**Mars Exploration Strategies: Forget About Sample Return!** D. A. Paige, Dept. of Earth and Space Sciences, UCLA, Los Angeles, CA 90095. dap@thesun.ess.ucla.edu.

**Introduction:** During the past months, loss of the Mars Climate Orbiter and Mars Polar Lander spacecraft have received considerable attention. In reality, NASA's toll of lost missions during this period has been much higher due to the cancellation of the Mars 2001 lander mission and the failure to plan a credible Mars sample return mission. NASA has commissioned a number of internal and independent investigations which have focused on the technical and management failures that were responsible for the failures of the '98 missions. However, the even more serious setbacks that the future missions in the Mars Surveyor Program are experiencing have not received the same degree of critical attention. In this paper, I attempt to identify some of the key science strategy issues relating to these problems, and suggest returning to a strategy for Mars exploration that is more closely aligned with reason, risk avoidance, and reality.

**The Pre-1996 Strategy:** In the course of researching this abstract, I read through the various Mars strategy documents that have been produced by NASA, JPL and the National Research Council over the years. One of the most well-reasoned was produced in January 1995 at the request of Michael Meyer of NASA's Exobiological Program Office, and is entitled "An Exobiological Strategy for Mars Exploration". The study advocates dividing the search for past and present life on into a logical sequence consisting of 5 phases, which are:

*Phase 1. Global Reconnaissance*, focusing on the past and present role of water, and the identification of sites for future, detailed study.

*Phase 2. In-Situ Exploration of Promising Sites*, focusing on describing their geologic, mineralogic, elemental, and isotopic characteristics, as well as the abundance and distribution of volatile species and organic molecules.

*Phase 3. Deployment of Exobiologically-Focused Experiments*, to provide detailed characterizations of the population of organic compounds, and to search for biomarkers of formerly living organisms, and extant life.

*Phase 4. Robotic Return of Martian Samples to Earth*, to improve the characterization of organic compounds, and to verify any evidence for biomarkers and extant life discovered in Phase 3.

*Phase 5. Human Missions*, providing detailed scientific characterizations of sites of unusual biologic interest, or sites that are inaccessible to robotic exploration.

This report, which was produced before the hoopla associated with the Mars Pathfinder landing and the "Mars Rock Discoveries" in 1996, provides a clear, step-by-step approach to answering the question of whether or not life ever emerged on Mars that takes proper account of our lack of scientific knowledge regarding the planet Mars, the distinct possibility of ambiguous results and interpretations of scientific data, as well as the significant technical challenges, risks and timescales associated Mars exploration. It is not a comprehensive strategy in that it is focused on exobiology and does not thoroughly consider investigations of the solid planet, the atmosphere and climate, and preparation for human exploration. However, it does provides a good model for how to accomplish a high-level scientific goal through a series of missions.

**The Post 1996 Strategies:** After 1996, the Mars program began attracting considerably more attention than it had in previous years, and I would argue, became a victim of its own success. After 1996, we saw significant increases in a) the level of visibility, interest and participation in the Mars program, b) the level of funding for Mars activities, c) the administrative levels at which planning decisions regarding the Mars program were being made) d) the overall level of naivete regarding scientific and technical issues associated with Mars exploration that was injected into the planning process. For instance, the successful Mars Pathfinder landing was interpreted by many to suggest that even more ambitious surface missions could be accomplished at even lower cost. In retrospect, we now know that the success of Pathfinder was the result of a very shrewd management approach which maintained large margins in all areas, including scientific performance, as well as very careful attention to testing. We now know that anything less thorough than Pathfinder will probably result in a developmental or mission failure. Also, the fact that credible scientists found "evidence for ancient life" in the ALH84001 meteorite was interpreted by some to suggest that such exciting evidence may be much more ubiquitous on the/ surface of Mars than had previously been imagined, and that confirming the ALH84001 discoveries would only be a matter of returning a suitable sample to Earth for detailed analysis. However, in retrospect, we now know that much of the evidence for ancient life found in the "Mars rock" is ambiguous or debatable, and that similar issues are likely to arise when robotically acquired samples are eventually returned to Earth. We also now have a deeper appreciation for the fact that Mars is a really big place with a complex history to unravel, and that it will take quite a lot of evidence to prove that life ever existed on Mars, or quite a lot of searching to prove that it never did.

During the 1996-2000 period, the incorrect notion that Mars exploration might be "quicker and easier" than thought previously led to a certain degree of impatience with the orderly process of scientific exploration that had been advocated previously. A number of attempts were made to create "leapfrog" architectures in which Phase 4 sample return missions came directly on the heels of Phase 1 global reconnaissance missions, skipping Phase 2 and Phase 3 altogether. The net result of this accelerated approach has led to a series of failed mission concepts for the '01 and '03 opportunities that in total, will probably end up costing the community about four years and on the order of 1 billion dollars.

**The Misguided Emphasis on Early Sample Return:** One of the most prominent aspects of the failed 1996-2000 exploration architecture plans was to accomplish the goal of sample return at the earliest possible opportunity. One could argue on philosophical grounds that this aggressive approach is in keeping with established pattern of human technological and explorational accomplishments, i.e. that most goals are achieved soon after they are technically feasible, and that the publicly stated justifications for accomplishing these goals often have little to do with reality. For the case of Mars sample return, there has been a strong tendency to equate the analysis of returned samples with "good science", and while it is undoubtedly true that one could do a lot of good science on returned samples, we are a long way from a situation where sample return is *necessary* to make further scientific progress towards the overarching goal of understanding whether life ever arose on Mars. If we use the phased exploration strategy advocated by the exobiologists in 1995 as a model, the Mariner 9, Viking and MGS orbiter data sets have/will provide a good deal of the global reconnaissance required in Phase 1, and the Viking and Pathfinder landers represent just the beginning of the in-situ analysis required in Phase 2. Simply put, from a scientific and technological standpoint, we are not at Phase 4 yet. We don't know where to go on Mars to get the samples we need to answer the life on Mars question, nor do we know how to design and build the vehicles and systems we need to accomplish a successful sample return mission, especially within the current resources of the Mars program. Putting sample return first is an extremely low-pay-off strategy that in most games, would signal naiveté, impatience, dishonesty or desperation.

**A Post 2000 Strategy:** The serious setbacks that have been experienced by the Mars program in recent months have provided us with a unique opportunity to reassess where we are going and what we are attempting to accomplish. I believe that the question of whether or not life ever arose on Mars provides a good unifying theme for the program. However, as has been pointed out in independent assessments of NASA's Mars exploration architecture by COMPLEX and elsewhere, that a definitive scientific answer to this question will require the accomplishment of a broad range of scientific investigations of all aspects of the planet – not just the analysis of a few grams of the first rock samples.

Since 1995, we have learned nothing which suggests that a phased exploration strategy as advocated by the Exobiologists should be substantially modified. The only outstanding issues relate to the pace of the program and its breadth. Clearly, the events of the last months have made it clear the "leapfrogging" is not going to work - not from a scientific standpoint, nor from a technological standpoint either. The notion that the next site we land at must necessarily be *the* site that we go to collect the first set of returned samples has got to be discouraged if we are ever going to explore the true diversity of the planet and its environmental history. Right now, we posses the technology and the resources to do a first-rate job of Martian global reconnaissance and in-situ exploration of a wide variety of sites.

There will always be scientists with laboratories who will advocate that NASA provide them with Mars samples for them to analyze. The fact is, however, that we don't yet have the technology to do this within acceptable levels of cost and risk. Those who are anxious to move the program forward toward sample return have more than enough to do in the areas of basic technological development, risk reduction and testing.

As we are able to attract more resources to the program, it is vital that we use them to in manner which maximizes program's excitement and further increases its scientific integrity. Key to this integrity is an increased emphasis on program breadth – to not just focus on the "life on Mars question", but to broaden the range of inquiry to encompass all relevant Mars science disciplines. While some may find this broad-based approach frustrating, one can point to a number of examples in the fields of earth and planetary sciences where the most important breakthroughs in real understanding have come from the comparison of data acquired from multiple disciplines. I believe that it is only through this approach that we will ultimately unlock the many secrets that the Red Planet has in store.