Ed,

I've uncovered another data discrepancy in the MODIS EM packet data; the value which is stored in the packet length field (the last two bytes of the CCSDS primary header) is not consistent with the CCSDS definition for this value. The description of this field (from the CCSDS Blue Book on Packet Telemetry, CCSDS 102.0-B-3, November 1992, follows:

"3.1.4 PACKET DATA LENGTH FIELD

a. The PACKET DATA LENGTH FIELD shall be contained within bits 32-47 of the PACKET PRIMARY HEADER.

b. This 16-bit field shall contain a binary number equal to the number of octets in the PACKET DATA FIELD minus 1.

c. The value contained in the PACKET DATA LENGTH FIELD may be variable and shall be in the range of 1 to 65536 octets.

d. Further constraints on the length of the PACKET DATA FIELD are specified in Paragraph 5.1.5.4."

Specifically, 3.1.4 b. is not met by the MODIS instrument packets generated during the EM testing. For MODIS day mode packets, the total packet size is 642 octets (bytes). The first six octets are the CCSDS primary header; the remaining octets (636) are the packet data field. The value that should be stored in the packet data length field should be 636 - 1, or 635 (hexadecimal 027b). An investigation of the packet data shows that this field contains 636 (hexadecimal 027c).

SBRC CDRL 305 (Engineering Telemetry Description) indicates that SBRC was aware of this standard. In section 30.7, the document clearly states:

"The PKT LENGTH (Packet Length) field contains the binary count in octets of the data in the Data Zone (defined above) as the number of octets. For MODIS, this will normally be N1 = [(5136-48)/8]-1 = 635 octets or N2 = [(2208-48/8]-1 = 269 octets."

Since this discrepancy involves a violation of the CCSDS protocol, the resolution of this problem should be given high priority.

-Keith Degnan/GSC-SAIC
MODIS SDST Lead Instrument Analyst

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Author: eknight@highwire.gsfc.nasa.gov (Ed Knight) at Internet
Date: 10/15/95 1:29 PM
Subject: Pattern Noise Displays--Response to Action Item

Subject: Proposed Fixed Pattern Noise displays
Response to Action Item 113 from Weber MODIS Team meeting.

Background:

During the Engineering Model Data Review, questions arose about the best method to display imagery to look for fixed pattern noise. The specification states:

"There are other coherent noise mechanisms that lead to structured patterns in the output data (e.g. herringbone or diagonal bars). These extraneous noise sources shall be imperceptible in data taken at the Ltypical radiance levels for each spectral band. This shall be verified by two-dimensional pictorial display tests used on respresentative bands." (3.4.5.3.3)

Note that there is a related requirement in channel to channel uniformity:

"For all live channels, the calibrated mean output of each channel with respect to every other channel shall be matched to within the NEdL. [. . .] This matching condition shall be met when the instrument views a uniform constant spectral radiance field at levels of approximately 0.5Ltyp, Ltyp, and 2Ltyp (or Lmax if Lmax < 2Ltyp)." (3.4.5.3.2)

My Action item is to "determine the best method to display a fixed pattern noise (herringbone, Spec. 3.4.5.3.3))." This email will address that action item by discussing the test, the current software, and the proposed display test.

The Test

SBRC plans two tests to check for fixed pattern noise. One will be to scan the inside of the Earth Aperture Door. The second is to fix the scan mirror so that it points at the Space View and collect 1354 frames for 10 scans. The latter was done on the Engineering Model with Charge Subtraction on and is described in PL3095-N05030. As of this writing, the Data from this STR has not yet been received by MCST (but should be on its way). SBRC's conclusion from the engineering model was that while there was striping between channels due to gain variations (intentionally not corrected for EM), there was no pattern noise.

Note that the test that collects data staring at the Space View will miss any pattern noise introduced by the mirror drive motor through the encoder or power/ground circuits. Therefore, successful specification compliance should rely on the results of scanning the inside of the door.

One other issue is that neither of these tests collects data at Ltypical, as required by the specification. Several individuals have questioned whether pattern noise will become visible at Ltypical that was not observed at a zero radiance level. SBRC may wish to justify why the behavior of pattern noise is not expected to be significantly different at different radiance levels, and can be spot-checked by looking for pattern noise in the data collected in other tests (while viewing the calibration targets or on-board calibrators).

Current TAC Software

MCST has a version of the Digital Number display software for the TAC now up and running. This software allows us to display 2 dimensional images of the raw counts. The x-axis is frame number, the y axis is detector/scan number/ mirror side, and the color indicates the DN value (z-axis). The y axis is indexed so that it appears in the display the same way a ground scene would look--from the top down it displays all detectors in scan 1, then all detectors in scan 2, etc. It is possible to display only a single detector or a single scan. It is possible to restrict the number of frames displayed. The color scale can be set to span the entire 4096 count range, the range from Min to Max in the given data set, or between any use defined values.

This software does have some limitations. Only 256 color levels are available due to workstation limits. The current software does not allow us to display digital numbers taken off the on-board calibrators. Finally, there are some bugs that have been identified and are being addressed by Jeff Bowser (GSFC) and Mike Schienle (SBRC).

I recommend we continue to use this software to demonstrate specification compliance, once some of the bugs are fixed.

Proposed Pictorial Display Test

I propose four images be used to verify specification compliance:

1. Single Detector, full scale. The image is 1354 frames by 50 scans, all on the same mirror side. This looks for any large scale structure, outliers, or data glitches. Since the color scale is limited to 256 steps from Black to White, this provides only a general quality check.

Note that SBRC's test currently collects only 5 scans. This creates an image that is a very narrow line, which is expected to be difficult to observe. 50 scans was chosen to enhance the aspect ratio, but will expand the required test time.

2. Single Detector, 1 DN resolution scale. The image is 1354 frames by 50 scans, all on the same mirror side. The gray scale is set such that the quantizing resolution is 1 DN. The mean value matches the mean DN. The range from min and max gray scale values (black to white) should be M, where M is 10-40 levels. The exact choice of M should be dependent on the Band's NEdL--M should be large enough to ensure that the range covers 10 NEdL's based on MSAP estimates of the NEdL. This should capture all the data and allow padding for outliers. Since the quantizing resolution is 1 DN, there is no averaging, or stretching, of the data to levels that will not be observed in a single image.

If the gray scale resolution can be preserved to 1 DN, the Min/Max scale option in the TAC software would be acceptable.

3. Full Band, full scale. The image is 1354 frames by 100 scans, using all the detectors in a band and both mirror sides. This will check for pattern noise in the readout electronics that are shared by the detectors in a band. Note that this may require the application of ground calibration coefficients to normalize the detectors to within the NEdL (as per the channel to channel uniformity specification) if they are not matched in Digital Number space. The current TAC display software does not display calibrated images and would require modifications to do so.

4. Full Band, 1 DN resolution scale, calibrated. Like number 2, this sets the gray scale resolution to 1 DN.

Conclusion

Using the currently planned test and TAC software, SBRC should be able to generate displays with 1 DN gray scale resolution to check for pattern noise. These should be sufficient for demonstrating the absence of pattern noise within individual channels. The current test can check for pattern noise between detectors but may require band-to-band normalization that is currently not part of the TAC software. However, these proposed display tests should sufficiently demonstrate the presence or absence of pattern noise.

Author: eknight@highwire.gsfc.nasa.gov (Ed Knight) at Internet
Date: 10/15/95 3:21 PM
Subject: Memo Reviews Weeks 200 and 201

Week 200 #2537, PL3095-N05275
"Master Curve Approach Applied to EM Test Data," Sept. 7, 1995, by
T. Pagano.

This memo summarizes Tom Pagano's application of his master curve approach to EM test data. This was the subject of a splinter session during the Calibration Peer Review and has been the subject of intense discussion within MCST. One note is that the conclusion claims that the analysis is both preliminary and proves that the method works. It is not clear which of these statements should carry more weight. In addition, the following comments are from Gerry Godden and Larry Goldberg.

Gerry Godden:

Comments Regarding PL3095-N05275 (Tom Pagano's IM Re "Master Curve Approach Applied to EM Test Data":

1) This is a very helpful IM presenting the basic assumptions, reasoning and equations for determining a "Master Curve" of response versus radiance, which may potentially be applied independent of "the optics temperature/instrument background".

2) A key assumption this formalism is based upon is that the FPA response is not dependent on the temperature of the instrument. Translated, or rephrased in different terms, this amounts to saying that when MODIS detectors are at a given operating temperature, their respective responsivities in Watts/Amp or Watts/Volt will be insensitive to the instrument background flux component of the total flux incident on the detector. This of course is definitely not true for low background instruments. Since MODIS is essentially an ambient temperature background instrument (i.e., high background) there is some validity (perhaps good validity) to applying this assumption. When the instrument background flux is sufficiently high (as in the case of an ambient temperature instrument) the background flux raises the level of detector charge carriers well above the regime where charge trapping and other non-linear effects take place.

It would be very helpful to have confirmation of this assumption at the detector level. Most likely the SBRC Detector Division has extant data relevant to this issue for the MODIS or MODIS-like detectors.

Some questions/concerns I have about the validity of this assumption are:

a) what is the range of applicability of the above assumption (especially near detector saturation)?

b) what is the temporal stability of this assumption (especially as radiation effects accumulate). Can the pre-flight measured constant a2 be considered constant up to 10E5 rads radiation exposure?

c) what is the temperature cycling stability of this assumption? Some detector materials go through annealing cycles and behave very differently depending on the range of temperature cycling.

d) what is the meaning/significance of the statement or fact that "some detectors do not behave linearly or quadratically"?

e) what is the scene temperature stability of this assumption?
Conceivably, scenes at temperatures much different than the instrument temperature might require a different "master curve", since the quantum efficiency of the detector is wavelength dependent. If we introduce a "colder optics bench" will this effect the validity range of the assumption?

f) how variable is this assumption from detector to detector? MCT response can be highly variable from detector to detector.

3) Presumably, to implement this for the PFM we will have a set of master curves for the expected focal plane operating range. Will SBRC determine a separate master curve for each of the three FPA operating temperatures (i.e., 83K, 85K and 88K) sufficient to meet required NEdLs/ NEdTs? What is the FPA temperature sensitivity of the pre-flight measured constant, a2 (da2/dT)?

4) In Tom's demonstration using the EM data set to determine the "pre-flight" constants, only one background temperature (280K) was used to calculate the needed constants. What happens if the constant is determined for a high temperature condition (e.g., 320K)? Might there be small differences (significant on the scale of allowed NEdLs/NEdTs) even though the basic assumption of temperature independence is usefully valid? Presumably, the PFM TV data set will allow us to check this.

Larry Goldberg:
1. The EM test data were so bad in the IR bands that no "master curve" that made any sense could be generated from the tests.

2. In the PV bands the dynamic range was severely restricted (see Fig. 3a) due to premature saturation. The linearity as measured in a particular band may not be representative of the full range.

3. Fig. 3b shows two (saturated) measured curves and states that there is good agreement in the LWIR PV bands at the 275K and 305K instrument temperatures. The big problem is that the curves are in direct conflict with Mother Nature; but who is asking for her opinion on what is considered good agreement?

4. Some minor points:
a) The last equation on the second page is not explained or referred to in the text. It is probably an equation that was in an earlier memo on a similar subject;
b) The paragraph heading "V0: Zero Flux Output Voltage from the Detector" on the third page contains the statement "The zero flux output of the detector is measured during pre-flight tests
.Measured ....
.Presumed ....
.Unknown in PC channels; assumed to be zero"
It would be appreciated if Tom described the situation where the photoconductor voltage is zero. A simple detector circuit schematic and a few words would clarify the issue.

I do not think it is a good idea to verify or justify an approach to be applied to the IR channels by referring to the invalid MODIS EM test data.

Action? Ken Anderson should forward these comments to SBRC.

5.0 Jose Florez and Mitch Davis(Microprocessor Speed)

Author: Jose Florez at 730
Date: 10/12/95 3:27 PM
Subject: Status of Electronics 11/12/95

Telephone Message from Ed Clement, 2:40 pm October 10, 1995

Only one item reported this week, but a significant one. SBRC was able to get the Single Board Computer up and running over the weekend. The problem was caused by the additional delay introduced by buffers which were added by a redesign from the EM, and that affected select lines to the Memory Management Unit (MMU). Once corrected, the board was able to operate at 15 MHz, even at low bus voltage. Clay Standford will be consulted for his opinion on the fix that was implemented.

Jose and Mitch

6.0 Neil Therrien (LWIR Light Leaks)

From: Therrien, Neil J on Wed, Oct 11, 1995 13:27
Subject: LWIR 'leaks', x-talk

I have reviewed plots from the scans taken during the LWIR focus and alignment operations. I was UNABLE to see the 10% leak observed on the EM between bands 33 and 27, nor was I able to see the ~1% leak observed near band 31. The test signals were typically low on the PCs (50 to 100 counts) which means:

1) The absence of a leak on band 31 may mean (probably means) that there was not enough signal to see it, and

2) The absence of a leak between 33 and 27 is probably true, since this was a 10% spike which I would have seen.

A quick acquisition with more signal (perhaps a wide, 1 IFOV slit) to check for leaks on these bands can be performed when the OBA is installed, to confirm the absence of our band 31 leak. With respect to band 33/27, we can check for it also at the OBA level. The LWIR leak problem was not characterized on the EM (as far as data acquisitions), nor has it been characterized on the PF. We could perform some spectral characterization if necessary at the OBA level. It would be good news if we did not have any LWIR leaks (that the problem was an EM only problem). But why did they go away?

En EFR HAS BEEN prepared for a x-talk anomaly noticed on the PV LWIR bands 27 through 30. I have looked at this in a little more detail, and I believe it to be electrical. The 'receiver' band's error signal is dependent on the 'sender' band signal. One case I observed was a 10% effect. Whenever the schedule allows, there may be some diagnostics we can perform in this area, such as seeing if there is a dependence on x-talk to detector bias or other voltage bias. Any suggestions, Mary?

Neil

MR
10/16/95