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UNIVERSITY OF CALIFORNIA OBSERVATORIES/LICK OBSERVATORY
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SANTA CRUZ, CALIFORNIA 95064

February 02, 2007

Preston Burch
HST Program Manager

David Leckrone
Senior Project Scientist, HST

Matt Mountain
STScI Director

Re: ACS and SM4

Dear Preston, Dave and Matt:

Let me start with a short "executive summary": I hope that the HST Project gives serious consideration to repairing ACS in SM4, and does so through a careful assessment of the scientific return, the technical issues and the broader policy/public-value issues. ACS has capabilities that WFC3 does not, complements WFC3 scientifically, and would provide a key imaging backup capability once repaired. WFC3, COS and ACS together would be an astonishingly effective scientific capability until JWST is launched some 5 years later. I recognize that this will likely involve choosing between STIS or ACS for SM4, given EVA time limits, cost constraints and Project personnel constraints, particularly since SM4 is only 19 months away, and so I would hope that this is clearly and explicitly part of a very rapid and focused assessment process by the HST Project and STScI.

I would like to outline the thinking behind this summary.

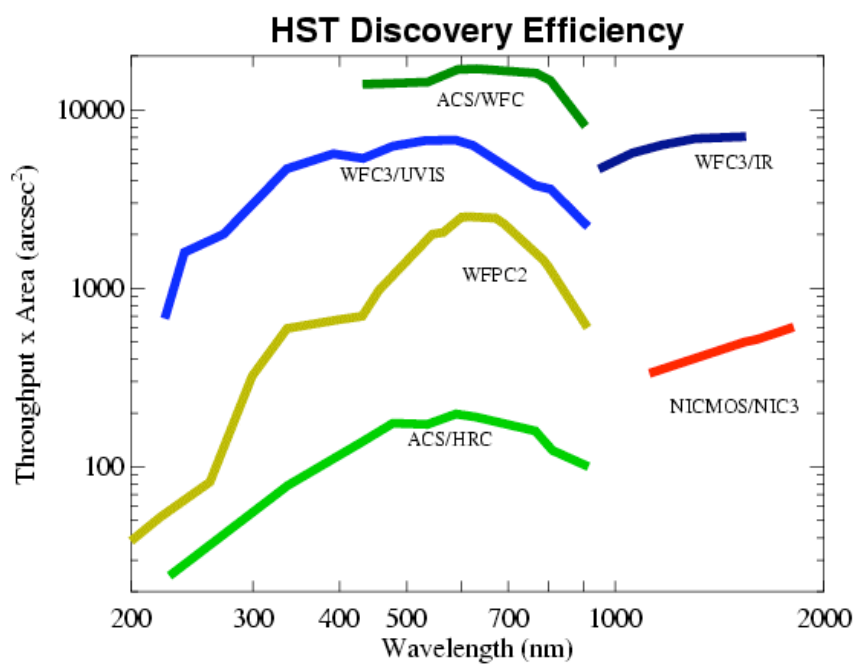
For some time I have been thinking about the capabilities that we will have in orbit over the next decade. Overall we are looking at rather lean times before JWST is launched. The announcement by Mike Griffin last fall regarding SM4 for HST was very welcome since that provides capability for significant continuing scientific return. In fact, HST becomes even more important given the limited capability of Spitzer post-2009 when its cryogen runs out, and given the uncertain lifetime of Chandra.

There are two aspects to the Great Observatories that have demonstrated their value to the nation - namely their science productivity and their role as the most visible public elements of the NASA science program (along with the aperiodic, but very important, visibility from missions like the outer planet missions and the Mars missions). Both science and engagement of the public have been served extremely well by HST. In fact, Hubble and its imagers are the heart of the public appreciation of NASA space science, both for their long-term science productivity and for their wide range of spectacular images. Ed Weiler appreciated this very early (even before the launch of HST) and strongly supported WFPC2 and also the subsequent cameras (ACS and WFC3) – as did the HST project at GSFC and at STScI.

The reaction to the recent dramatic, virtually-complete failure of ACS (SBC science is only a fraction of the total use) emphasized again the special role that the imagers play on HST, and also the broad policy issues they raise because of that special role. That special role is exacerbated since I suspect that the next ~5 years will continue to be very challenging for science at NASA. The budget issues for NASA with the transition from STS to a new launch capability are very substantial.

Imaging capability from Hubble during this period will be particularly important to demonstrate to Congress and the Administration again and again that science at NASA is important. WFC3 provides such capability, but the failure of ACS means that we now have no backup (like the 13-yr old WFPC2 is now providing) – unless ACS is repaired.

WFC3 will clearly be a very powerful instrument. Yet from the broader perspective I am concerned about it being our only imager, given the importance of imaging capabilities. If WFC3 has problems after SM4, the impact of having a much less capable Hubble with minimal or no imaging capability would be very substantial.



But it is not just the policy and public visibility aspect that is a concern.

There is a major scientific loss without ACS. ACS provides coronagraphic capability on the HRC that is uniquely powerful (the recent results on the disks in HD107146 and Fomalhaut, for example, exemplify the cutting-edge advances this coronagraphic has made). Given the increasing interest in star and planet formation a premature end to that capability would be a substantial loss. None of the other instruments (NICMOS or STIS) would provide capability as powerful as the ACS coronagraph.

Probably the biggest impact though is from the loss of the wide-field survey capability. ACS has been a huge success because of its *Discovery Efficiency* – and it would remain uniquely powerful even with WFC3, as the HST Project's chart indicates (the discovery efficiency is "throughput X area"). ***The Discovery Efficiency of ACS/WFC is 3-4 times higher than the WFC3/UVIS in the red part of the optical spectrum***, where much of the most high-profile science is done. This does not mean, at all, that WFC3 is inferior to ACS, since WFC3 will have many capabilities that ACS does not, but it illustrates the basic point that WFC3 cannot replace ACS. The two working together would bring unprecedented imaging power to Hubble and revolutionary advances on some of the biggest problems in astronomy.

As an example, for distant galaxy studies, it will be significantly more difficult to build on and extend the scientific studies that have developed from GOODS and the UDF with the demise of the ACS. The discovery efficiency of ACS *vs* WFC3 points directly to the challenge of building on surveys with WFC3 alone. At first glance it may appear that WFC3/UVIS is well matched to WFC3/IR. However, this fails to recognize that most astronomical objects are red and so obtaining good IR and optical images requires more investment in the visible than in the IR (this explains why Spitzer is so astonishingly effective at detecting high redshift galaxies, even though it is a tiny telescope). Doing the UDF with WFC3, for example, would require ~800-1200 orbits, depending on whether one matches the area or not. WFC3/IR studies will be limited primarily to the fields for which existing very deep optical ACS imaging has been taken.

This issue was brought home to some of us recently when we were trying to understand what we would need to do to build up a large sample of galaxies at z~7-8, the current frontier for high redshift galaxy studies. WFC3/IR provides an opportunity to break the logjam (only a handful of such sources are now known due to the small size of the NICMOS NIC3 field). But, without ACS, it will be very difficult to build up the deep optical fields with their very long integrations beyond what is already available (since ACS is ~4X as efficient for this as WFC3/UVIS). The key here is that large areas need to be covered to obtain statistically-significant samples.

A similar challenge arises when we try to establish the properties of the full range of galaxies at redshifts 1.5-4. This period, some 2-5 Gyr into the life of the Universe encompasses the time of the most active buildup of galaxies. If we really want to understand what is happening in this epoch we need extensive and deep data sets from the visible to the IR to both select and study the galaxies. WFC3/IR does this very efficiently, but it is a challenge to get the corresponding visible data that is necessary without the ACS. Both these projects require space-imaging capability. Neither are practical from ground-based data alone – even with 8-10 m class telescopes.

Finally, there is the very substantial value that will come from parallel operation of WFC3 and ACS. The science return from WFC3 imaging programs will be substantially enhanced from parallel operation of ACS. The very obvious WFC3 IR and UV mapping of GOODS and the UDF will require many hundreds of orbits. Having many hundreds of orbits of additional ACS data would provide an additional remarkable dataset for improving the statistical weight of observations of distant galaxies (large numbers of different types). Such deep visible fields (along with GOODS) would also become the baseline for JWST initial studies because of the huge investment from the current Great Observatories and large ground-based facilities that is being undertaken in these fields.

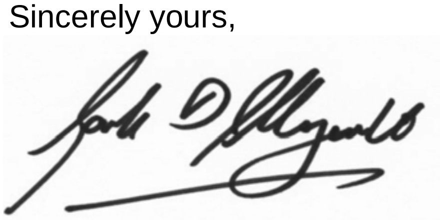
I would not like to leave the impression that WFC3 is inferior. It is not. WFC3 is an extremely capable instrument. It is just that for many forefront science problems WFC3's power is greatly enhanced and leveraged with ACS.

I would like to suggest that careful consideration be given to the possibility of repairing ACS during SM4, and to do so through a thorough assessment of the scientific return, the technical issues and the broader policy/public-value issues. I recognize that this will likely involve consideration of the choice between STIS or ACS for SM4, given EVA time, cost constraints and Project personnel constraints, and so I would hope that this is clearly and explicitly part of the assessment process. Because of the short timescale to SM4 (19 months) I would hope that the assessments be carried out in parallel as rapidly as is practical by the HST Project and STScI. The great power of COS *vis-a-vis* STIS, the performance of the now 10-year old STIS detectors, and the relative interest of the community as expressed through the last observing cycles when both STIS and imagers were operating, would also be useful input to the assessment process.

For the usual admission of other interest in this... Obviously I have an interest in ACS from my role as Deputy-PI of the instrument. But this email is not an effort to keep alive an old instrument for sentimental reasons! It relates to my deep concern that continuing HST imaging capability is essential for its broad science and public interest role during the challenging and relatively barren period we face over the next 5 or so years for astrophysics space science missions at NASA. The concern for the future grew from the major problems last year, warning that loss of ACS was likely, but were brought to a focus by the recent failure.

Let me wish you all the best with all the preparations for SM4. I am delighted that the HST project did its (major) part in being ready to support SM4 so that the Administrator could make his announcement to go ahead.

Sincerely yours,



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