DEIMOS Spectrograph
Response to the Preship Review Report
Version 2.0, Janunary 3, 2002

This report summarizes the status of our progress on the recommendations given in the Preship Review Committee Report of November 28, 2001. It also summarizes progress on other items that were not foreseen or deemed to be necessary at the Preship Review. It is our opinion that we are on track to start disassembly and shipping in early January, and we recommend that this activity commence on January 7, 2002.

This document is intended as background to the DEIMOS Mini-PSR Review scheduled by telecon for January 4, 2001. It is our understanding from Fred Chaffee that the mini-review process will be focussed on the disassembly/shipping decision and on the camera testing process. The PSR Committee made valuable suggestions on several other items that we have not had time to respond to yet. We state our thinking on these in this document but do not include detailed plans. CARA has agreed to work with us on a longer timescale concerning these items.

This document has changed somewhat from Version 1.0 written Dec. 28, 2001. The sections that have changed the most are section 3 on camera testing and sections 2.2.3 and 2.2.4.

We would like to take this opportunity to thank the Preship Review Committee for their wisdom and advice. As always, the review process has proven to be an invaluable stimulus to our own thinking and a major source of helpful recommendations.

Part I contains the items mentioned in the PSR Report. Part II contains additional progress in other areas since the review.

Part I. Items from the PSR Punch-list

2.1 Software Issues

2.1.1. Complete the user GUI and add protections against undesired instrument operating modes.

Good progress was made here. Many improvements have been made to the GUI, and it now is capable of supporting standard observing. We have not yet added the intelligence to screen out undesired or illogical combinations of instrument parameters, but this and other improvements can continue during commissioning, when we know from experience with ESI that the need for other unforeseen changes will arise. The opportunity for De Clarke to make these changes in Hawaii exists on the schedule.

Here is the present punch-list of items for the GUI.

- Turn on limit checking (auto limit-alarms).
- Implement other alarms including the “red question mark” convention for questionable configurations.
- Finalize the rotator panel.
- Finalize the grating detail panel.
- Rationalize the “Pending/Written/Aborted” disk-file indicator for stopped and aborted exposures.
- Fix cosmetic items such as the coercion of all “quit” buttons to have the same label (Dismiss) and the same colour scheme.
- (Optional). Implement some version of the “which setup am I closest-to-using” feature.

2.1.2. Test and calibrate the new piezo fixture.

Done. The new piezo is behaving well. In the process of installing it, we gained more insights into the failure of the previous piezo, which we describe below in Section 4.6.

We also discovered an important error at the PSR regarding the maximum stroke available to the FC system in the Y direction (parallel to the dispersion). This is the motion actuated by the piezo on the tent mirror. The effective stroke is reduced by the anamorphic factor, an elementary point that we had previously missed. This notably reduces the available stroke in spectroscopy mode, especially for high-dispersion gratings. For example, a typical anamorphic factor at red wavelengths with the 1200-line grating is 1.7. Since the stroke was previously marginal in the Y direction using the tent mirror alone, it will now be absolutely necessary to implement a combined approach to correcting the image Y position by using the grating tilt to maintain a coarse position and fine-tuning it with the piezo, and we are planning on designing the FCS control loop this way.

2.1.3. Improve the recovery from SM insertion errors.

Done. The problematic error was the one in which the sensor at the far end of the mask form fails to trip because the mask buckles instead of sliding under the end rollers. This can happen if a mask becomes curved the wrong way, as develops after many dozens of insertions. When this error occurs, it is now possible to back out the mask and try again, which in itself often cures the problem. If it does not, it is necessary to visit the instrument, remove the offending mask, and manually bend it backwards. The defense against this problem is to inspect each mask at loading to make sure that it is straight.

2.1.4. Complete and test the high-level barcode reading software and verify its interaction with instrument configuration software and procedures.

Good progress was made on this item, and we anticipate meeting this recommendation by January 7. All barcodes are now installed. The fixed barcode scanner reads barcodes fairly reliably. Once in awhile it has a buffer initialization problem, but this will be compensated for when we write the client software. The handheld barcode scanner is simpler and works reliably. The script to inventory barcodes on loaded slitmasks is also finished; it takes 3 minutes to run (reduced from 15 minutes). A companion script was written that inserts each mask into the slitmask form on a trial basis to spot any masks that have been cross loaded and fail to insert; it runs takes about 5 minutes to run.

Still remaining as of this writing is the instrument configuration software itself. It is our understanding that we are to provide a prototype version of this software that has been tested with the instrument at least minimally before disassembly. Such a prototype has been designed but has not as yet been coded. It makes use of the fact that it is possible to scan the barcodes on filters and gratings with the hand-held scanner *after* these elements are installed, just as for slitmasks. The installation software then automatically knows which slot is being scanned, which makes the interaction with the operator easier.

The proposed design is as follows: A daemon constantly watches for hatches that open at designated load PAs for the filters and gratings. When a potential load condition occurs, it listens for barcode events and records the barcode number at the current location of the filter wheel or grating load position. The scanned element must be in the instrument at its correct location, and the correct hatch and PA must be observed. To provide feedback to the operator, we suggest audible success and failure signals sent through a set of headphones that would be worn by the operator. The operator would communicate back to the software via command barcodes affixed to the outside of DEIMOS.

This plan obviates the need for typing on a laptop on the Nasmyth platform, something we never liked. The alphanumeric name for each barcode, which is needed to load the stage menu on the GUI, would be entered into the database prior to loading.

By giving high priority to this item this coming weekend, we believe that we can have a prototype version of this system working by January 7. However, even if this is not possible, we note that we do have a perfectly serviceable configuration code that is already working and could suffice for commissioning and early observing. Thus, we do not recommend delay in disassembly should this item not be fully completed.

2.2 Analysis and Test Issues

2.2.1. Run unattended tests to exercise all mechanical parts of the instrument.

Effectively complete. Extensive ktests were run and the following items are illustrative of the problems that were uncovered:

1) The science filter wheel motor encoder is sometimes found to be outside the specified window of 100 encoder counts, which corresponds to 0.03 inch at the edge of the filter wheel. The filters all weigh the same amount to within 4 oz., so it is not an out-of-balance problem. Measurements indicate that the motor encoder is drifting but that friction is actually keeping the wheel in position. No further action is planned.

2) The actual TV focus is systematically slightly positive relative to the desired value by up to 10 motor encoder counts. However, it may be only the motor encoder that is moving when the stage shuts off; like the science filter, the TV filter wheel itself may be properly located. If real, the discrepancy corresponds to a geometric focus blur of $0\farcs1$, which is not detectable in images. The main consequence is to make it impossible to step the focus in units as small as 10 counts, as the stage thinks it is already within the desired window. The instrument is operable in its present state, and no further action is planned.

No other adverse interactions were found when all the instrument systems were run in concert. In particular, no strong correlations were seen between motor stage failures and either PA or active rotation. Ktests will continue up until the actual moment of disassembly.

2.2.2. Test grating clamp-up while slewing the instrument rotator.

Done. Considerable information on grating clamp-up reliability was available prior to the Preship Review. As of that time, all sliders clamped up with 100% reliability while tracking, and with 90% reliability while slewing at the maximum rate. Since then, some adjustments were made to the clamp-up routine and one further clamping-while-slewing test was run on each of sliders 3 and 4; no clamp-up failures occurred. Please note that, in the event of a clamp-up failure, the software now automatically includes a retry. As a final fallback, clamp-up should occur with complete reliability when converting to tracking speed after slewing.

2.2.3. Understand the effect of clamp-up variations on image rotation.

Done. A full series of flexure images was taken in spectroscopic mode while clamping up at 6 out of 8 possible PAs for slider 4 and at 8 out of 8 possible PAs for slider 3. Spot groups were identified at opposite ends of the X-axis on the science detector (spatial direction, called A and B) and at opposite ends of the Y-axis (dispersion direction, called C and D). The phenomena seen with the two sliders differ in detail but broadly are as follows:

- The relative motion of group A vs. group B in the Y direction is a measure of rotation and/or image shear. This is an important direction, as any motion along Y changes the wavelength on a pixel, and thus leads to flat-fielding errors as well as image blurring. The observed motion is ≤ 1 px for both sliders, which is smaller than the quoted value of 1.5 px at the PSR based on preliminary data. Thus, we are more than meeting the error budget for this term.
- The relative motion of group C vs. group D in the X direction is likewise a measure of rotation and/or shear. This combination shows a more complex behavior. At a given clamp-up angle, the curves resemble those for A vs. B, being smooth sinusoids with a total amplitude of 1-1.4 px. However, at different clamp-up angles the curves are systematically displaced from one

another. The total range covered is 2.4 px for slider 3 and 3.6 px for slider 4, which are 1.6 and 2.4 times the nominal error budget.

However, this is not as bad as it seems because image motion along the X direction does not lead to wavelength shifts or flat-fielding errors. The observed behavior thus has two negative effects, both of them mild. First, the motion within a single clamp-up causes image blurring. However, this motion is again within the specified 1.5 px limit and is thus not too damaging. Second, there could be an image motion *perpendicular* to the dispersion of as much as $3.6/2 = 1.8$ px worst-case between an afternoon flat-field and an evening observation (this is for slider 4; the maximum for slider 3 is 1.2 px). Spectra will appear to translate sideways, in one direction at the far blue end and in the other direction at the far red end. The flat-field registration will consequently not be perfect in these places, and flat-field data will not be available to flatten all the pixels in the observation. If this were crucial, the observer would have to write off the top or bottom two pixels of every slitlet, incurring an effective loss of available slit length. For 100 slitlets, the loss would be amount to 200 pixels along the spatial direction, or a loss of 2.5% in total observing efficiency. This is annoying but not devastating. We stress that this is the worst case—for the vast majority of calibration scenarios, the motion is less than 2 pixels, and this is the maximum motion at the very ends of the spectra. The great bulk of the data will be less affected.

The above summary differs considerably from that in Version 1.0 owing to the analysis of more data.

2.2.4. Understand the image droop and either eliminate it or demonstrate that it will be fixed by the FCS system.

Effectively complete. We took two kinds of data to study image droop: image motion data from which a full flexure solution can be derived, and shadow data using pinholes to illuminate dust on any moving camera elements. The shadow data are thus far inconclusive in that no measurable shadow motions are detected. However, this null result implies that any drooping elements must be near the front of the camera, as there spots are softer. Drooping of Element 3 would be compatible with this conclusion. In particular, any droop of the detector relative to the dewar window is strongly ruled out.

The full flexure analysis implies that the droop motion must be post-grating but cannot specify it more directly than that. However, when combined with the results of the shadow motions above, the data indicate that the droop must be occurring near the front of the camera. Finally, we observe that the droop is not axially symmetric with respect to the camera, as its behavior varies strongly with DEIMOS PA. This is evidence against the droop's arising in Multiplet 1, as its elements are supported by axially symmetrically annuli of RTV. Multiplet 3 in contrast is supported by three hard pads, which are not axially symmetric. Thus, though perhaps not airtight, the total picture points strongly to the droop's arising in Element 3. If so, it should be fixed when we stabilize this element.

Regardless of the exact origin of droop, we have established that the droop motion is a pure translation to within 0.1 px, and thus the FC system should be able to remove it.

2.2.5. Align the gratings and the grating sliders.

This process is complete for now. The gratings and the grating sliders have been aligned, but the direct imaging mirror must wait for Hawaii when the final Zerodur mirror is installed. The following grating alignments were achieved (1 px = $0\farcs12$): 1) the X-zeropoints of each grating individually agree to within 20 px between the two sliders; 2) the X-zeropoints of all gratings agree to within 16 px in a given slider; 3) the spectral walk of each grating on the detector as it is tilted is less than 16 px for any grating/slider; 4) the divergence in spectral orientation for all gratings in all sliders is less than 10 px peak-to-peak across the width of the array; and 5) the total X-range occupied by spectra produced by all gratings in all sliders at all tilts is 30 px, or 3.6 arcsec. This is the amount of dead space that will have to be left at the top (or bottom) of each pair of CCDs if one wanted to observe the same set of object slitlets with maximally different spectral locations. This is a loss of 1.5% of the total slit length, which is tolerable.

2.2.6. Carry out more careful fringing tests to assess flexure control system requirements.

Done. The fringe amplitude on CCD4 (a Lot 14 device) was measured to be roughly half as large as we assumed at the PSR, but the fringe period was found to be twice as short. The propensity to fringe assumed in the PSR is therefore essentially correct. More precisely, wavelength shifts of up to 0.6 Å can be tolerated on a pixel and still meet the 0.2% flat-fielding stability requirement for a 600-line grating. The number stated at the PSR was 0.5 Å.

2.2.7. Check that slitmasks are inserted properly under all expected rotator tracking conditions. Try to improve the slitmask insertion at PA=0.

The problem at PA=0 was solved by increasing the insertion air pressure from 30 lb. to 40 lb. Masks now insert with good positional repeatability at all PAs in static tests.

We have not yet tested the reliability of slitmask insertion at all PAs while slewing; this is a last-minute test that we will report on at the mini-review. Further, due to the complications caused by flexure and moving camera elements, we have been unable to devise a test to confirm the repeatability of slit mask insertion while slewing; any lack of slitmask repeatability is too small to be detectable in the face of these larger variations. This second test is therefore on hold.

2.2.8. Test the rotator repeatability more carefully.

Good progress was made here though the data are still being analyzed. Recall that the specified tracking accuracy of the rotator is 17 arc sec, which corresponds to a tracking error of $0''.05$ at the ends of the slit. Several tests and procedures were carried out:

1) A laser, level, and a mechanical height gauge were used to measure simultaneously the ability to turn exactly 360° using the keyword ROTATVAL. This quantity is DEIMOS' best guess at its true PA and is based on an average of the two separate Renashaw encoder readheads. A scale error was found amounting to $57''$ in ROTATVAL, which was traced to an improper floating-point value truncation. This was corrected, and any scale error in ROTATVAL over one rotation is now less than 2 arcseconds.

2) The laser is the most precise of the three measuring methods, with a precision per measurement of about $4''$. At this level, no evidence of non-reproduceablility in PA could be detected over many separate rotations of 360° .

3) The level and a gauge block with accurately parallel sides were used to rotate DEIMOS by known 180° increments to look for resultant errors in ROTATVAL. The starting PA was varied in order to trace out the error $\delta(\mathrm{ROTATVAL}_{180})$ vs. starting PA. The resulting curve can be used to measure any error in ROTATVAL of the form $\cos\theta$. A small error term was found having an amplitude of $\pm 34''$. This was corrected using an interpolation table, and the test was repeated. No $\cos\theta$ error remains in ROTATVAL at a level of about $\pm 5''$, which is the level of precision we can determine using the level as an absolute angular standard.

4) Taking measurements in the above test spaced 180° apart (necessitated by considerations of access to the level) aliased out any term in $\cos 2\theta$. However, because the Renashaw read heads are 130° apart, such a term can be detected by looking at their difference. This difference proves to be quite large, $\pm 270''$, but is mainly of the form $\cos\theta$. The data are currently being analyzed for any $\cos 2\theta$ term, and the results will be available January 4.

5) A mechanical model was developed that accounts for the origin of the $\cos\theta$ terms in both ROTATVAL and (Renashaw(1) – Renashaw(2)). The model was motivated by runout measurements that were taken of the surface on which the Renashaw tape is mounted. The total TIR of this surface was found to be 0.003 inches. We assume that the surface is intrinsically circular but eccentrically mounted on the axis of rotation of DEIMOS. This model together with the location of

the Renashaw heads explains both the large difference that is seen in (Renashaw(1) – Renashaw(2)) as well as the smaller $34''$ correction term in ROTATVAL. Because the heads are nearly 180° apart, any $\cos\theta$ term nearly cancels in ROTATVAL and the remaining error in ROTATVAL is small, as observed.

6) Correction data in the form of interpolation tables were gathered for each Renashaw head separately versus the (now corrected) value of ROTATVAL. This value of ROTATVAL should be quite accurate provided error terms of the form $\cos 2\theta$ and higher are not significant. The data were taken at a variety of position angles and speeds to detect possible errors in the Renashaw readings versus speed and direction. The analysis of these data will be available on January 4.

7) A level was mounted on the cover over the Renashaw heads, and readings were taken vs. PA. The cover is rigidly attached to the Renashaw mountings, and any tilt or clocking of the mount system as a whole should reveal itself as a tilt of the level. Such a tilt would translate directly to an error in DEIMOS PA. Only small tilts were detectable, amounting to $\pm 3''$. Similar measurements were made to detect rotations of the entire back end of DEIMOS using height gauges above the floor. Again, rotations of order $\pm 3''$ were found, small compared to the allowed error of $\pm 17''$.

The above tests suggest the following conclusions regarding the PA drive system:

1) Static tests of the PA system are encouraging and indicate that the system should be capable of meeting the PA accuracy requirement of $\pm 17''$. Errors in the static tests are at the level of $10''$ per measurement, which is fully attributable to the accuracy of the level. The results of the dynamic tests (item 6 above) are still TBD.

2) The present calibration methods can measure the zeropoint and scale of each Renashaw head separately, plus the amplitudes and phases of any $\cos\theta$ and $\cos 2\theta$ correction terms. Measurement of the $\cos 2\theta$ term from the difference Renashaw(1) – Renashaw(2) is still in process. Limits can be placed on the existence of higher-order terms at the level of $\pm 10''$, but the form of these terms cannot be derived. This level of calibration should be adequate to meet the required accuracy spec of $\pm 17''$.

3) The individual Renashaw heads have a significant error term with an amplitude of about $\pm 300''$. Should one of these heads drop out, it will be necessary to apply a large correction to the remaining head. The correction terms therefore need to be measured and applied to each head separately. The software was designed to allow this, and the necessary calibration data are being taken.

4) The measured TIR of the Renashaw tape surface of 0.003 inches is near the limit that can be tolerated by the Renashaw heads. No dropout failures have occurred so far, but there is some concern that we are near the limits of the system. There are no plans to do anything about this at the present time.

5) Data have been taken to derive the necessary correction tables for each Renashaw head separately. However, these tables depend on the exact mechanical configuration of the system (the Renashaw tape TIR, to be precise) and should be remeasured during commissioning. The 360° laser rotation test, the 180° level test, and the Renashaw(1) – Renashaw(2) difference test will be redone for this purpose prior to putting DEIMOS on the sky.

Remaining work this week on the PA drive system includes: final tuning of the servo-loop parameters, final spot checks of the Renashaw interpolation tables using the level test, and fixing a present inability to change modes gracefully between tracking and slewing. This last is a neatness issue that should not delay disassembly.

2.2.9. Check the CCD response to overillumination.

This question has two parts:

1) What is the magnitude of the residual image after overexposure?

After CCD saturation by a factor of 10, the next dark image has a residual amplitude of 1-2 DN. We will measure the decay in this image over subsequent exposures this coming week. (Note: as of Version 2.0, this test is being deferred to the CCD lab for lack of time.) We have a scheme that might further reduce the magnitude of the residual image, which we will test with the red detector in the CCD lab.

2) What is the nature of the cross-talk between CCDs, and how large is it for very bright images?

Cross-talk at the level 3 DN (out of 64,000 DN) has been known to exist for some time between the A and B amplifiers on the same video board owing to a design error in the Leach II boards, probably cross-talk through a common ground. Recently, however, crosstalk of similar magnitude (but variable sign!) has been seen between *different* video boards. The exact cause is unknown but clearly reflects some interaction in the CCD electronics on broader scales.

The first kind of cross-talk is a feature of the present system that is deeply ingrained—there is no plan to improve it or remove it. The second kind of cross-talk will be pursued further in the CCD lab when the red detector is assembled. Neither impacts the decision now to start disassembly.

2.3 Mechanical and Electrical Issues

2.3.1. Disassemble the camera lens to identify the source of image motion. Improve the lens mount and retest for image motion.

Not relevant to disassembly. For further discussion, see the discussion of camera testing in Section 3.

2.3.2. Stabilize the mounts for the FCS fibers.

Effectively complete. The fiber mounts were tightened and hysteresis eliminated. The FCS1 fibers are now very stable with respect to the science detector. The FCS2 fibers have a small sinusoidal motion of less than 1 px pk-pk, but the motion is repeatable and modelable. Any residual motion relative to these smooth curves is less than 0.1 px rms; we propose to take them out in future FCS software. All fiber flexure motions will be remeasured in Hawaii during commissioning.

2.3.3. Reduce the illumination level for the FCS system and improve the baffling to reduce scattered light on the science CCD array. Verify that the resulting light levels allow sufficient S/N for the FCS system.

Complete for now. A BG40 (1mm) color-balancing filter reduces the flux in the red by about 1000 while maintaining the flux from faint lines in the blue. Diffuse scattered light on the science detector in a 1000 sec exposure is now undetectable. We still have streaks scattering onto the detector that originate where the FCS spectrum crosses the edge of the dewar window. As explained at the PSR, we believe that these streaks are caused by light bouncing off the vertical lip that holds the dewar window. An opaque baffle to cover this and other unused areas of the dewar window will be installed with the red detector and should cure this problem.

An improvement in FCS S/N by a factor of 1.5 was obtained by increasing the integration time per pixel in the FCS controller from 1 μsec to 4.88 μsec. This increases the readout time per px by 8 μsec and the overall readout time for each FCS detector by 1 sec. However, it also decreases the RO noise from 9 e^-/px to 6 e^-/px, which is the best these Orbit CCDs can do. This change will improve the centroiding accuracy from faint lines (see below). It also cured the low-level fixed-pattern noise that afflicted these CCDs; cosmetically the FCS noise now looks much better.

Tests with a preliminary centroiding algorithm indicate that, after all these changes were made, CuAr spots of adequate brightness fall on the FCS detectors for all gratings and grating tilts. Centroiding noise is less than or equal to 0.1 px rms in each coordinate. However, the situation in the far red is somewhat marginal; the far-red lines of CuAr beyond 9000 Å are widely spaced apart and are still faint with the current BG40 filter; every line must be usable to cover all grating tilts at high dispersion. If more light in these red lines proves to be needed (it would not be more than a factor of two), we will custom-thin some BG38 filters by various fractions of a mm and try them out at commissioning. The BG38 (2 mm) transmission curve is nearly identical to that of the present BG40 (1 mm), and thinning by about 0.2 mm would increase the transmission in the red by the needed factor of two. The amount of extra diffuse scattered light would still be negligible.

2.3.4. Add final instrument baffles and eliminate light leaks.

This is complete except for a bit of testing. The twin baffles that cover the focal plane area have been installed. They are flocked on the rear surface as well as the front. Many smaller baffles covering up cables and other holes in the focal plane were also fabricated and installed. Still smaller holes were plugged temporarily with tape, which we will install more carefully at Keck.

The interior of DEIMOS was checked for light leaks using an infrared camera. The interior is basically quite dark with no light pollution coming from internal electronic devices. A few light leaks around the cable holes leading from the interior into the electronics bay were noted and plugged with black cloths. All three man hatches leaked light; their edge seals were improved and their inner surfaces blackened; they will be tested further next week. The biggest unsealed light leak is the 1-inch gap between the drive disk and the rear of the nose cladding. Two different sealing methods were tested, and an interlocking layer of black foam with a light trap between was selected. A test will be tried next week, and the final version will be installed during teardown when access to these surfaces is easier.

We also tracked down and cured (in temporary fashion) all visible traces of scattered and extraneous light on the detector in both spectroscopic and imaging modes. This light was coming either from holes in the focal plane baffles or from light reflecting off the detector, traveling back up the light path, and bouncing off shiny rear-facing surfaces in the slitmask area. We plugged all holes and blackened the rear-facing surfaces and got very clean-looking spectra. At disassembly, we will inspect the back of the focal plane area more carefully and flock every shiny surface.

2.3.5. Insert and test a comb structure to prevent incorrect slitmask loading

There was a slight misunderstanding here. The new comb structure is intended to reduce the chance of cross-loading but cannot eliminate it. We made the new structure and it indeed is an improvement, but cross-loading can still occur. As a further measure, we have written a script that test-loads all masks at the time and detects any that are cross-loaded (see Section 2.1.4).

2.3.6. Replace the grating drive sprocket.

This was done, and the grating system was subsequently requalified.

2.3.7. Install the barcodes. Test the function of the barcode reading software as called out above.

Done. For further information, see 2.1.4.

2.3.8. Pin the optics for easy reassembly.

This is in the teardown plan; it is not relevant for disassembly. The process of measuring and pinning the optics has actually started.

2.3.9. Adjust the dewar focus mechanism for the proper range.

We decided not to do this before teardown as access to the mechanism is difficult and the adjustment is extremely delicate. We note that there is no question about our ability to set to a desired focus—we simply set to the wrong value. The focus will obviously be reset more carefully when we install the red science array.

3. Testing the Camera Lens

3.1 Items from the PSR Report

There are essentially four points raised in the PSR Report regarding the camera:

3.1.1 Cold-test the camera at operating temperature as soon as practicable.

We agree that cold-testing the camera is a crucial matter. However, it hopefully will not be as long as 6 months before we get cold images on the current schedule, which shows the camera fully installed and ready to take images by mid-April. The question is whether we should work hard to get cold images before that date.

There are basically two options for cold-testing the camera: a) locate a cold facility in California or some other nearby place in the continental US and test there; b) ship the camera to the summit of Mauna Kea and test there. Either way we have to fix the moving element in the camera first, so the ship date is the same. Testing at an entirely unfamiliar facility would require planning and effort and might even delay the first images at Mauna Kea. Alternatively we have considered a "cool" test at Mt. Hamilton, which could reach as low as 45-50 F. It would be conducted by imaging an light source in the near field of the camera. Raytracing shows that the camera produces images that are acceptable for diagnostic purposes with objects as near as 2000 inches (167 feet). For this test, the camera would be set up in the basement of the 120-inch dome and image a pinhole located in the parking lot. We prefer this test because, unlike our standard collimator lab test, it illuminates the entire camera aperture, which is needed to bring out the comatic tails. These tails are strongly visible in spectroscopic mode, not imaging mode, because of the longer effective pupil length from the tilted grating. Our standard lab test in the past illuminated the camera with parallel light from a 6-inch collimator, which was not large enough to excite the tails. This is why we did not see them until we put the camera in the spectrograph. It is clear that the manner of illumination of the camera is critical in any proposed test and that having the ability to illuminate all or most of the aperture is needed. This "cool" test on Mt. Hamilton is the best option we can think of for testing in California.

Let us now consider the pros and cons of conducting the same test on Mauna Kea rather than on Mt. Hamilton. The dates of the two tests would be about the same, just after the moving element is repaired. On MK, the camera could be placed in one Keck dome and the light source in the other, taking advantage of the hall that connects the two. The total path length available exceeds 200 feet. Moreover, the camera would actually be located in a real Keck dome at a temperature of 0 C; no extrapolation to the operating temperature would be necessary. Finally, this test provides a check on the final assembly of the camera in Hawaii, whereas the camera tested on Mt. Hamilton would be disassembled. On the minus side, we would lose advance knowledge of any problems before leaving California. However, given the approximate nature of the Mt. Hamilton "cool" test, we feel that it would be necessary to test the camera more precisly at temperature on MK anyway before formulating a repair plan. If so, the earlier Mt. Hamilton test would contribute very little.

The delay caused by adding such a cold test to the schedule should be small. The test could fit into the two weeks of slippage between the arrival of the camera on Mauna Kea and its installation in the spectrograph. We already have the necessary hardware to attach the detector to the back of the camera. We would probably want to do a dry run of the test in Santa Cruz for practice, but this should not take more than a couple of days.

What might such a test on Mauna Kea reveal, and what would be our response? The possibilities are as follows:

1) Asymmetries across the field of view, most likely coma, indicating tilts or decenters. Though we will check for these previously and remove them (see below), new such aberrations might become visible on account of the better images. Since they likely would be small, the response would be to retune the lateral adjustment of Body 4, which can remove small amounts of lateral coma.

Alternatively, large amounts of asymmetry seen at this stage would signal that the camera was assembled wrongly with decenters. We would then repeat the assembly process, either in Waimea or on Mauna Kea (see below).

2) Failure of the radial coma to resolve as predicted by the thermal model. Depending on its size, such a discrepancy could be extremely serious and would indicate some source of radial coma in the camera due to a fabrication error. The two most likely sources are a wrong aspheric coefficient or a wrong index of refraction. Our response to serious, persistent coma would be to convene a committee of experts to advise us on future optical tests and measures that could be taken. Meanwhile we would continue commissioning of the spectrograph; this requires use of the camera and would also yield better test optical data on the camera problem. From what we know now, it appears that resurfacing or refabricating one of the aspherics in multiplet 4 could remove persistent coma. Making an entirely new element would allow the camera to remain in use during the fabrication period. Further speculations now on a detailed repair plan are premature.

3) Other unforeseen aberrations. The response here would depend on their size and type. We are in the midst of systematically investigating the effect of all fabrication errors on the nature of the images to identify the kinds of aberrations that may be expected. So far, the only major categories to appear are the aforementioned lateral and radial comas. If the new effects were large and we could not identify them, we would again consult outside advice. A repair that simply required rearranging the multiplets could be accomplished in Hawaii. A repair that required disassmbling the multiplets or making new elements would require taking all or part of the camera back to Santa Cruz.

Essential to the above plan is an image-quality specification that, if not met, would warrant extensive reworking of the optics. As noted, the most likely extensive reworking would be the repolishing or refabrication of an aspheric element. This would be time-consuming and costly and would not be undertaken without careful study. We prefer not to state a simple criterion here for this threshold image imperfection, as the scientific implications could depend on the exact nature of the problem. Instead, we would present test data on the optics as operating on Mauna Kea and have them reviewed by both astronomers and optical consultants. This program would be supervised by the Science Steering Committee in concert with CARA and would provide yet another occasion for outside optical expertise and advice. During such a study, DEIMOS would continue to be used and further information about the optical performance gathered.

In conclusion on the question of testing, we have decided to repeat our 6-inch collimator lab test in Santa Cruz after the repair of the moving element. This is in addition to the moving spot test that we will run in any case to verify that the moving element has been stabilized. Ray-tracing shows that a 6-inch collimator test can detect even moderate decenters, giving us a chance to test the ability of Body 4 to remove them. It also measures back focal distance. If this quantity is still incorrect (see below), there would be an opportunity to try retuning by adjusting the spacing of the multiplets while still in the laboratory.

3.1.2. Measure the back focal distance as a clue to camera performance.

The back focal distance is approximately 0.030 inches too short for the respaced camera at 22 C. (This number is absolute, not relative to the presently improperly located detector.) This is a significant discrepancy. We will investigate this by carefully rechecking all the element spacings as we disassemble the camera when it comes out of the spectrograph and as we reassemble it after fixing the moving element. Then we will remeasure it using the 6-inch collimator test after repairing the moving element. We will also use raytracing to understand the effect of all indices of refraction and element spacings on back focal distance.

3.1.3. Carefully compare the actual and ray-traced images both on and off axis to see if theory agrees with observations for the warm camera.

This is not easy at present because of the considerable number of already identified parameters that would have to go into such a model, some of which are poorly known. These include the locations of both moving elements (which are affected by hysteresis), the axial and lateral displacements of Body 4, the known miscentering of Body 1 (by 0.002 in), and the inability to go through focus with the current detector location. Given this large number of known errors (and their uncertainties), it is not clear that more careful analysis would be able to uncover the existence of yet smaller errors lurking below the surface, which was the committee's concern.

A second objection is practical—we do not possess in Santa Cruz a warm model with which to experiment and would have to re-engage ORA for what could amount to the investigation of a large parameter space.

It seems a better use of resources to fix the moving element as rapidly as possible and conduct the cold test described above on Mauna Kea. At that point, the Santa Cruz optical model should be valid and all known alignment problems corrected. Comparison to models will then be much more meaningful.

3.1.4. Check the viscosity of the Cargille optical coupling fluid LL1074 at –20C. A DEIMOS published paper reports that this fluid will not pour out of a test tube at an operating temperature of 0 C.

We apologize for this error. There was evidently a typo in our paper on tests of optical fluids. We have retested the viscosity of LL1074 and find that it indeed flows readily with the consistency of a thin syrup at –20 C.

3.2 Additional Camera Questions Raised by CARA

3.2.1. Consider assembling the camera element in Waimea rather than on the summit.

Waimea offers the advantages of more benign conditions and a longer working day. The summit offers the advantages of more space, less disruption to ongoing activities, a possibly better-controlled thermal environment, and on-site opportunities for optical testing. We are looking into both options with the help of Greg Wirth and Bill Mason and are preparing a memo setting forth the lifting, thermal, space, tooling, and other requirements for camera assembly. This memo will be useful regardless of where we actually decide to assemble. We would like to have the assembly location finalized by the third week in January.

3.2.2. What is the fall-back plan in the event that testing of the camera on Mauna Kea reveals problems?

This subject has been covered in section 3.1.1 under the discussion of cold testing.

4. Other Recommendations and Comments

4.1. Coating at Newport may be a schedule driver. They need considerable logistical and engineering support to coat large mirrors, and the process can be slow.

We take note of this concern and have been pursuing this task energetically. Final coating designs are in hand, and a packet was sent with drawings of the optics to be coated and the present handling fixtures. We have an appointment to discuss this packet on January 2, during which they will note what changes need to be made to our equipment, and we will design and fabricate them. Newport is responding rapidly to our emails and telecons and say that they are prepared to coat our optics in February. They appear to have learned a lot from the previous SAO mirror of comparable size. In short, so far, so good.

4.2. DEIMOS rotation via remote control should be disabled when someone enters the Nasmyth platform. When someone is on the platform, only manual control from the platform should be enabled.

We are committed to implementing this but need to find out from CARA what kind of signal they will provide to DEIMOS to indicate the presence of a person on the platform. Also, we understand the safety concerns that motivate this recommendation but wish to reiterate the importance to observers of having control over the PA rotation system during daytime testing, independent of the DCS system. The design of the personnel interlock system should take this need into account.

4.3. Can a second beam be accommodated without exceeding the weight limit of 20,000 lb?

This is a good thing to know, but it does not affect the teardown date. We will do it, but not now.

4.4. Review the lifting plan into the Keck dome.

The lifting plan is now under the direction of Kyle Kinoshita of CARA.

4.5. Bob Kibrick may be overcommitted during the next three months. During this time he is to characterize the new CCD array and to code the flexure control software, along with other tasks listed in Section 2.

As of January 7, all items involving Bob from Section 2 will have been completed. The remaining two items are major, and we propose to develop the schedule for Bob during the week after January 4.

4.6. Consult the vendor about the reliability of the piezo.

We sent our mechanical drawings again to the vendor and should have more information on that by January 4. However, two electrical conditions came to light while installing and testing the new piezo that may shed light on the previous failure. First, we learned that overdriving the piezo by applying too high a voltage can lead to failure. It is not possible to do this using the control panel, but it is possible to do so under software control. We can now see that our previous software instructions were outside the safe range. In a related mode, two loose electrical connections were discovered that may have interrupted the applied voltage. Since the piezo operates in feedback mode and increases the applied voltage if the desired motion is not achieved, a sudden good connection could result in a dangerous voltage condition. Both of these problems have now been addressed: the improved electrical connections have been installed, and software now places strict limits on the voltage that can be applied.

4.7. The installation schedule needs to be reviewed and fleshed out in considerably more detail.

Done. The installation schedule was extensively discussed with CARA during a week-long visit by Dave Cowley to Hawaii in December.

4.8. The schedule for instrument tests during commissioning needs to be fleshed out in more detail. This includes both on-sky and off-sky tests. Discuss with Observatory management the possibility of more than the usual 10 nights of on-sky testing.

This item is still open but should not affect the decision to start disassembly. We will address it in the next two weeks.

4.9. Multi-HDU files should be implemented at the time of commissioning.

This capability already exists but has not been extensively tested because it necessitates using ds9 as the quick-look real-time analysis tool. Ds9 has several bugs with mosaic images, some of which have been fixed since the PSR but some of which remain. We expect that these problems will soon be rectified, and we will be working with ds9 and multi-HDU images in the CCD lab while testing the red array.

4.10. The slitmask alignment procedure should be developed and documented for novice observers.

Yes.

4.11. Measure the time needed to produce and clean a full 130-object slitmask.

A first test took 34 minutes to drill the slitlets, plus 5 more minutes for cleaning. However, the slitlet edges were ragged and far inferior to those on previous masks we have made. This test needs to be repeated.

Part II. Items not on the PSR Punch-list

This section describes progress in several other areas that were not noted at the time of the PSR.

1. Improvements were made to the grating dithering software that have important implications for the functioning of the whole instrument. The dithering software was consolidated onto a single Galil controller thread, and we ceased dithering the grating that is not installed. These two changes made the rest of the control software run quicker and smoother. The changes entailed major alterations to the Galil code, and some bugs were generated along the way. These problems have now been dealt with but did delay tests of the various aspects of the grating system.

2. Galil software was completed to handle the manual pushbuttons for unloading the filters, but similar software for the gratings is not finshed. Moreover, experience with running the grating transport system showed the need for keyword and dispatcher changes to allow a grating change operation to be reliably halted. The disassembly schedule maintains the grating system and its associated electronics intact for the week of January 7 in order to complete work on these two items.

3. The SM cassette intermittently refused to move due to a cold solder joint in the slitmask comb-misalignment detector. An interim fix was made, but finding this problem delayed the running two key SM reliability scripts. The culprit circuit board needs to be reworked and some sort of shock-absorbing mounting provided.

4. An old, intermittent problem with the CCD power supply and power monitor board recurred. The power monitor board detects a momentary loss of the -16v supply, and it turns off all analog power to the CCDs. Resulting images read out as all zeroes or near zeroes. We managed to make the problem go away but suspect that it may strike again. The problem should not delay disassembly because it cannot be fixed until it worsens.

5. A large, previously intermittent attenuation of the video signal from CCD3 also worsened. It was traced to a bad coax connector in the CCD electronics box and fix(ed.

6. We successfully swapped in nearly all of the new CCD 61-pin cables with redesigned connectors. The connectors include stiffening collars that protect the wires inside from stress. However, at present we are operating with only half the science array owing to a bad connection that developed on a board that interfaces to one of the 61-pin connectors in one of the electronics boxes. This bad connection appears to have been exacerbated by stress put on by the new cable, which has a subtly different fastener than the old one. Fortunately we can finish all necessary tests in the remaining week with half an array and will fix this problem in the CCD lab. (Note for Version 2.0: we have since replaced the suspect board, and the whole array (minus CCD4) is now operating again.)

7. The science filters were compared for optical quality and consistency of focus. They are indistinguishable at the present level of image quality except for the BAL12 clear spectroscopic window, which focusses farther forward than the others by an estimated 20-30 μ. Image positions were also compared and the differences were at most a couple of pixels, indicating that wedge is small, as specified.

8. The TV guider focus was adjusted to put it near the middle of the range, and best-focus was measured for the various TV filters over the full field-of-view. There is some variation among the filters, but the differential geometric blurring is only about $0\farcs25$. If confirmed on sky images, we might want to institute a filter-focus discipline for the TV like the one we envision for the science filters. However, the TV is ready for disassembly.

9. The mystery absorption lines that were noted in the red spectral region of the internal quartz lamp are present in every continuum source we have looked at, including many incandescent sources

and a visible-light LED. The lines are 5-10% deep and localized to 9300-9600 Å. We now suspect that they are caused by something inside the spectrograph as they are always the same intensity regardless of the source of illumination. An obvious culprit was the optical coupling fluid, but an absorption spectrum through a vial of it it was clean. Fortunately, the features are not very deep, and the same positional accuracy needed to control fringing also suffices to flat-field out these features. We do not see a way of making further progress on this problem.

10. The original internal quartz continuum lamp (which is piped in via fibers) proved to be too faint, and we have replaced it with a 40W Sylvania halogen bulb mounted directly in the nose. Seven candidate incandescent lamps were tested, and the Sylvania was the brightest and the bluest. Exposure times are less than 10 seconds at all dispersions except for the deep blue. We considered installing a blue LED along with the halogen lamp, but its intensity in the dim region below 4500 Å was not much of an improvement over the halogen lamp.

11. We attempted to increase the slew speed of the grating select drive and initially reported success (a near-doubling of the speed), but that proved to be illusory as a lateral resonance was excited in the counterweight screw. However, even though the slew speed cannot be increased, grating clamp-up time can still be reduced significantly by streamlining the clamp-up software. The time that can be saved is comparable to the time we would have saved by slewing faster. These improvements can be implemented at commissioning.

12. Distortion in spectroscopy mode was mapped using Phillips' optical model, and a correction function was derived for slitlets located at different Y-values that successfully eliminates spectral collisions between neighboring slitlets. This is important for stacking slitlets as densely as possible along the slit.