**Advancing Science of the Moon**

**Report of the Lunar Exploration Analysis Group Special Action Team[[1]](#footnote-1) : Comment**

We have known for the past decade that aspects of our model-based understanding of the fundamentals of the origin and evolution of (i) the Solar System on the one hand, and (ii) the Earth-Moon system on the other, are less robust than we had hoped to date. To put it bluntly, it appears that we have potentially employed incorrect methods and drawn incorrect conclusions in both instances. Most importantly, Earth’s Moon plays a critical role in models of both of these fundamental issues.

First, our flawed understanding of the significance of absolute dating of Apollo Program lunar samples has led us to apply a ~3.8-3.9 Ga yardstick to heavily cratered terrains across the Solar System from which we have constructed an history for the early Solar System, based on the concept of a Late Heavy Bombardment (a period of puzzlingly late accretion) provoked by a complex migratory model of the gas and ice giant planets during the ~4.5-3.8 Ga[[2]](#footnote-2) period.

Second, over the past two decades, contradictions in our astrophysical and geochemical understanding of the formation of Earth’s Moon during the primordial (first 100 Ma) accretionary phase of Solar System history have led to models ranging from collision between a Mars-size impactor and a proto-Earth, via models of multiple impactors – both sets of models associated with partial mixing of target and impactor - to a recent model of total volatilisation and mixing of both the proto-Earth target and a single impactor : none of which are scientifically complete.

A decade ago, growing awareness of our lack of observational data pertinent to these two facets of the accretionary and evolutionary history of the Solar System led to a Community White Paper by Bottke et al. (2009)[[3]](#footnote-3), that was - and is still – excellently focused on these two fundamental questions.

The current report, dated 2017 - *Advancing Science of the Moon : Report of the Lunar Exploration Analysis Group Special Action Team* – was conceived as an updated evaluation of an earlier National Research Council Report (2007) regarding the scientific context for the exploration of the Moon that *“remains the definitive articulation of the scientific priorities and goals of lunar exploration”*.

It is unfortunate that the authors of the 2017 document were unable to step beyond the constraints imposed by limitations of the 2007 report. Critically, the 2017 report fails to distinguish ***essential*** from ***"nice to know"*** science. It emphasises personal favourite, curiosity-driven projects - rather than fundamental science that will drive and expand our understanding of the Solar System and the Earth-Moon system.

The Moon is indeed *“a cornerstone of planetary science”,* but this is not apparent from the 2017 document. This report appears to have been written in segments, by different teams – each more intent on obtaining funding for their on-going personal projects than on addressing fundamental scientific issues. Inexcusable editorial glitches and scientific overlaps between “Concepts” combine to highlight a lack of coherent scientific vision and leadership. To be blunt, 2017 is not a professional document : it could have been written by, albeit well intended, students.

Shackled to the structure and concepts of the 2007 report, the authors of the 2017 document skim over the first fundamental issue (doubt regarding absolute dating throughout the Solar System using impact statistics). In Science Goal 1a. Test the cataclysm hypothesis by determining the spacing in time of the creation of lunar basins the fundamental role of the lunar cataclysm hypothesis at the scale of the Solar System is not even mentioned : nor is the all important Community White Paper by Bottke et al. (2009), referred to above.

Similarly, the second fundamental issue (origin of the Moon) is relegated to a New Goals “post-script” section, alongside lunar volatiles and late lunar contractional faulting. Volatiles really belong in the volatiles concept inherited from the 2007 report, as the 2017 text itself clearly states : little new is added beyond a recent appreciation of a possible tri-partite (primordial-surficial-sequestered) origin of volatiles. Worse, the late lunar contractional faulting proposal is a type example of curiosity-driven science : the cited bibliography shows that this topic is of concern to only a small inter-related group of self-interested researchers.

A science program whose costs will figure in the billions must be thoroughly justified, and the justifications must relate to fundamental science advances. Obviously, once the fundamental program is agreed to, it makes sense to piggy-back other secondary/tertiary projects onto the infrastructure that will support the primary objectives. However, the subsidiary activities should have better justifications than “we need inventory”. Let me be clear : I do not disagree with most of the goals proposed in the 2017 document[[4]](#footnote-4), though much of the text comes across as unfocussed and scatter-gun in approach – even poorly thought through[[5]](#footnote-5). However, the report is written backwards on at least two counts. First, the fundamental scientific issues are glossed over, as discussed above. Second, the details of the secondary/tertiary lunar work are presented either without (most commonly) or prior to all too rare references to the scientific questions to be resolved. Most importantly, no attempt is made to justify the fundamental nature – assuming it to be the case – of the science being proposed : why is this work important - and to whom? ***A poorly written document does a great disservice to science, no matter how potentially interesting the science may be*.**

**To summarise :** This report concerns a scientific proposal of the highest priority for Solar System science. While I may quibble about certain specific minor goals, overall I do not dispute the pertinence of much of the contents. However, the report is poorly written, badly constructed, and lacks a clear sense of priorities. Two fundamental science issues that concern the dating of the early history of the Solar System and the origins of the Earth-Moon system are drowned in a plethora of curiosity-driven (“nice-to-know”) projects. Reference to the fundamental issues is scattered across the report. The “nice-to-know” questions are presented with little or no justification of why they address important goals to achieve.

Personally, as well outlined by Bottke et al. (2009), I believe that it is critical to Solar System and planetary science that we return to the Moon to address the two fundamental science issues. I also believe that it is valid to piggy-back secondary/tertiary projects on the primary infrastructure. However, this report does a very poor job of presenting and justifying a multi-billion dollar mission to the Moon’s surface. A poorly written document – such as this report - does a great disservice to planetary science in general and Solar System science in particular, no matter how potentially interesting the science may be.

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1. Lunar Exploration Analysis Group (2017) “Advancing Science of the Moon: Report of the Specific Action Team”, held 7-8 August 2017, Houston, Texas, United States of America : <https://www.lpi.usra.edu/leag/reports/ASM-SAT-Report-final.pdf> [↑](#footnote-ref-1)
2. Ga = billion years [↑](#footnote-ref-2)
3. For example : https://www.researchgate.net/publication/291787149\_Exploring\_the\_bombardment\_history\_of\_the\_Moon [↑](#footnote-ref-3)
4. Especially ground-truthing GRAIL, investigating multi-ring basins and peak rings, evaluating electrostatic dust as a settlement and science hazard [↑](#footnote-ref-4)
5. E.g. seeking ancient regolith beneath basalt lavas in surface pits ! The lavas are kms thick, the pits are 10-100s of metres deep. [↑](#footnote-ref-5)