Solar System Exploration Decadal Survey: Mars Panel
Tempe, AZ
Sept 9-11

Serina Diniega, University of Arizona

THE FOLLOWING ARE OPEN SESSIONS

Overview

The open sessions at this first Mars Panel meeting focused on:

(1) An introduction to the Decadal Survey Process and the past Mars Exploration Program,
(2) Lessons learned from the last Decadal Survey (2003) and past missions,
(3) The three centers that the Survey Committee can contract for mission studies (JPL, APL, GSFC),
(4) Mars missions currently in development (MSL, TGM, ExoMars, MAX-C),
(5) Possible components, implications, and challenges of a sample return mission.

- Anything preceded by a bulletpoint (not enumerated) was based on discussion.

Day 1 – Wed, Sept 9

Decadal Survey Process and Mars Program Introduction

Doug McCuistion

The aim of the Decadal Survey is to create a fully-integrated view of solar system exploration, and to set science priorities, not expectations, whose associated missions fit within the known Planetary Science budget.

The way that the proposed Mars Exploration Program (MEP) fit into the previous Decadal Survey (2003) identified major themes, as well as strategic core missions and PI-led/competed missions. The major Mars themes identified in the last Decadal Survey:

(1) Mars Sample Return (major progress; MSL),
(2) Mars Aeronomy (major progress; MAVEN, Trace Gas Orbiter),
(3) Habitability/"Mars as an abode of life" (major progress; MSL, ESA's ExoMars),
(4) Mars Network (little progress).

Gathering community input and investing in research and E/PO programs have also been identified as important facets of the program:

(1) Research: Mars Data Analysis Program (MDAP), Mars Fundamental Research Program (MFRP) = $20million/yr;
(2) E/PO programs have been very successful at engaging the public interest.

The Mars budget has been dropping over the last years (for example, down by 50% in 08 from 05). It has partially recovered, but forecasted slow growth within the next decade only matches inflation. This Panel should not expect the budget to grow too much, as the national focus is on other areas, like launch vehicles.

However, the expectations for good science have not dropped. This means that there is less money for technology development outside of what's necessary for mission development. There should also be more effort to utilize outside technologies, such as the Advanced Stirling Radioisotope Generator (ASRG) and the deep space optical communication initiative.

Mission concept studies can be commissioned by this Panel to better define possible mission parameters and architecture, which will be needed for cost estimates. These studies will be done by key centers assigned by NASA, and will provide analysis and supporting documentation to the Decadal Steering Committee and the NRC-contracted independent cost reviewer. Lisa May (Lisa.May@nasa.gov) is the POC for the Mars Panel and will facilitate and organize mission concept studies.

There are ongoing efforts to merge NASA and ESA MEP into single initiative – this Panel should consider a possible Joint Program as an implementation (not scientific) approach (starting in 2016, and possibly impacting Trace Gas Orbiter, Sample Return, etc.).

Discussion during and after the presentation:

- How do we cost/include international missions? A: If cost is estimated, this becomes "cost not required by NASA" due to international cooperation. However, the Panel should try to keep out of this as much as possible, as we are tasked with developing a program that fits in the NASA budget, and it is up to NASA HQ and such to weigh international cooperation, possibly make the budget envelope larger. Our first task is to define scientific priorities – based on this we can identify which missions are important and affordable under NASA budget. Perhaps we then can also identify missions possible with international cooperation.
- Should we consider possible collaborations beyond NASA-ESA, with JAXA, etc.? A: The ongoing discussions concern the merging of MEP in the largest 2 space agencies. Anyone else can get involved, and opportunities will be offered, but the leaders will be ESA and NASA, and they will set up architecture, leadership, and program structure.
- How can we effectively work on a plan for 2013-2023 given the long time needed to develop missions, and the fact that there are already developing missions in 2016 and 2018? A: The decadal and 5yr cycles are too short for long mission timescales. At least the mission planned for 2016 should be fairly locked in by the time the report comes out, but perhaps some adjustments can be made based on current discussion. This Panel can also provide an interim report on that mission, if NASA would want that, in addition to the MEPAG assessment previously done.

<u>Mars Panel Mission Studies POC</u>
Lisa May
All POCs for the different Panels will be working together to make sure that uniform inputs and outputs are acquired for the studies, better enabling the Steering Committee to look at a consistent, integrated picture.

The POC will receive study requests from the Panel, then will assign the study to a key center (considering any Panel suggestions). The POC's role is to manage the study process and Center efforts, not to participate in the definition or outcome of the study – it is up to the Panel to provide the science focus.

<u>JPL: Mission Studies Center 1</u>
Kim Reh/Chet Borden/Keith Warfield (Jet Propulsion Laboratory)
Three types of studies are available for examining possible mission architectures and related cost:

(1) Rapid Mission Architecture (RMA, CML 3):
   a. An RMA study aims is to find good balance between science return, cost, and risk while exploring a broad trade space driven by science objectives. This study rapidly analyzes preliminary feasibility of multiple architecture options, including innovative paths, which are then prioritized.
   b. A small team (6-10 people) is involved in 5-8 concurrent sessions over 2-3 weeks. The report is written over the next two weeks.
   c. Inputs needed for this study are: science objectives, desired measurements, and preliminary information about prioritization and instruments (system level cost estimates, mass/power/longevity requirements, etc.). The study will output: mission candidates for future study, documentation of the basis and rationale for assessments, preliminary cost class (e.g., Flagship, Discovery, etc.).

(2) Team X (CML 4):
   a. This study yields rapid design of space mission concepts, using many study products (such as trajectory analysis/visualization, risk analysis charts) to create mass/power requirements, and cost/risk estimates.
   b. Team members (~20 engineers) are discipline experts with significant flight project experience, pulled out for three sessions held over a week.
   c. The outputs of this study generally consist of ~200 viewgraphs, a bulletized write-up, and a 30p text summary. The end estimates from this study can be used as inputs to an independent cost estimate.

(3) In-depth studies (CML 5):
   a. This study provides a comprehensive understanding of mission/specific element(s), and provides insight for high science impact/high technical risk
   b. Teams involved include technical/management/cost experts as needed, and studies last 2-4 months.
   c. The study output can include: science/requirement traceability, technology detailed assessment, and a refined cost/risk estimate.

The concept maturity levels (CML) reflect a way to estimate mission plan uncertainty (in technology, risk, cost, etc.) to allow comparisons between mission plans. Due to the inverse relationship between formulation phase/total mission cost (%) and mission cost overrun (%) observed in past missions, it only makes sense to compare missions at comparable CML level. This system was conceived by Mark Adler, and was inspired by the technology readiness level (TRL) scale: CML 1 = "cocktail napkin" sketch and CML 8 = preliminary design review (PDR).

We estimate that CML 4+ is needed for inclusion in the Mars Panel report. For more complex or expensive missions, a more in-depth study may be needed to achieve the same CML, especially as the absolute cost overrun matters more.

Discussion during and after the presentation:

- Has the development of the MAX-C or TGM missions been placed on this scale? A: We have done Team X studies for each.
- Each panel will get ~2 in-depth studies. Three other panels have elected to start with RMA studies as these are faster, can cover a lot of territory, and really help focus more expensive Team X studies.
- How can we use existing MEP studies? A: This depends on the study. We can ask a center to redo a study or expand upon it.
- How do you compare the CML for missions with different structures? A: There are key differences between, for example, orbiter and sample return, as landed/larger missions require a higher fidelity level due to their higher complexity. It is better to compare missions with similar components/size, and larger missions can be compared component-wise.
- Of past studies done at JPL, ~10% go beyond Team X, and ~10% of those get built. It is difficult to trace cost overrun on those missions, as generally many design components change.
- If the Panel is interested in a cost sensitivity to requirements study, then this should be done with an RMA; Team X deals more with a point design.

<u>APL: Mission Studies Center 2, by Robert Gold</u>
(Applied Physics Laboratory, John Hopkins University)
Mission studies can be done at a few levels and provide:

(1) Basic Trajectory Analysis/Mission Feasibility ($10s K).
(2) Preliminary Concept Evaluation ($50-100 K):
    a. These studies are similar to RMA, as the aim is to evaluate trade space to find at least one good point design with prelim estimates.
    b. The study is run in APL's Concurrent Engineering (ACE) Lab, which is similar in layout and design to the room used by Team X at JPL. APL provides a provide study manager and science and engineering support, consisting of discipline engineers/scientists who are concurrently working on existing flight programs – allowing the study team to interface with large skill pool for questions that arise. A Preliminary Concept Evaluation typically lasts for 3 weeks: one week for gathering background

information, one week of half-day meetings, and another week for writing the report.

c. To begin the study, the team will need science/mission requirements. The study output will consist of a study report outlining science rationale and objectives, trajectory design, mission architecture/operations concept, vehicle concept design/implementation, payload definition, system performance/margins, technology assessment (maturity, availability), risks, basic development and flight schedule, and inputs for cost modeling efforts.

(3) Full Concept Design ($100s K).

The Decadal Survey will probably need several Preliminary Concept Evaluations, and a few Full Concept Designs.

Discussion following the presentation:

- Would APL feel comfortable studying something like Mars EDL? A: JPL has more experience in that area, so we would probably collaborate with them. Also, APL is smaller in size (than JPL), so would collaborate for missions requiring larger studies.

GSFC: Mission Studies Center 3

Michael J. Amato, via conference call (Goddard Space Flight Center)

GSFC studies can be done at a few levels:

(1) Rapid basic trades and approaches, which takes 2-3 weeks (like RMA).
(2) Basic mission study, which takes 8-10 weeks (like Team X).
(3) Longer, more involved studies (like JPL in-depth studies), such as instrument studies.

a. All studies do cost estimations (two methods: Price H and grassroots), risk estimations, Mass/power, instrument/mission concept architecture, trade studies, and technology assessments – the differences are in the amount of detail and cost uncertainty. GFSC has experience with all maturity levels, and has worked with cost-capped missions (ex. Discovery).

b. All studies are done in an Integrated Design Center, which contains dedicated Mission Design Lab and Instrument Design Lab (similar to the Team X room). The team is made up of dedicated scientist/engineers for the study duration.

c. Inputs required are the target, science goals, etc. The study will output a baseline mission design, trajectory and flight information, alternative designs/trade studies, estimates of mass/power/data rate, risk and cost estimation, and mission recommendations.

With the dedicated center, GSFC can do two of the shorter-medium studies concurrently. The center will partner as needed, and feels it can most help studies where it is the lead in instrument expertise or capability.

Discussion after presentation:

- How are we going to deal with changing parameters while getting missions costed? A: This will depend on the maturity of the proposed mission idea – if the mission idea is from a white paper vs. an idea that has already been studied. A completely new idea can be put in front of RMA, then Team X, etc., while a more mature idea may require less additional study to bring it up to the right maturity level. Additionally, we will have to consider our budget/time, so need to identify a few missions we want studied as soon as possible.
- Who makes the decision about studies? A: This will be decided by the Steering Committee, to make sure there is fair and balanced use of talent available. No more than 15 full studies can be done in the full Decadal Survey. We should try to utilize pre-existing studies. For example, since 2016 and 2018 are well-studied, we will mostly make interim reports. New proposals that require studies start ~2020.

<u>Decadal Survey Overview</u>

Larry Soderblom

This Decadal Survey will run over 2009-2011, and focuses on a science-driven plan for solar system exploration during 2013-2023. The Steering Committee consists of the chair (Steve Squyres), vice chair (Larry Soderblom), vice chairs of the five panels and nine others. The Panels are:

(1) Inner Planets (Ellen Stofan, Steve Mackwell),
(2) Primitive Bodies (Joe Veverka, Harry McSween),
(3) Mars (Phil Christensen, Wendy Calvin),
(4) Outer Planet Satellites (John Spencer, Dave Stevenson),
(5) Outer planets (Heidi Hammel, Amy Simon-Miller).

Astrobiology is distributed through 5 panels, at that community's request.

This Decadal Survey is really trying to get community input, through white papers (due Sept 15), town hall and open meetings, and conference outreach sessions (e.x. EPSC, LPSC). The entire process is aimed at articulating a program for the coming decade that is informed by the Planetary Science community's views. The end reports need to aim for true consensus, be inclusive and transparent, and have emphasis on cost realism.

By end of the second Steering Committee meeting (end of Nov.), mission studies should be well underway for all Panels. The evaluation of candidate missions by Panels should place great emphasis on evaluation of technical maturity and probable costs. The overall objective is to produce a realistic set of candidate missions for NASA to carry out in the coming decade. Technical support for mission studies will be by GSFC, JPL, APL; all cost estimates will be done by NRC-contracted independent group (Aerospace Corp.).

Discussion during and after the presentation:

- Mars exploration has a fairly well-set mission architecture through 2018. The Panel needs to discuss and endorse these missions at same level as anything else examined by the Steering Committee, but most of the need for suggesting science and architecture is for 2020+.

- It is important to recognize the realism achieved by these mission studies – it's not realistic to put in $1million for an $8billion project and expect a perfect cost estimate. The aim is to improve all cost estimates up to a consistent, reasonable level.
- Lessons can also be learned from MSL to prevent similar overruns: a lot of mission components (launch, etc.) were not included in numbers, and the mission overall wasn't well-defined or evaluated at the last Decadal Survey.
- How is this process not going to be too prescriptive on what will fly and preventing the use of new technology? A: It's been done before, and there's a lot of information about past Mars mission payloads. There's no money available for payload development, so you can't let a payload drive the mission.
- How should we include (or not) the role of PI-led or internationally led missions? A: Our best strategy is to recommend a strategic and highest-priority sequence of missions to explore Mars, and leave it to NASA to specify implementation. We can recommend a few high priority science topics or mission proposals for New Frontiers, but we need to make sure the primary science objectives and proposed mission architecture are not dependent upon a competed mission which may not be selected (or upon international collaboration).
- We need to consider how to incorporate science priorities/missions not included in the direct path (for example, sample return vs. Network). If we were to return a sample, we also should think about what would be needed to know how to interpret/understand the sample. This may lead to other important missions that are not obviously within the direct path. We are not asked to explicitly name or prioritize Discovery or Scout-class missions.
- We have a chance to think anew about sample return. Perhaps break it into pieces, which would make it easier for international collaboration to take parts of it. If we can start towards sample return and accomplish a part in this decade, then that will be a good success.

<u>Mars Science Laboratory (MSL) Status Report</u>
Fuk K. Li
MSL is a rover that will carry 10 instrument packages to explore and assess potential habitat for past/present life. Its launch has been delayed to 2011; at this point many instruments have been delivered and there has been no erosion of the science payload. Current plans include: FY09 -- Risk reduction/design completion, FY10 -- complete hardware builds and prepare system programs, FY11-- complete tests and launch, FY12 -- cruise and landing, and FY13/14 -- surface operation.

This mission is expected to include a lot of feed-forward technology, such as the EDL communication system (which will be used in the next decade+), the ability to land a metric ton with precision landing (which will increase access to high-priority science targets), the rover's long-distance traverse travel capability, and flexible and robust sample acquisition and processing.

Discussion during and after presentation:

- Can we increase by factor of two in landing mass? A: No. The technology (Sky Crane) is near its limit, although maximal landing mass depends on landing details (such as latitude, etc.).
- From the MSL experience, what advice do you have about creating cost estimates? A:
    1. (1) Try to minimize number of technical miracles needed for mission.
    2. (2) Use analogy when possible (MRO, MSL, etc.).
    3. (3) When you count, count everything (launch vehicle, technology development, payload) so result is comprehensive for what mission is supposed to do.
- Based on the MSL experience, what type of technology development (caching rover, etc.) is considered reasonable? A: MSL spent $80M on technology development, out of $2B total mission cost. Some advice:
    1. (1) We need to do early studies defining the mission concepts and necessary new technology, and be sure to not be overly ambitious (especially about the number of technology miracles needed). Technology readiness of portions of MSL was not correctly assessed, and there was a collective underestimate of scope and complexity of mission.
    2. (2) We also need to learn to better discern that certain technologies won't make it, and need a backup to still do mission with scientific value/achieving goals. For example, MSL spent far too long developing non-successful actuator designs.
    3. (3) The slowness and overruns of MSL are not due to any single problem or a science-engineering disconnect. Instead, we extrapolated too far in the technology required. Note that the Level 1 science requirements never changed.

<u>Mars Science Goals</u>
Jeffrey R. Johnson
Since 2001, the Mars Exploration Program Analysis Group (MEPAG) has compiled and updated a report identifying top science goals (4), objectives (10), investigations (58), and measurements. The four highest-priority science goals are: life, climate processes and history, evolution of surface and geology, and preparation for human exploration. This document aims to identify current compelling science to guide program implementation decisions, and aims to generate and document community consensus.

Discussion during and after the presentation:

- What's your sense of involvement of the community at large in forming these goals? A: Based on MEPAG meetings, there are ~400 full-time Mars scientists in world, ~100 of whom attend. However, many fewer are commenting on the report contents (~35 comments on posted report draft). We believe this to be due to an acceptance of the process, as we do have pretty lively debate among the committee members before compiling the report draft. Few people say "what were you thinking?" (Some do come up, and we welcome them onto the committee.)

- Has anyone taken these investigations and compared them to missions planned in past and current sweep? A: On a spreadsheet listing 76 investigations vs. missions, each entry has some match. There are few holes, and identification of such can be used to define future mission architecture.

Day 2 – Thurs, Sept 10

Lessons Learned from the 2003 Decadal Survey

Jack Mustard

When the last Decadal Survey was written, Mars Global Surveyor (MGS) was in operation, the Odyssey mission and MEPAG had just started, and NASA was recovering from twin spacecraft failures. Although the Survey report had a comprehensive review of Mars Science at the time, the Survey Committee operated as a traditional Space Studies Board committee and did not look for broad community input and consensus. There was little constraint on costing and there was no robust/thorough examination of key technologies. Sample return was strongly emphasized, but it was not connected to other priorities – the exploration of Mars was seen as a collection of missions, not a program. MSL was poorly received at the time, as science goals were not well-defined or communicated.

Discussion during and after the presentation:

- Why were the Mars missions pulled out from rest of solar system (in organization and funding)? A: A separate Mars program did exist at that time. However, it was ill-defined as there was a disconnect between what NASA and scientists were thinking and envisioning.
- Historical context greatly affected how the committee thought about the Mars program, and the recommendations were more of a reaction than forward thinking.
- In 2001, we had a sense that sample return was the only way to get useful information about Mars. Now we know better about in-situ measurements, and how hard it is to get to samples. Can do more intelligent selection now, and can fit it into larger picture, but it needs to be well-defined and articulated. Additionally, the Astrobiological community has become more excited about sample return.
- In 2001, the main debate was between rover and deep drill missions.

Overview of Mars Science Status

Jack Mustard

This information was previously presented at MEPAG meeting a month ago, and was well-received. Discussion afterwards was then added in, so this presentation represents the community effort. In recent years, missions and studies have shown us that:

(1) Mars has diverse mineralogy, including alteration by water;
(2) A wet climate has occurred in the past, as supported by in-situ confirmation and a global distribution of water ice;

(3) We don’t fully understand atmosphere/volatile current conditions and past evolution, including the surprising detection of methane;
(4) Dynamic surface and climate evolution has occurred in the past and is occurring today;
(5) Orbital data is limited, and surface measurements done in-situ with sophisticated instruments are necessary.

Mars has proven to be an even more compelling target for exploration in the coming decade offering crucial info about early evolution of terrestrial planets/habitable planet, origin/evolution of life, long and short-term climate change, interior planetary structure and origin (especially in comparative studies with the Earth and Moon), and possible human exploration. Top questions deal with the:

(1) Diversity of aqueous environments (requiring detailed mineralogy and absolute ages),
(2) Trace gases in atmosphere,
(3) Interaction between the planet and space environment,
(4) Changes in climate,
(5) Interior structure and activity.

Discussion during and after the presentation:

- Many results presented came from orbit, how do you see the balance between orbital and surface data? A: We’ve seen that sometimes there’s a strong disconnect between what’s seen from orbit and on ground. We need to acquire definitive ground truth to understand orbital data, and MSL will help with this. We also can’t explore subsurface environments from orbit, and exploration at depth is necessary for evaluating habitability potential.
- We have shown that high-resolution orbital data is really useful, but how much do we need that, beyond MRO’s lifetime? A: MRO is essential for assessing site-specific science and spacecraft safety. We currently have pretty limited locations where we have sufficient coverage, mostly centered at MSL potential sites (which may not be optimal for future missions). The community needs to think about what data is needed in future missions, while we have capability. In MRO, CHRISM has projected shortest lifetime, as coolers are finicky, and we cannot predict when it will fail. Without it, we have to question whether to send a lander to a site where surface mineralogy is not known.
- How many sites can be fully characterized? A: Thousands of “interesting” sites can be partially characterized. “Really interesting” sites that need full stereo characterizations can be done ~10 per year. This number is related to the size of the landing ellipse, which is related to navigational/landing/EDL abilities.
- There is a growing need for absolute age determination, which is necessary for estimating climate change (and rates of change) and major moments in planetary evolution. Development is ongoing for doing radiometric measurements with in-situ packages, and this should be mentioned in the report (there is a white paper on this). Even a few ages determined from diverse crater surfaces would make a great difference in accuracy of current dating techniques.

- In terms of samples, what is needed to precisely tie down timing within models of core formation? A: With regards to core formation: if SNC meteorites are from Mars, then acquiring another sample won't tell us much more. What we need are samples from different units: core, crust, etc., and volumetric estimates for those units, with age calculated to within 100Myr. Knowing the age/composition of surface rocks would at least validate SNC hypothesis.
- Which questions can be addressed by small-scale missions, which we are not identifying or prioritizing? A: There are measurement possibilities that lend themselves to a Scout class. Perhaps better to consider which ones lend themselves to a mid-large size mission and need to be in a logical flow of missions.
- Infrastructure (such as laboratory techniques and equipment, computer modeling) complementary to missions needs to be considered as well. For example, MSL has created a pipeline for putting different data sets together (making DTMs, etc.) that can be utilized by future missions.

Past Accomplishments/Future Architecture Studies
Richard Zurek
It is difficult to consider the science and infrastructure implications of an individual mission, as science data return from a mission increases drastically when combined with returns from other assets, like MRO, and sometimes many missions are needed to achieve one objective. Thus, it is best to consider an integrated strategy.

In designing mission architecture, start with the science questions. Then consider the mission building blocks (such as TGM, MAX-C, in-situ science/step towards sample return, network), perhaps breaking a large mission into pieces. This can create way stations, which better allow for flexibility in technology and site selection, as well as enabling competed missions to fit in and fill gaps. However, the missions need to be kept close enough to each other to keep expertise fresh and allow for leverage of technology and skill.

It is also important to recognize what has changed – we have a narrower budget and new technology (such as Sky Crane, which yields a smaller landing error ellipse). When considering astrobiological objectives, it is also important to remember that detecting life is difficult, so the program has to and has been moving towards evaluating habitability, which creates a focus on both geology and biology.

A study done by MATT/MART (Mars Architecture Tiger Team, Mars Architecture Review Team) suggests:
(1) Trace Gas Orbiter (TGM) in 2016 with relay capability for future landed missions.
(2) Exo-Mars.
(3) Big lander in 2018: Mars Astrobiology Explorer-Cacher (MAX-C).
(4) Network in 2020.

(5) Mars sample return mission(s) in 2022-24. Perhaps this mission can involve a dual-objective mission (geological and biological) to allow for flexibility and cost spreading.

Discussion during and after the presentation:

- A Network mission would also be of Flagship class, but we first need to develop a landing method (Sky Crane is overkill, penetrators have past mission failure). ESA is also looking into methods for landing small packages, so perhaps network probes can be done internationally. It would also be good to have some idea of activity/location of possible martian quakes, via a prior geophysical observer mission. This could perhaps be done with a Discovery class mission. A first station to characterize the crust does not require knowing where a quake originates, although you need to at least two decoupled stations, and preferably four, to get better science. A mission setting up four stations is significantly harder than three (and takes out of Frontier class), as the fourth needs to be antipodal, so perhaps it would be useful to have one station from previous mission.
- Before we do a sample return, we need to think about whether we can fully understand a sample without knowing the crustal context.
- Using the early orbiters (such as TGM or MAVEN) as telecommunication infrastructure for other missions increases their cost and complexity -- takes TGM out of New Frontier and MAVEN out of Discovery class (additionally, MAVEN orbit is not well-suited for relay abilities).
- Does Mars Network require an orbiter? It would not need to do large data transfers A: TGM can give image coverage of critical events, such as landing. It can also give information about surface processes that affect landers/rovers (ex. dust storms). All of these assets greatly increase value of an orbiter in 2016 – 2018 as the best opportunity to get a large landed package onto surface. To use an orbiter for monitoring as telecommunications, it’s just a question of money and how many functions to add to the orbiter. Perhaps useful to do cost estimate of proposed satellites with and without added payload/capability.
- We should remember that we are not a Mars program architecture panel. Each of our proposed building blocks needs to be scientifically valuable and be important alone as well as within the program. However, we do need to consider assumptions about assets and necessary infrastructure. Perhaps we should include a section on “importance” of Mars science, implementation.

<u>MEPAG’s Trace-Gas orbiter (TGM) SAG</u>
Michael D. Smith, via conference call
The Trace Gas orbiter Mission (TGM) aims to study atmospheric chemistry processes and inventory by detecting a broad suite of gases (with a focus on trace gases) and looking at temporal and spatial variation. In particular, this mission will definitely measure methane levels, as current photochemical models cannot explain the presence of observed methane in the atmosphere, variations, production or destruction, etc.

All science analysis studies have recommended atmospheric remote-sensing instruments with high sensitivity and extensive mapping capability. Adding a moderate camera (1m resolution) minimizes the gap in atmospheric/surface monitoring (after MRO). A sample payload includes: solar occultation spectrometer, sub-millimeter spectrometer, WA camera, thermal-IR spectrometer, and high-res camera. It is also possible that this orbiter can provide telecommunication support for future missions and have synergy with MAVEN extended mission.

Discussion during and after the presentation:

- According to SAG-2 (Science Analysis Group, study 2), the study would not be able to localize a source within 100s km; can we do better now? A: Source localization can be improved with inverse modeling and complementary surface imaging, perhaps down to 10s km.
- What instruments are included in the estimated cost (total $750M)? A: The cost includes notional instruments, and numbers are from the science definition team and last round of Scout missions.
- The total mass allocation for payload is ~100kg, although this is still being negotiated. FTIR (Fourier Trans IR spectrometer) is the largest instrument. The inclusion of a high-resolution camera can also add payload mass.
- Can you supply us with firm spacecraft and science requirements for this mission, so we can submit it for a cost estimate? A: After the mission team's next meeting, we should have a firm design and be able to provide good mass/power/etc. numbers. A couple cost studies have been done, and all have come back with comparable total cost estimates.

MEPAG's Mid-range Rover (MAX-C) SAG

Scott M. McLennan/Chris Salvo

The proposed Mars Astrobiology Explorer-Cacher (MAX-C) Mission aims to prove in-situ science exploration capability, and collect, document, and cache surface samples (evaluating habitability potential and search for biosignatures), and provide key stepping-stones in search for life. Top science priorities are to look at Early Noachian astrobiology, Noachian-Hesperian stratigraphy, and new terrain with high potential for astrobiological studies. Surface studies on Mars dealing with these three priorities mostly involve differences in site selection, not the needed instrument package, so a single rover with general capabilities is needed to explore sites relating to each three priorities.

Target sites are those with high habitability and high preservation potential for physical and chemical biosignatures. The rover will need to be able to access outcrops, to select and investigate targets, and to document sample context. Additionally, one top priority is monitoring atmospheric pressure – this has low-mass requirements. There are several secondary science objectives that may also be included, such as paleomagnetics.

There are many feed-forward aspects, such as a process and method for collecting and organizing (5-8) samples into suites that represent a diversity of products of processes. This is useful as variation can clarify the history beyond what we can find with a few absolutes (for example, a stratigraphic sequence). Additionally, a sample return mission is high cost/high risk, so it needs to deliver high value, which requires finding and delivering “outstanding samples”. To identify these samples, we need to understand the context, which is easiest if the instruments are available at the site when the sample is collected. MAX-C can collect, document, and package samples for potential return (with planetary protection capabilities), making it the first component of a future sample return campaign. This eases mass and technology development requirements for the later sample return mission.

We have done a full Team X study to look at in-situ measurements and sample-caching objectives. By creating “strawman” payload estimates and landing site restrictions, we created a mission architecture with a cost estimate of $1.5-2.0B (with launch in 2018).

Discussion during and after the presentation:

- How did astrobiology arrive on top? A: We broke into groups, listened to discussion, and then voted on prioritization. Regardless of (diverse) backgrounds and vested interests of the science team members, consensus led to putting astrobiology on top.
- We have a rover traverse goal of 200m/sol for getting to exciting terrain within a 1 martian year life; the minimum requirement is 80m/sol. In working towards this goal, we see that standard centralized motor control is high in power/mass and are exploring other options, such as distributed motor control. Alternatively, if we explore technology for cleaning dust off solar panels, then we can perhaps extend the spacecraft lifetime and decrease the traverse requirement.
- We need more data transfer/storage abilities than MER, but less than MSL. There will be many spectra taken, and some images, but we can prioritize what data is needed immediately vs. what can be sent over the next week. MRO has sufficient data relay capability.
- How much development is needed in payload? A: Technology development is needed, for example, in micro-mapping. We have TRL estimates for instruments, and all are TRL 3 and above.
- Are we considering too many large missions? A: We ask the Steering Committee to consider four Flagship missions, but we can request two, if the science is of high quality and high priority. However, even at 2 we really need credible, solid answers about why this is needed by the science. Remember, it is our job to ask for what is best for Martian exploration.
- Do we have enough budget to include a sample return mission? A: As presented, not in 2018 without international help. With US only, pushes to 2020+. However, our task is to formulate a sequence of missions, and external factors define the rate. We need to present something credible, that won’t be dead-on-arrival, but that will allow us to accomplish good Mars science. If the Steering Committee agrees that something is top priority, then they will need to work out the money and possible implementation. For example, with the MER (Mars Exploration Rovers), the science was compelling, it was the right mission at the right time, and it was made to work.

- Everything is going up dramatically in cost (ex., launch vehicles), so everyone is going to have trouble showing up with something under $1Billion without compromising the science. Ex., MAVEN, lower cost but had to cut some science.
- This spacecraft looks like MSL, which means people will immediately be concerned about cost overrun. A: The best approach is to use MSL experience, to work on what we know about doing. And, this is not just a copy of MSL, as there are new instruments and additional challenges in collecting and storing samples, and it is 65kg (15kg instruments + 50kg arm, rover, and cache) vs. 237kg on MSL. This mission really is MER with added instrumentation and caching abilities.
- The ability for sample caching is the main value and an identifying characteristic of this mission, as that creates a first step towards sample return. We did include other instruments, but the vehicle does not get much smaller if we remove instruments, as caching sets the size of the rover. And, we have to show that the next rover to Mars is more than "just another rover going to another interesting place," but instead is part of progress and adding to the technology and science. For example, MSL is also developing the start of sample return with cruise and landing.
- By doing caching now, are we precluding getting samples from other locations, which perhaps are better choices for sample return? A: We can design an architecture for sample return which does not mean we have to use previously cached samples.
- How does your 3-part sample return mission fit into timing and cost? A: MAX-C could be the rover, collecting and caching samples. We can design the cache so that samples can be collected from dead rover, so that we can restart the sample return mission at a later time. If the rover does not find a compelling sample, then it at least gives technology development. A lander/orbiter (possible rover) can continue in next decade or later, and we could fly another MAX-C. Having caches at the surface lowers risk and the potential to do other missions and go other places, but create caches for possible later collection.
- Nowadays we have a good reason to do sample return, even for higher price. If sample return is so important, why not just propose sample return as Flagship mission? A: If we go in with one, large $5B mission, that will have low credibility. Instead, we should propose a $2B component mission which is part of a larger plan (especially as we cannot credibly scope full sample return mission before report is due). Since the last Planetary Survey, we have learned a lot about the cost and technology needed, and the importance of credibly estimating these items.

<u>The Importance of Samples,</u>
Meenakshi Wadhwa
Samples provide a unique perspective with high spatial resolution/high analytical precision. Additionally, small samples can contain records of planetary/SS-scale processes. Returning a sample to Earth allows for a large number of scientists to be involved, as well as allowing for follow-up studies, manipulation of samples, and study flexibility.

Samples are required beyond SNC meteorites, as those offer a highly-biased view about a small subset of Martian surface. A Mars sample return (MSR) mission would provide samples with context from a different environment (for example, aqueous), which is vital for habitability/life questions. It is the one mission that can address all top four science MEPAG goals.

There are 11 candidate science objectives for MSR missions, and no one site will support all of these. We need different samples (in mineralogical type, location) and suites of samples. Ideally, we need an iterative, integrated program of observations compared with in-situ characterization, and then sample return from (previously) characterized site, then continued with further samples. No single mission will achieve all science objectives, so it is better to think of multiple smaller missions, with the first logical step possibly as going to a previously characterized site (MER-A, B, MSL, etc.) with a rover with a drill. Future missions can be guided by results of earlier missions.

Discussion during and after the presentation:

- What is the simplest mission that will still yield good science? A: We can collect a sample without a rover, but the sample may not be ideal. So, we do need mobility and fetch ability. We probably need a drill as well to get below the surface. Maybe we don't need to thoroughly document everything, but do need a microscopic imager.
- It seems that MAX-C is close to minimal, and previous studies have shown this. Additionally, much of its instrumentation is direct heritage from MER with few bump-ups from MSL.
- The biggest uncertainty in designing a sample return mission is the ascent vehicle (MAV). This technology is not proven or developed, although there are existing studies. This is another reason to break sample return into more elements – our understanding of the rover is maturing faster than our understanding of the MAV. If the Panel is to include sample return, it would be useful to at least estimate the pace of technology development needed for sample return: collection/cache, MAV, etc.
- Do we have a cost and technology development estimation for developing a quarantine laboratory? A: A bio-safety laboratory can be built for $75M, but that is just for containment and doesn't include what we need for preserving samples in pristine condition, handling of samples, etc. Additionally, it is quite different considering clean suits dealing with bacteria vs. a gram of Mars material in a sleeve – we can scale down for size and scale up for containment.
- What type of instruments/measurements would you want to use if you had a sample? A: This is presented in a set of slides in a prior SAG presentation: there are 20-30 investigations that could be done with few grams of Martian meteorite for chemical, elemental, etc. characterization. Much of this has been done with SNC meteorites, but we lack context or an idea of how standard the sample is. Additionally all of these are igneous from 3-4 ejection events; a sedimentary sample would be better.

- People are doing cores on Earth and doing detailed analysis from individual grains. There have been advances in isotope analysis allowing for a very rich amount of information about the entire planet from small amounts of samples.

<u>ExoMars Status Report</u>
Jorge Vago
ExoMars is the first mission in ESA's Aurora Program, and is currently being included in a plan for international collaboration in Mars robotic exploration (with TGM and MAX-C). This missions aims to demonstrate technology involved with landing a large payload, having a several km range on the rover, the ability to drill down 2m to acquire samples, and sample analysis techniques. The scientific goals of this mission are to search for biological markers/residues of past (present) life and characterize water/geothermal cycles, within the ancient cratered terrain in the northern hemisphere.

Samples will be collected from rock outcrops, not regolith, so they will need to map out subsurface portions of outcrops to guide identification of good sites. ExoMars will use TGM for data relay up to 2022, as well as use TGM's imaging abilities to map source regions and look at temporal evolution of trace gases with biological interest. The spacecraft will carry seven instruments: stereo/wide-angle cameras, central high-resolution camera (also used to look closely at samples), ground penetrating radar, IR-spectrometer, Raman spectrometer, Mars-XRD (X-ray differentiator), MOMA (mass spectrometer).

The mission team is quite interested in using the MSL Sky Crane system, due to its small landing error ellipse (15km ellipse vs. 75km with older system) and ability to land a large payload. However, the current plan is to fit two rovers on a pallet (MAX-C type + ExoMars), and this has not been studied. Additionally, if two rovers are going to be delivered to the same place, then we need to make sure that

(1) We have compelling reasons (can do rover-to-rover imaging, MAX-C can cache subsurface samples collected by ExoMars),
(2) These rovers have complementary and not overlapping capabilities,
(3) We do not compromise the science objectives of either rover.

Discussion during and after the presentation:

- Before MSL Sky Crane, how were you getting to surface? A: Ballistic entry with release from orbiter. The switch to Sky Crane will help scientifically. However, as an initial motivation was to learn how to land on Mars, there are political issues. Additionally, the fact that the spacecraft now must go to the same site as MAX-C and cannot be landed at a different site disappoints some.
- The more the Survey Panel can emphasis MAX-C's caching abilities, this mission appears more complementary with ExoMars and makes possible entanglement less likely.
- We still have to explain why it is okay to send two spacecraft to the same site, but site selection is a scientific decision that'll be decided in 6-7 years.
- We also need to consider what happens if only one rover can be landed in 2018. ESA interest is very strongly committed to Exo-Mars and collaboration with

NASA on TGM is tied to assistance with Exo-Mars. Currently, we cannot accommodate two rovers in MSL-type heatshield envelope.

- If a geophysical network is scientifically important, then the US will have to take responsibility for it.
- What is ESA schedule, in relation to NRC, NASA? A: We get our budget approved at the end of 2009, after the director general submits a report in Sept and ESA board meetings in Oct and Dec. This is in parallel with NASA, which has an Executive board meeting at the end of Oct. There will be an AO for instruments (offered in jointly in ESA and NASA) later this year, and an ESA ministerial meeting at the end of 2011. Selection for ExoMars is pretty settled – we have many developed prototypes and have spent $200-250M.