TECHNICAL REVIEW #1

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Dear Dr. Vorosmarty:

I apologize for the last minute response, but this past few weeks has been very busy. I was tied up with a task that I had scheduled some time in the past. However, I found the paper very interesting because of its relation to other issues that we have been examining at NRC.

SUMMARY

The issues are well presented and the paper recognized the problems as well as the potential payoff. Each of the Scenarios has both science and spacecraft accommodation issues that need to be addressed more fully before a final choice can be made. Considering spacecraft design and accommodation issues only, Scenario A and C are similar and present no major issues for accommodation of the instrument. Orbit control to ± 125 m track repeat is an issue. Although I have not done any specific analysis of this spacecraft, previous work indicates that it is a matter of frequency of maneuvers. Higher altitudes (800-1000 km) would be better than 400 km to minimize the orbital disturbances. Scenarios B and D present more challenge in accommodating the payloads. The larger antennas, higher data rate, and higher power for Scenario D present more challenge. The ATI-SAR would potentially generate more spacecraft attitude disturbances that might affect the data. Packaging and deploying the larger antennas needs study. The overall plan for studies is good and the results will provide a good basis for a future decision.

INTRODUCTION

As you realize I am not involved in any aspects of hydrology, so I will not comment on the underlying science. I did think that the overall presentation in the report was very good. There was a good tie between the science strategy and the missions and it did not neglect the difficult topic of validating the satellite results. The presentation of the basics in the paper was very clear and I was able to follow the arguments that set the goals for the satellite systems. To set the background, I have been in the satellite business for 35 years and have had a long association with EOS and similar programs. When RCA/GE was working with Wyman Baker, I did some conceptual design for Windsat and LAWS and I am familiar with the work done on the laser for Baker by our group at GE.

Based on results from previous missions, the mission data requirements specified in the paper, put serious requirements on the spacecraft and its orbit. However, the approach presented would like to eliminate some of the tight orbit control and design features that applied to Topex-Poseidon. My overall impression is that these are going to be very difficult missions, especially within the cost limits that are set. I am never quite sure these days what is included in the $100M for a mission . How much of the cost of the studies would have to be charged to the mission? This is an ambitious program, which attempts to provide high accuracy from a series of space-based measurements while relaxing the requirements on the satellite. People spent years planning and then understanding the T-P measurements. I think that the paper recognizes this problem as reflected in the discussion of Section 5.

DISCUSSION

As a realty check, I listed some of the tasks. Perform a subset of the studies listed in Section 5; design or modify an instrument; build that instrument; carry out extensive prelaunch characterization and calibration of the instrument; perform a satellite design or at least modify a design; integrate and launch the satellite; perform in-orbit validation using in-situ measurements and some cross-correlation with other programs; and then operate the system for 3-6 years. Although the paper characterizes the instruments as available or requiring simple modifications, this is a claim that needs more substantiation. Perhaps some additional study as proposed in Section 5.

The data rates for the four options are all relatively high, even with selective targeting. Data system costs can be a significant factor in the overall mission costs. This would be an evolving system and the processing algorithms will improve over time. Therefore, all the data would have to be archived for at least a few years beyond the mission duration (archive for say 6-10 years).

The arguments vis a vis the surface in situ measurements vs. satellite measurements are well presented, and these arguments are also used by other communities. For example, the recent NRC report *Reconciling Observations of Global Temperature Change* indicates a loss of historical ground measurement sites and a lack of consistency in some of the ground-based measurements. While the satellite measurements have the advantage of being well sampled and perhaps more consistent over time, the data reduction for the satellite measurements is considerably more difficult and adds complexity in interpretation of the results. One trades complexity of measurement and analysis for the difficulty of maintaining the ground network. In the hydrology case, the issue of access to the data and the sensitivity in some countries and regions adds weight to the need for a satellite system.

The assessment of the need for a series of studies is well developed, and indicates that the group has recognized the inherent difficulties in these missions. Each of the mission scenarios has its own set of spacecraft problems. I assume that these are intended to be relatively small spacecraft. Launch and all other costs can escalate quickly as the size goes up. I personally have always been a proponent of matching the spacecraft size to the mission needs, but that does not work unless the budget can also be matched to the mission. In the case where the mission design must be adapted to the budget, it is necessary to pick the important goals and parameters and eliminate what cannot be accomplished within the budget. The need for a ± 125 m track repeat, the low (~ 400 km) orbit, and the large antenna are competing requirements. Before a serious satellite system proposal could be written, I think that a scrub of the requirements is needed.

Typically, the altimeter mission altitudes are high for several reasons, but orbit stability is an important factor. The use of GPS will help in defining the orbit for post-pass processing, but it does not ensure that the ground track is repeatable. It is too early for any detailed analysis, but the work done for EOS and Landsat a few years ago indicated that tight crossing specifications could require frequent orbit adjusts, especially during the solar max. This translates into more propulsion and increases the operations load. I know that the JPL assessment in Appendix A states that this is achievable, but it does not indicate the methods or the impact on the spacecraft.

SCENARIO A

This is the most reasonable from the spacecraft viewpoint. It has a higher orbit, small antennas, and relatively low power requirements. The data rates are still high and onboard storage and downlink rate for data dumps would drive the design, but it is well within the current art. From the description in the paper, it requires a fair amount of ancillary study and validation.

SCENARIO B

This is more difficult. The low altitude and larger antenna make it difficult to meet the ± 125 m track repeat. I have been working on a Ka-band communication satellite project these past weeks and the higher power (> 1-2 W) solid state amplifier is still not readily available. Rain fade is definitely a question and the paper indicates the potential for decorrelation due to foliage plus other potential for interference with the returns. Large parabolic antennas are being developed for geosynchronous cellular telephone systems (Lockheed Martin ACeS or Garuda system and Hughes also has a system). Harris and others are actually developing the antennas, but these are packaged for the large launch vehicles like Ariane 5 and Atlas IIAS. This would be a serious difficult for the smaller launch vehicles. Even a linear array for a SAR would be difficult to fold into a small package. The design details for a Ka-band system presented in Table B of Appendix A indicate a somewhat marginal SNR when one considers rain and some other potential losses. The data rates are high and downlink rate and downlink time would constrain the targeting. A single target every orbit would result in 2-3 Gb/day at Ku-band and 5-7 at Ka-band. This volume is not inconsequential for a science project and the GSFC study in progress for NPP (NPOESS Preparatory Project) has predicted higher costs for a similar data volume (and downlink rate) than one would desire for these ESE post-2002 missions.

SCENARIO C

This spacecraft design is of intermediate difficulty. Similar concepts were studied many times in the past and the laser system has evolved to a point where the satellite accommodation of the instrument is a reasonable problem. A 25-cm scanner is similar to the SSMIS on the Defense Meteorological Satellite System spacecraft. Depending on the repeat cycle, coverage, and repeat track requirements an orbit altitude on the high side of the range would provide fewer disturbances. However, there are many questions to answer concerning the science and the ability to measure velocity. My recent informal discussion with Dave Emmitt, while it added much information and clarified certain parts of the paper essentially did not change my view that the science is the difficult aspect of Scenario C.

SCENARIO D

This is another difficult accommodation problem. The large antenna has to be packaged and deployed after launch. An analysis of the attitude disturbances created by a large, flexible antenna is needed to assess the impact on the science data return. This payload requires higher power, both the power for the SAR and the high-rate data system so the solar array will be larger. This just exacerbates the orbit disturbance. The high data rate and large data volume also drive up the difficulty.

TECHNICAL REVIEW #2

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Comments on Hydra-Sat:

I have a basic problem. I do not understand the purpose of the paper. You do not appear to be basking NASA to do anything. It appears to be a justification for the Hydra-Sat mission science and this it does very well, but, it does not exactly say what steps NASA should take. It has been my experience if you want NASA to do something, then you have to bluntly say what that is. You talk about a flight in 2005 but give no details of what is to be done to get there. You estimate cost without any indication what the basis for the numbers are. You give a number of scenarios but no management, schedule or funding profile needed to support a study to make a decision. Is this a talking paper to get the ball rolling? Is it a request for guidance? NASA may already have a funding number in mind which would limit the options.

I found the paper very interesting and enjoyed reading it but it is not exactly something I feel confident in making comments since there is very little technical or management information. I do think however that the comparison of the Amazon river data on page 12 is significant enough that some statement should be made in the text and not just under the chart. I believe with the increase ability to carry large in-orbit memories and a good computer, one should plan for the spacecraft doing as much data preparation as possible to aid rapid ground operation. The only nit pick is that on page 8 paragraph 2 sentence 3 Interferometric appears to be missing something.

Hope this is of some help.

TECHNICAL REVIEW #3

Overview

There is little doubt that global hydrological data is in short supply, and that the problem is becoming more severe as the *in situ* measurement infrastructure continues to deteriorate. Thus I agree in general with the high-level motivation behind this initiative. However, the technical response as represented in the draft sent to be for review is weak. I informally rated all four scenarios, based on their technical content and likelihood of success against their objectives. All four rated one on a ten-point scale. Frankly, I find the technical concepts self-contradictory, poorly thought through, and perilously close to dishonest as represented in their respective technical specifications. In several passages these shortcomings are acknowledged, leading to claimed needs for further study. The paper appears to be a hunting expedition: give us support and we will begin to think.

Aside

The paper s title bears an unfortunate similarity to the name of a mission proposal submitted to NASA for the 1998 ESSP opportunity. That proposal, which relied on L-band radiometry, was aimed at the global soil moisture problem. HYDRA-SAT as you propose is aimed at quasi-open water bodies, observed using active systems in the short microwaves or optical (lidar) domains. You would be well advised to choose a more unique and descriptive name for your initiative.

General criticisms

The coverage scenario is poorly defended. The main requirement seems to be that the overpass repeat should be 3 to 7 days. Unfortunately, for any satellite in low-Earth orbit, this requirement carries with it a corollary: more frequent temporal repeat implies less dense spatial coverage. Thus, a 3-day repeat implies track separations at the equator on the order of 1000 km, and about 500 km if the repeat period is 7 days. Given this sparse spatial coverage, how is a global mission to be designed? The draft proposal does not even raise the issue, let alone offer a rationale for trade-off considerations. (There is an example of potential dishonesty here. Figure 4 purports to show coverage with respect to flow gauge stations. Unfortunately, the example is taken from ERS-1 coverage, which appears to me to be from its 35-day repeat orbit, thus offering a five- to ten-fold spatial coverage advantage over the HYDRA-SAT baseline of 3-7 days. There is no scale included in the figure, so reader beware .)

The resolution claims are misleading. Assume for the moment that the effective resolution cell is on the order of 100 m along-track, and 1 km across-track. (Please see more detailed comments on these numbers below.) Thus, the aspect ratio of the altimeter s footprint is 10:1. Such a footprint can achieve its intended resolution only for those linear features that are aligned within 10% or so of the axis of the sampling footprint. Stated more concretely, the supposed benefits of high resolution will be enjoyed only for stretches of a water boundary or river that have quasi-linear extent of more than one kilometer, and which happen to have linear features that are close to orthogonal to the satellite s surface path. There is no analysis presented to show that such a sampling constraint might provide useful data over the Earth s water bodies, as observed within the orbital constraints of the nominal mission.

The river models are naive. In many of the world s rivers, especially those subject to seasonal flooding, the concept of river bank has little meaning. Consider the Amazon or the Pantenal of Brazil. During the wet season, in those regions the mean water level rises 10 meters and more, spreading into the forests or flood planes many 10's of kilometers. Trees, houses, plantations, and highlands protrude. Where are the river banks to be found? The same problem occurs in the United States, as illustrated by the Mississippi and Red River floods of recent years. Not all rivers are as well constrained as the Rhine through central Germany, and even that river can spread over its banks for many kilometers below Bonn as it falls to the Netherlands.

Given these situations, the optimum sensor would be an imaging radar, as has been proven time and time again. Where then is the defense of the proposed instrumentation against that capability? The level of any constrained body of water can be well estimated by its areal extent. DTM s can be developed for flood planes that are accurate to less than one meter. Given that the proposal is focused on water bodies, it cannot make a credible case without a convincing comparison to the capabilities and limitations of currently operating or planned imaging radar systems.

Finally, the cost of the proposed systems (my estimates) ranges from \$150M (scenario A) to \$600M (scenario D). If these sums were to be invested in, say 1000 *in situ* gauges, then there would be available \$150K to \$600K per measurement station. These are generous budgets indeed! These stations could be installed optimally, and monitored continuously. In my view, the cost-effectiveness of the ground-based approach far outweighs that of the proposed approach. The proposal lacks credibility without a persuasive argument against such alternatives.

Specific comments

Scenario A: Ku- and C-band radar altimeters. Many problems! Radiometry over land does not yield sufficient accuracy for water vapor corrections (partially acknowledged in the draft). Storage and down-link of raw data for subsequent Doppler analysis is mentioned frequently, yet the implied data rates and data volumes which are considerable are never even mentioned let alone estimated. It is alleged that simple platform/software/hardware modifications would allow the claimed enhancements, yet there is no solid discussion of those modifications. In my experience, they are not simple . For example, it is not feasible to modify the range and range-rate tracking loop of (conventional) altimeters (credit John MacArthur) so that landform variations can be accommodated. Consider the Niagara Escarpment, the Great Rift Valley, or the Mosel; none of these can be tolerated by a tracking window of 100 meters depth, no matter how fast its response might be set. The ERS-1 land gate (credit Rapley) sacrificed resolution for speed and tracking window size, and so it goes. Tracking requires averaging, which places a limit on response time. The Technical Specifications (Table 1) are incomplete and mutually inconsistent. The combination of PRF and (along-track) antenna size are insufficient to allow coherent (Doppler-sensitive) data collection. Range resolution of 0.5 m, if it is to support vertical height accuracy of 5 cm, requires a minimum of 100 statistically independent samples, yet that is impossible for the cited along-track spatial resolution of 1-5 m. There is no error budget to support the 5 cm accuracy claim. The cited 10 kb/s telemetry data rate is grossly insufficient to support raw data for subsequent Doppler analysis. Doppler beam sharpening by definition discards information for the sake of resolution. The name of the game in precision radar altimetry is degrees of freedom (number of statistically independent samples), not resolution. Doppler beam sharpening, at least in the conventional sense, drastically reduces the degrees of freedom available for subsequent signal processing. If the conventional approach is intended, it will not work. If a more capable method is intended, it is neither described nor justified. Averaging is required: where does it exact its cost? Loss of spatial resolution along track? If so, say so, and quantify the trade-off. If not, then please fill in the missing physics! The second bullet on page 21 is total malarky without considerable expansion.

Scenario B: Beam-limited cross-track Doppler altimeter. There are key differences between this scheme and that of Scenario A. Given one frequency, the possibility for ionospheric (electron density) delay correction is not possible. Hence, the interest is in water height relative to adjacent land references. This relative measure is reasonable in principle, but it faces challenges in practice. There is no convincing argument in the proposal that is sufficient to establish the land reference accuracy to better than a few centimeters. To do so would require an analysis of the effect of cross-track orbit errors (as a function of terrain type), and so forth. It is not sufficient to say simply that a number of scoping studies are....required. There are problems with the system technical specifications that parallel those enumerated above. In addition, there are other problems. The second paragraph of the scenario opens with the phrase The hardware would consist of a standard deployable antenna system... and closes with the phrase ... Such antennas are technology [sic] feasible, but have not yet been demonstrated in a spaceborne mission. Right! I for one would assign very high risk to that element of the mission. The use of open-loop tracking betrays a deep misunderstanding of radar altimeter systems. For example, the on-board closed-loop tracker for the TOPEX radar altimeter maintained the dynamic range word to a precision of less than one millimeter. An error analysis of the end-to-end measurement budget would support the necessity for such internal precision. The proposal does not pretend to offer a comparable analysis in defense of its open loop concept. Of course, one could claim that such a scheme worked for the Magellan radar and radar altimeter, but then the tolerances were measured in many tens of meters, not centimeters. The scenario depends on beam-limited performance in the cross-track dimension, but there is no error analysis to support this approach, let alone to justify the size of the antenna aperture selected. Beam-limited operation has its advantages, but its disadvantages are the potentially costly aspect. What is the impact on satellite size? Attitude control and knowledge? Aperture efficiency? The rather casual suggestion of Ka-band suggests a lack of appreciation of the impact of wet tropical atmospheres on propagation loss at that frequency. Presumably, tropical water bodies are of interest, in which case, if Ka-band is to be entertained seriously, then their coverage (and coverage loss) has to be well defended. I found no such discussion. Again, give us the money and we will do our homework . Sorry, not acceptable. There is a fatal misunderstanding of the effect of full de-ramp demodulation. The instantaneous bandwidth is reduced, but the total data volume is not reduced. Range resolution is still proportional to one over the signal bandwidth. Thus, if a de-ramped signal is to be downlinked, the same number of bits are required per pulse return, regardless the demodulation scheme. The original discussion of full de-ramp denotes the process stretch (credit Caputi), which conveys the main idea. Either way, there is no mention in Table 2 of the required data downlink capacity. It will be substantial. Do the numbers. Using 7kHz PRF, 400 Mhz bandwidth, 50 microsecond pulse length, and 4 bits (I&Q) per sample, I get something just under 100 Mbits/sec. Not trivial! Ought to be mentioned.

Scenario C: Lidar, conical scan. I found this suggestion to challenge my credulity. What evidence is there that a quasi-specular surface, such as an inland water body, can yield a sensible return from an oblique laser view? If bistatic, I could swallow the suggestion; witness solar glint studies of standing water, especially in broken terrain and under forest canopies. Backscatter from water at 30-45 degrees off nadir? From glass-flat Amazonian flood waters under the canopy? Requires convincing evidence! The next step is even more challenging: surface velocity measurement. The claim is that the velocity resolution is better than 10 cm/s (Table 3). Now this is to be accomplished from a platform moving on the order of 7 km/s. Even if there is sufficient signal, how is that to be done? There are major implications on the attitude control and knowledge of the scanning lidar beam. At broadside, 10 cm/s corresponds to an angular error (in knowledge) of about 0.1 mrad, which is not realizable. One might posit land reflections to offer a reference, but Doppler noise will arise from wind over vegetation, among other sources. What is the error budget? What confidence can one expect from this scenario?

Scenario D: Along-track SAR interferometry. Yes, this technique works, and has been demonstrated from aircraft over expended water bodies such as open ocean and coastal currents. It is a major leap, however, to extend this to a space-borne system, meant to observe the surface current in a river. Again, the case is not made sufficiently. The draft identifies several of the potential limitations, citing the need for further study. Mmmmm. No convincing. The problem is that the resulting system is relatively large and complex (and expensive), having capabilities that go far beyond the needs of surface hydrology. If such a system were to be funded for some reason (such as Discoverer 2), then this would be a potential secondary application. The proposal makes no attempt to demonstrate that the cost of such a system might be justified by the benefits to be gained from the hydrological data that might be produced.

Ancillary comments

The section on Mission Readiness (page 50) is in my review professionally irresponsible. It is a gross misrepresentation to suggest that Scenario A can be based on ... the T/P mission altimeter with modifications of onboard data storage to accommodate the Doppler processing technique. It is not true that in all cases the instruments recommended are based on proven technology as claimed. Indeed, it is for this very reason that NASA is supporting development of the first multi-look Doppler-processing radar altimeter through an Instrument Incubator Project (the theory and technology for which the draft proposal fails to cite appropriate references).

SCIENCE REVIEW

I had a chance to read your Hydra-Sat prospectus this weekend.

Needless to say, I found the satellite justification for the data requested to be correct and significant. However, the proposed target sizes would exclude many important environments, i.e., many small bodies can make up to a large area.

The modification of radar altimetry systems to river level analysis is worthwhile, but needs more work than is obvious from the prospectus. Also, it is important to have imagery to complement the altimetry, at least for wetlands and some lakes. It is not clear what system will provide the necessary L-band SAR data.

The instrument proposed for surface current measurements seems high risk. I am not convinced that 50% access for LIDAR is correct. It seems that scenario D requires the correct orientation of the sensor relative to the river to obtain surface currents. Also, I would like to see a demonstration that river discharge can actually be determined from such measurements with an incorporation of the uncertainties inherent in the measurements.