Review of Hartmann, History of the Terminal Cataclysm Paradigm…

This is an interesting if somewhat rambling discussion of the impact history of the Moon and specifically whether or not that was a ‘spike’ in the impact flux at around 3.9 Ga. It is written in a somewhat unusual style, combining anecdotes with extensive literature review and historical perspectives, and some new ideas and calculations for evolution of the lunar mega-regolith. Basically, it is a comprehensive statement of the author’s personal view of the long history and debate over this topic, and I think it would be appropriate for publication in this special issue as a ‘memoir’ from one of the pioneers in the field (Wood 2019, this issue).

That said, I do have several comments and concerns over the way the story is framed and some of the content, especially regarding the samples and the information obtained from them. If the author does not wish to make at least modest revisions along the lines indicated below, then I would suggest adding an explicit statement to the Introduction that the paper constitutes the author’s personal perspective on the topic rather than a balanced and objective overview.

First a minor gripe. I do not think the term ‘paradigm’ as applied here is appropriate. A Kuhnian paradigm shift involves a fundamental conceptual change in the way that a field is viewed at the level of scientific theory, for example, the shift from classical mechanics to quantum mechanics or in the earth and planetary sciences, the shift from geosynclines to plate tectonics or from an earth-centric to a sun-centric view of the solar system. That is not what we are talking about here. Rather, here everyone agrees that about the basic process, i.e., impacts of asteroid-size bodies, and the argument is really only about the timing (Lines 139-140, 2153-2154). I think this is what Kuhn would call ‘normal science’, not a paradigm shift because the alternative hypotheses can be identified, tested (e.g., get reliable basin ages), and evaluated without the need for fundamentally new concepts. There may well be broader implications, as the paper amply demonstrates, but that is not the same thing as a Kuhnian paradigm shift. So I would suggest deleting the word ‘paradigm’ from the Title, Overview, and Conclusions (it is not really used much elsewhere in the discussion) and replacing it with something a little less self-referential, like ‘concept’, ‘hypothesis’, or similar.

A more significant criticism is the author’s attempt to equate the LHB exclusively with a ‘spike’, and his claim that the concept of a single late spike has long been widely accepted within the planetary community. Importantly, he explicitly links Wetherill’s (1975, 1977) original use of the term ‘late heavy bombardment’ with the ‘Cataclysm View #1’ (a single late spike) of Tera et al. (1974). This then becomes the basis for considering all previous mention of the LHB as equivalent to advocating a single spike at ~3.9 Ga, and for distinguishing the author’s concept of an ‘early intense bombardment’ with the implication that he was right all along and was often the lone voice in the wilderness (that is my impression of the tone of much of the paper anyway). However, while this might be the view of the author, I do not think that such a precise conceptual linkage between the LHB a spike can be sustained based on a careful reading of the Wetherill papers (which I just did). For example, Wetherill (1975) was specifically concerned with whether or not the **end** of the heavy bombardment was synchronous across all terrestrial planets rather than with the exact form of the time-dependence of the prior bombardment, and he choose his words carefully to avoid presenting the idea of a single spike. E.g., on p. 1549, he says “If one wishes to interpret the photogeological and radiometric age data to indicate a peak flux or “cataclysm” at ~3.9 Ga and possibly at discrete earlier times…”. This is clearly a nod to Tera et al.’s ‘Cataclysm View #1” and ‘Cataclysm View #2 rather than promoting or supporting either of those alternatives specifically. In his Introduction, he also distinguishes “…an ‘early heavy bombardment’ which more or less accompanied the accretion of the planets, from a “late heavy bombardment” which continued for hundreds of millions of years and may have been episodic in character (Tera and Wasserburg, 1974). Near the bottom of p. 1540, he says that his aim is to evaluate the possibility that “the lightly cratered post-basin surfaces with crater counts of ~120/10e6 km2 represent a ‘marker horizon’ throughout the inner solar system corresponding to a time of 3.9 +/- 0.1 Ga, as indicated by the lunar radiometric chronology.” So very clearly he is assigning that time to the **end** of the LHB and not the entirety of it. In fact his Fig. 3 shows the results of his modelling with impact-related ages ranging from 4.2 to 3.6 Ga and a broad peak between 3.8 and 4.1 Ga. The caption to that figure states “An apparent peak at ~4 Ga results from the combined effect of the intense flux eliminating ages >4.2 Ga and then ceasing to reset ages at ~3.6 Ga because of the decay in the flux”. This sounds to me astonishingly like Hartmann’s view! And then on p. 1555 he states “decay to a flux equal to that of the lunar and Mercurian plains units would require an additional 3 half-lives or about 600 million years”, again generally consistent with Hartmann’s Fig. 16 of the current paper. Of course, p. 1557 reveals one reason that the current author may be getting his own back as Wetherill states “It is believed that this model of the LHB should be taken seriously, rather than dismissed as ad hoc (Hartmann 1975). It is not ad hoc, being associated in an essential way with other processes in the solar system, namely major planet and comet formation, rather than simply being invented to explain the LHB.” Hartmann’s claim that Wetherill’s concept of ‘storage places’ for the impactors equates to support for a spike doesn’t hold up because even Hartmann’s declining bombardment needs to keep at least some impactors dynamically isolated (stored) for several hundred million years to produce the youngest lunar basins.

A careful reading of Wetherill (1977) further demonstrates that Wetherill’s original concept of the LHB **did not require a spike** and that he associated the 3.9 Ga age only with the **end of the LHB**. For example, the Introduction of that paper says “This includes the causes of the LHB of the Moon which terminated about 4 b.y.…” and Fig. 2 of that paper showing a smoothly declining calculated impact rate with half-lives almost identical to those on Fig. 16 of this manuscript. Wetherill (p. 11) also states “If the lunar resetting at 3.9-4.0 b.y. is to be attributed primarily to basin-resetting events…this would imply that many basins were formed within about 100 m.y. **The evidence on this question is not clear…**” (bold added for emphasis). While Wetherill’s concept of resetting derived from his view of basin ejecta as primarily ‘cold’ and therefore requiring many events to reset an age, obviously he was cautious about interpreting the data in terms of a spike.

In my opinion, therefore, equating the original concepts of the LHB as described by Wetherill with a single spike as described by Tera’s ‘Cataclysm View #1” is poor epistemological scholarship and must be revised. It would also be appropriate to mention that Tera et al. also allowed for a significant prior history of major impacts that was largely erased or obscured by the younger events (their Cataclysm View #2), in addition to mainly representing Imbrium as highlighted in the ms.

This revision would make the author's critique of all prior uses of the term 'LHB' more cumbersome, but I think revision here is necessary to accurately reflect the content of the work that is being criticised. But Wetherill is not really the problem; to my mind the trend of equating the term LHB with a spike (e.g., Boehnke and Harrison) can be traced to the much more recent set of papers from the Nice group and other ddynamicists, e.g. Gomes et al. 2005. Those high-profile papers solidified the concept of the LHB as a spike in the mind of the broader audience that Hartmann refers to, which was unfamiliar with the history and development of the science. From my conversations with dynamicists such as Bottke, I can vouch for the difficulty in getting them to modify their language ('LHB' is such a convenient linguistic shortcut), and I would suggest revising the historical discussion presented here more along these lines. Perhaps it will get their attention.

I would also question the author's repeated assertion that the Cataclysm View #1 was widely accepted, especially by the earth and planetary community, prior to about 2005. Ryder certainly was a strong advocate for Cataclysm View #1, even extending it to all lunar basins, which Tera et al. clearly did not intend. However, from a brief literature search I could not find compelling evidence of the widespread acceptance claimed by the ms, and the manuscript does not provide much support for this view apart from the author's impression that he lost a debate with Ryder at the Perth MetSoc meeting (I was there). While I am sure that was traumatic for the author (with Uncle Jerry's words still ringing in his ears), it is not adequate to support the version of history that is presented in the manuscript.

For example, Ross Taylor's 1982 book Planetary Science: A Lunar Perspective states: "One interpretation of the age data…could support the concept that Nectaris and the post-Nectaris basins all formed within about 100 m.y. There are, as noted earlier…, many pre-Nectarian basins, and the occurrence of several large collisions in such a period is **not suggestive of a spike** in the cratering flux and depends heavily on interpretation of individual samples…There are sufficient uncertainties in the interpretation of the age data to make the cataclysm a **non-unique interpretation**." (p. 242, Section 5.10.3). And his 1992 book Solar System Evolution: A New Perspective lists 11 arguments both for and against the "cataclysm" (p. 172, Section 4.11). The summary paragraph on the next page says: "…the view is taken here that the accretion of the Moon was essentially completed by 4440 m.y., and that the record is consistent with a major spike of basin-forming collisions between about 4000 and 3850 m.y. Whether impacts spread over 200 m.y. constitute a "cataclysm" may be left to experts in semantics; the term is now too thoroughly entrenched in the lunar literature to be changed.". He refers to Fig. 4.11.2, which shows two curves, one representing a declining accretion with superimposed small spikes and the other Ryder's strong view with all basin ages between 3.8 and 4.0 Ga. Ross' view at the time was that a spike could be due to break-up of a single large body as a stochastic event. This is a much more balanced view than implied by the current manuscript, and, with respect to Graham, I think Ross Taylor was more widely influential.

In a similar vein, the review of eucrite ages by Bogard and Garrison (2003 MAPS) states: "However, whether the older (pre-Nectarian) lunar impact basins have ages of ~3.9–4.0 Gyr or are considerably older than 4.0 Gyr is still not clear. In the absence of good age estimates for the older lunar basins, we cannot know for certain when an increase in flux of large impactors actually began, or even if a dramatic increase above the background flux actually occurred." And further "…the lunar rock chronology that defines the impact cataclysm is associated almost entirely with only 2–4 large and recent lunar near side basins, primarily Imbrium and Serenitatis, and less certainly Nectaris and Crisium. The time duration is still unclear for the cataclysmic bombardment involving smaller impactors that produced, not impact basins, but large lunar craters on the back side of the moon (and the largest craters on Vesta, which are much smaller than lunar basins)."

As another indication of the view in the earth sciences, Brent Dalrymple's 1991 book The Age of the Earth includes a chapter on the Moon that presents a strat section and a table (5.4, p. 221) of basin ages that go back to 4.4-4.5 Ga, with numerous references including several of Hartmann's papers. More recently, Zahnle et al. (2007, Emergence of a Habitable Planet, Space Sci Rev) has Section 8 – The Late Bombardment, which states "whether these [large] impacts were the tail end of a sustained bombardment dating back to accretion or whether they record a catastrophe associated with rearrangement of the architecture of the solar system (e.g., Gomes et al 2005) is contentious but obviously of some importance to the Hadean environment". They then emphasise the "two extreme views" referencing Ryder and Hartmann, and present both types of models along with a couple of others on their Fig. 12. They conclude by saying that histograms of Apollo age data suggest a spike but don't demand it (citing Wetherill and Bogard) and that "on balance, we prefer cataclysms over monotonic decay. To our minds, the most telling argument against a huge unseen Hadean flux is that it doesn't explain anything else in the solar system that needs explaining; it's kind of a dead end. By contrast, a cataclysm (or cataclysms) fits well with current concepts of how a solar system might evolve." To my reading, that is hardly a ringing endorsement of a spike but a philosophical stance that further emphasises the important influence of the Nice models among the wider community rather than Ryder's petrological or geological arguments. Zahnle's paper has been cited ~160 times so it has been influential.

Another relatively recent example would Chapman, Cohen, and Grinspoon (2007, What are the real constraints on the existence and magnitude of the LHB? Icarus), which is not cited in the manuscript. This is a significant paper (cited 98 times) because it addresses several of the issues mentioned in the current manuscript, so it is surprising that it was not included in the discussion. Those authors do link the LHB more explicitly with Tera's cataclysm at 4 Ga (consistent with its post-Nice publication date), but they also state that "the evidence concerning bombardment history prior to Nectaris is quite incomplete; there is much debate about whether the post-accretionary bombardment rate ~4.4-4.0 Ga was light, in which case the LHB was a comparatively large spike…, or instead the rate was heavy, so the most salient feature of the LHB is that its end marks a sudden decline after a long history of bombardment." Their literature review also emphasises the influence of the Nice papers, remarking that those models were in fact tuned to produce a spike. The primary aim of that paper is a critical evaluation of the lunar age data, and they discuss "potential problems with Ryder's argument" concerning no melts = no impacts and propose a sampling bias related to Imbrium. They also offer an explanation for the abundance of younger ages in Cohen's study of lunar meteorite clasts, which is that those clasts were produced mainly in small impacts whereas the large samples of crystalline Apollo melt rocks favour larger impacts. Based on the petrology of these samples, I find this explanation appealing and something that needs to be considered in the current manuscript. It is not good enough to just point to the age distributions; if the author is going to make those arguments then he is responsible for critically evaluating the data and the samples, and exploring alternative explanations. Interestingly, Chapman et al critically evaluate Hartmann's stonewall and find that the process is "untenable" (p. 244). Perhaps that is one reason this paper was not cited in the current manuscript! Finally, they critique the simplistic approach of plotting individual age data on histograms as done in the current manuscript, and suggest that a better way is to consider the evidence for individual events (see also Norman et al 2006 GCA for a more detailed discussion of this for A16 melt rocks, also not cited).

Finally, the definitive review chapter by Stöffler et al. (2006, NVOTM), on which Ryder was a co-author, provides the following statements:

p. 583: "…the cratering rate between 4.5 and 4.0 Ga is not known and there is still room for speculation about a possible late lunar cataclysm (Tera, Ryder, Hartmann)."

p. 583: "We believe that the review of all available data discussed in this chapter provides **no solid arguments for assuming a low cratering rate early in the Moon's history and one distinct, late cataclysm** (around 3.8-3.9 Ga), but it leaves the possibility for the existence of discrete spikes in the pre-Imbrium cratering rate, which in general appears to decline smoothly from a maximum rate existing very early in the Moon's history (see also Hartmann 2003).

p. 584: Open Questions: "The existence (or not) of a late cataclysm or of spikes in the impact flux prior to 3.75 Ga…The problem with the lack of fundamental groundtruth data concerning the lunar cratering rates in the "old" pre-Nectarian…can only be solved by new sample return missions…"

There is also an extended discussion of Hartmann's views as expressed in this manuscript. Clearly this definitive and relatively recent review does **not** favour a cataclysm, although it leaves open the possibility.

Further, the invited article by Norman (2009, Elements, cited in the manuscript) states: "Resolving a singular spike…from a more continuous but perhaps bumpy prior history may sound like a straightforward problem, but the available record is sparse and often lacks critical context. The paucity of impact melt rocks with crystallization ages older than 4.0 Ga has been cited as strong evidence supporting a late cataclysm (Ryder 2002), but our poor understanding of regolith dynamics diminishes the significance of this observation (Chapman et al., 2007)." I would invite the author to examine the strat section and accompanying discussion about basin ages also presented in that paper for any bias in favour of a cataclysm.

And even more recently, the review by Fassett and Minton (2013 NGEO) stated:

Three scenarios[5] for the nature of this cataclysm are: (1) many, or most, of the main impacts on the Moon date to this very short period of time; (2) several of the chief impacts date to this period of time, but other large basins formed in the 500 Myr before 4.0 Gyr ago; or (3) the samples were universally affected by the formation of the Imbrium Basin ~3.9 Gyr ago and were radically altered by this last large event near the landing sites….There were objections raised against the cataclysm (or Late Heavy Bombardment) interpretation almost immediately[7–9], especially in its most catastrophic form. …"

And Norman and Nemchin (2014 EPSL) state: "The strong version of the late cataclysm hypothesis in which all of the lunar basins formed between 3.8 and 4.0 Ga (Ryder, 2002; Gomes etal., 2005; Abramov and Mojzsis, 2009) appears untenable."

The brief literature review presented here suggests to me that the current manuscript promotes an unbalanced, not to say incorrect, view of the way that the LHB and cataclysm concepts are and have been perceived in the earth and planetary community, especially in the period of the 1970-2000 but even as late as 2006-2017 and continuing to the present. The discussion comes across as still fighting old wars with Graham in which the author would like to cast himself as the underdog, but I just do not see the evidence for that. Rather it appears to me that anyone who has thought seriously about the problem recognizes the uncertainties and the lack of critical data necessary to evaluate the competing hypotheses, and takes an appropriately cautious view (unless they are seeking Nature papers of course…).

That said, I agree that the LHB now tends to be equated with a cataclysm in much of the broader scientific world, and I agree that this is a problem. However, I see this as due primarily to the influence of the prolific and high-profile Nice group rather than how these concepts were viewed within the earth and planetary sciences prior to ~2005. Also, given the audience of this journal (Geosciences), I am not sure the current manuscript will receive the necessary traction to reach that broader audience.

On a slightly different topic, another uncited paper that I would consider to have been highly influential within the lunar community but also uncited here is Odette James (1981 PLPSC, Petrologic and age relationships of the Apollo 16 rocks: implications for subsurface geology and the age of the Nectaris Basin). This was an important paper, firstly because Odette was also a strong the advocate of the Cataclysm hypothesis, but also because it provided to be what appeared to be a well-defined and quantitative constraint on the age of Nectaris. Her proposed age of $\leq$3.92 Ga provided a strong pin for a terminal cataclysm, and was adopted into many compilations and reviews, e.g., Stöffler and Ryder among others. Her interpretation, which was based on the assumption that the A16 Descartes Formation was Nectaris ejecta, stood virtually unchallenged until Norman et al. (2010 GCA Imbrium Provenance for the Apollo 16 Descartes Terrain…) showed that the Cayley and Decartes Formations are coeval, supporting early, post-mission geological interpretations by the USGS that they were both Imbrium ejecta. Without meaning to toot my own horn (too much), I believe that my GCA paper (again not cited here) and related presentations at numerous conferences made a meaningful contribution to the current view of a widespread Imbrium overprint at all Apollo sites (together with Spudis et al., 2014 JGR, also not cited). Although this idea does support the headline conclusion of Haskin et al., the argument is not so simple because Haskin et al. believed that all of the mafic Apollo melt rocks were produced directly by Imbrium whereas Norman et al. (2006 GCA) showed that coherent petrological and geochemical groups of A16 melt rocks have distinct ages consistent with multiple impact events toward the end of the LHB. Schaeffer and Schaeffer (1977 PLPSC, uncited) also reached a similar conclusion regarding the ubiquity of Imbrium ejecta at the Apollo sites.

In summary, I think the manuscript substantially over-states the Terminal Cataclysm (TC) as a "paradigm" when its actual status would be better stated as a hypothesis. The manuscript also over-states the degree to which the earth and planetary science community accepted the strong version of the TC hypothesis, especially prior to ~2005. I therefore recommend revision to present a more balanced view on both of these topics. Fortunately, that affects primarily the introductory and more historical parts of the manuscript, but similar comments are scattered throughout the paper. A number of comments are listed below by line numbers, including instances where I think the author has mis-construed or mis-stated a few aspects.

Line 8: A paradigm is, by definition, widely accepted so the phrase 'widely accepted paradigm' is redundant.

Line 11: As noted above, the original use of the LHB did not define it as a spike.

Line 18: The Norman et al LPSC abstract with the 4.2 Ga age for melt rock 67955 was presented in 2007.

Line 34: unclear which critiques these were or why the author considers them to have been ineffectual.

Line 39 and elsewhere: 'martian' is an adjective like 'red' and so should be written in lower case whereas Moon is a place name (now recognised officially by NASA) and so be capitalised.

Line 60: This characterisation of the papers by Wetherill is not accurate and should be revised.

Line 64: While equating the 'Terminal Cataclysm' and LHB is a convenient rhetorical device, it is historically inaccurate prior to about 2005 when the Nice group papers came out. It was really Gomes et al. 2005 who equated these concepts with their title of their Nature paper: "Origin of the cataclysmic Late Heavy Bombardment period of the terrestrial planets"

Line 82-84: This assertion that various authors have redefined the LHB out of recognition is not compelling and might stand only because the author has adopted an unrealistically restrictive definition of the LHB.

Line 173: "Genesis"

Line 228: It is unclear how "widespread" that event was from the available samples as they are carried mainly by Imbrium ejecta. As noted above, this sentence is not historically correct and the Lunatic Asylum may have been the only 'true believer'. Even Grenville Turner took a cautious view in his 1977 review of early lunar bombardment (Phil Trans R. Soc. London):

"The clustering of most lunar highland ages in the interval 3.9- 4.0 Ga has impressed all involved in the measurement of these ages. It has been referred to by Tera *et al.* (1973) as the lunar cataclysm and interpreted broadly in terms of the **termination** of the intense bombardment of the lunar highlands. What is not yet clear is whether this 'cataclysm' represents simply the effective termination of an approximately monotonic decrease in the early cratering influx or whether it represents a period of increased influx to close to 4.0 Ga. The solution of this problem is currently a major aim of lunar chronologists….If the ages represent the termination of a decreasing influx then mechanisms must be found to account for the implied capture lifetime of the colliding solar system debris. This problem has been discussed recently by Wetherill (1974, 1975) in terms of orbital dynamics. If on the other hand the 'cataclysm' represents a real and significant increase in influx then some catastrophic event is implied, for example a major collision between two asteroids providing a source of new impacting objects."

Note Turner's characterisation of Wetherill's work as referring to a declining flux and the TC as the **termination** of the LHB.

Earlier in that same paper, Turner states: "Several authors (see for example Turner et *al.* 1971, 1973; Jessberger *et al.*1974; Schaeffer & Husain 1974) have made the assumption that, to a first order, the age distribution of the rocks at the sites sampled is dominated by a relatively **small number of major basin forming events**…" so that the characterisation here that "many" of the largest basins forming in <200 Ma is not an accurate statement of the situation in the mid-1970's as reflected in the literature from leaders in the field.

Line 241: I am not sure that characterising Imbrium as Tera et al's first choice is completely accurate. Certainly they did mention that possibility, e.g. on p. 15 of their paper, they state: "It is possible that the widespread metamorphism is the result of the Imbrium event only". However, they go on to say that the data suggest at least 2 events, which they assigned to Imbrium and Crisium, and therefore all of the basins in between these stratigraphically. Distinguishing two events derives from Jessberger's Ar dating and also differences in ages and initial $^{87}Sr/^{86}Sr$ implied by the CIT mineral isochrons on lunar melt rocks. Importantly, however, they also say that "Other major impacts defined by the older [i.e., pre-Crisium] must have occurred in the 0.6 AE preceding the cataclysm at 3.9 AE…". And their 1974 PLSC paper (not cited) states: "Precise details of the history prior to ~4.0 AE is at present somewhat obscure." So even the proposers of the cataclysm themselves explicitly did not call for every basin to have occurred simultaneously at 3.9 Ga (Ryder 2002 is responsible for that). Note also that the Tera et al. 'spikes' are superimposed on a smoothly declining background, e.g., from the caption to their Fig. 6: "Apollo data resulted in cratering rates which required first an exponential and then several exponential functions with different time constants for a good fit of isotopic ages."

Line 257: Another irony is the line from Michael et al: "The 'lunar terminal cataclysm' hypothesis is one possible element of the **early intense bombardment**." Unfortunately, that paper also made the mistake of referring to "…a strong peak in the bombardment of the Moon when most of the craters now observed in the lunar highlands…were formed", referencing Tera et al., but as we have seen, Tera et al never said any such thing. So clearly there is confusion over terminology and concepts even among practitioners, which admittedly complicates and hampers effective communication.

Line 309: What is the basis for asserting "the lack of a positive reception of [Hartung's] abstract"?

Line 383: The claim that lunar melt rocks were thought to offer an **ideal** measurement of basin-forming impact history is over-stated. Even very early (e.g., Turner et al., 1971; Papanastassiou and Wasserburg, 1972; Jessberger et al. 1975 PLSC) recognised the complications associated with relict clasts and technical problems such as recoil in the Ar isotopic analysis. The word 'ideal' should be replaced by 'best available samples'. I don't follow the next line "much better than the igneous rocks alone".

Line 386: The phrase “once and for all” can be deleted. As we have seen, Ryder’s view was an extreme one and it was not adopted universally. Also in this line, the word ‘impacts’ should be replaced by ‘basin ages’ as that was really all that Ryder was addressing.

Line 436: some references are needed to support the assertion that ‘Ryder’s rule’ was specifically adopted as a constraint on theoretical models.

Section at Line 438: Haskin’s idea was presaged by Schaeffer and Schaeffer (1977 PLPSC) based on their compilation of Ar data.

Line 452: This line strongly over-states the “prevailing view” as noted above.

Line 524: The “assumption” that ejecta from a single impact is compositionally homogeneous was actually based on evidence from large terrestrial impact craters such as Manicouagan (e.g., Bill Phinney, Chuck Simonds, etc.). Unfortunately, it may have been mis-applied to the Moon because the terrestrial studies were on central melt sheets, and those did appear to be largely homogeneous until very recently when some evidence of crystal fractionation associated with thick, down-dropped sections has emerged. In contrast, the lunar melt rocks may be associated more with over-rim deposits, which are not well preserved in large accessible terrestrial craters. Note here that Norman et al. (2006 GCA) correlated textures and ages for A16 melt rocks, and distinguished several groups. This appears to support the idea of multiple impact events during the period 3.75-3.95 Ga. See also Jessberger et al. 1975 for arguments supporting multiple events – certainly multiple events were resolved by the Rb-Sr isochrons produced by the CIT group, which was not adequately addressed by Haskin et al. nor mentioned in this manuscript.

My conclusion is that Haskin et al. got the basic idea right, that the Apollo landing sites are dominated by Imbrium ejecta but this does not necessarily mean that all of the melt rocks were formed during that event. E.g., younger large (but not necessarily basin scale) impacts could have formed some of them and they were transported later to the site for collection by the astronauts.

Line 531: The statement that the TC was considered as forming “most of the major lunar multi-ring basins” is not an accurate statement of the actual content of the Tera et al. paper or subsequent reviews, as noted above. Tera et al. applied the concept only to a relatively small number of basins, specifically Crisium and younger. Again, perhaps the author should present this as his personal impression rather than one that is supported by specific literature.

Line 593: Here again the manuscript over-states the “general perception”.

Line 618: I just don’t buy this bit about petrologists and geochemists being biased (but perhaps that’s because I belong to that group). Obviously any discipline is going to promote itself and its approach, especially during proposal time. Here NASA didn’t help by setting up silo programs that separated geology from cosmochemistry, and the way that the early PLSC volumes and sessions were organised by discipline, so perhaps it is not surprising that the author views the geochemists and geologists as different species. Going into the 1980’s there was certainly a movement toward 'consortium' studies that included geologists as well as petrologists, geochemists, and geochronologists.

Line 840: It is unclear which A15 samples are being referred to here as there are not many A15 melt rocks. E.g., mainly the classic 15445/55 pair, and these do have a number of petrologic similarities to the A17 poikilitic melt rocks. That argument is usually applied to A17 vs. A14 and A16 in an attempt to distinguish Serenitatis from Imbrium ejecta.

Line 926: The statement that the Norman and Nemchin 2012 abstract proposed an age of 4.2 Ga for Nectaris simply is not correct. A search of that abstract for the word 'Nectaris' turned up no matches – it is not even mentioned. Note that this study would be better referenced to Norman and Nemchin (2014 EPSL). That paper contains one mention of Nectaris at the end of the discussion: "Although Morbidelli etal. (2012) assigned an age of 4.1 Ga to this shift, the timing of any inflection in the model curves is poorly constrained as we still do not have an absolute age that can be assigned with confidence to any of the older lunar basins, including Nectaris." Norman and Nemchin (2014) proposed entrainment of the clast in Imbrium ejecta, and the Norman et al (2016 GCA) study of that same sample (67955) concluded that it crystallised from a KREEP-rich melt and so its source crater is better placed in the PKT rather than coming from Nectaris.

Perhaps the author is thinking of Fischer-Godde and Becker (2012 GCA), who report a 4.21 Ga age from a North Ray sample 67935 based on Re-Os isotopes. Those authors provide a scholarly discussion of possible relationship of this sample to Nectaris but then conclude that "a definite relationship…to Nectaris to a specific basin (Nectaris vs. Imbrium) remains ambiguous and falls beyond the scope of this study. And Fernandes et al. (2013 MAPS) considers a 4.2 age of Nectaris more specifically.

Caption to Fig. 10: Are you sure this isn't Apollo 16? A17 is not generally considered to be close to Nectaris.

Figs. 9 and 10, please provide the NASA numbers of these images.

Line 946: I do not follow this criticism of the 'traditional' radiometric/petrological methodology as both of the Norman et al studies that established the 4.2 Ga age (2014 EPSL, 2016 GCA) were conducted using exactly that kind of 'traditional' geochemical and petrological methodology.

Line 982: There may be some confusion here as I am not aware of 'clusters' of melt rock ages at 4.2 and 4.3 Ga. Many of these ages are from either disturbed zircons or metamorphic rocks (the lunar granulites). And not melt rocks. See, e.g., the histograms of Michael et al. (2018) referenced earlier. As shown in Fig. 8 of that paper, many of the impactites with ages of 4.1-4.2 Ga are "breccias with no melt". As these types of breccias can be produced by impacts of all sizes, the older ages do not provide a compelling case for older basins, which is the key aspect of the argument.

Line 997: This statement simply is not correct. Wetherill referred to the Tera et al results as marking the **END** of the LHB (see notes above).

Line 1011: It looks to me that the Morbidelli model cited here is fully consistent with the original useage of LHB by Wetherill.

Line 1023: This statement that "the original definitions of LHB explicitly proposed that most or all prominent multi-ring basins formed at ~4.0 to 3.85 Ga" simply is **not correct**, as shown above. This is a statement of Ryder's 'strong version' of the LHB/Cataclysm dating from 2002. Perhaps this idea is inextricably embedded in the author's mind, but it is not supported by a careful reading of the literature.

Line 1026: This old joke will only be understood by Americans of a certain age. It's not helpful and I suggest deleting it.

Line 1090: This assessment of the Cohen et al. data assumes that the meteorite clasts and the Apollo melt rocks all represent similar size events, but this is probably not a correct assumption. Rather, the meteorite clasts have textures consistent with formation in much smaller events, which obviously continued to occur on the Moon long after the basin-forming epoch had ended (see Chapman et al. 2007). As stated by Cohen (2008 Early Solar System Bombardment workshop): "It appears that impact-melt rocks created in post-basin bombardment dominate the very surface of the lunar regolith and are readily incorporated into regolith breccias" [i.e., lunar meteorites].

Section 11, eucrite ages: Author take care to state that crystalline melt rocks analogous to the Apollo samples are **not found** among the HED meteorites. Rather the impact ages for Vesta are based on brecciated eucrites (Bogard 2011 Chemie de Erde is a more recent reference). Therefore the events sampled by the eucrites are not of the same scale as those that formed the 3.9 Ga lunar melt rocks, and were probably much smaller for a variety of reasons (size and speed of impactor, loss of melt ejecta, fragmentation, etc.). So I am not sure there should be a supposition that the ages would match up exactly, but in any case the author needs to state that assumption and offer some justification for it. It is interesting (to me at least) that the eucrite ages match up better with the lunar meteorite ages, which is probably also biased toward smaller events. Bogard (1995 and 2011) gives some discussion of this relative to size of the parent body. E.g., Bogard 2011 says: "Of course, even Vesta could not have survived the very large impactors that formed the larger lunar basins, and the [eucrite] ages …represent impacts significantly smaller than those that formed the large lunar basins."

Note that the interpretation of the eucrite data in terms of dating of impact events by Bogard and others directly contradicts "Ryder's Rule" because virtually all of those ages were obtained on fragmental or partially melted breccias and NOT on coarse-grained, crystalline melt rocks such as the 3.9 Ga Apollo samples. There appears to be some lack of appreciation of the petrology of the eucrite samples being used for this comparison in this manuscript.

The manuscript should mention that the mesosiderites show a very consistent age of ~3.9 Ga (review by Bogard 2011). This is worth mentioning as it indicates a disruption event consistent with a large impact similar to that which happened to the L-chondrite parent body at 0.5 Ga otherwise it gives the appearance of cherry-picking only observations that support the author's preferred interpretation. Note also that Burnett and Wasserburg (1967 EPSL) also found a 3.8 Ga Rb-Sr isochron for silicate inclusions in the Kodiakanal IIE iron meteorite, a result that was confirmed by Gopel et al. 1985 Nature using U-Pb isotopes (her age was actually 3.68 Ga but within error of the Rb-Sr isochron) and also by K-Ar. The Watson and Netscharavo IIE irons also have Ar ages of ~3.7 Ga (Bogard 2011). These need to be mentioned in any objective discussion of meteorite ages.

Line 1126, 1130, and 1144: If the 'H' here refers to howardites, these are achondrites and members of the howardite-eucrite-diogenite (HED) clan. They are not the same as the H-chondrites. Howardites probably come from Vesta; H-chondrites do not come from Vesta. Note that howardites are regolith breccias, and so are analogous to the lunar meteorites and not the crystalline melt rocks.

Line 1149: I do not believe that any of the rocks being referred to here are really impact melts. The author needs to be clear and precise in the terminology here to avoid further confusing things.

Line 1152: This over-arching statement is contradicted by Bogard 2011, who says: "The range of Ar–Ar and some other radiometric ages for impact-heated lunar highland rocks is rather broad, ∼3.4–4.2Ga (e.g., Turner, 1977; Bogard, 1995; Bogard and Garrison, 2003; Norman et al., 2010)."

and

"large impacts on Vesta occurred over a similar time period (3.5–4.1 Ga) as did impacts that reset lunar rock ages and formed lunar-sized basins".

As well as this from Bogard (1995):

"Collectively, the evidence indicates that the HED parent body experienced a similar bombardment in about the same time period as did the Moon."

And from the review of eucrite ages by Bogard and Garrison (2003 MAPS): "…[a] summed probability curve suggests heating events at ~3.45, 3.55, 4.0 Gyr, and possibly also at ~3.7, 4.05, and 3.8–3.9 Gyr. Seven samples give ages of ~3.95–4.05 Gyr, and 7 samples give ages of ~3.45–3.55 Gyr. Only 4 analyses give ages in the range of 3.80–3.95 Gyr, which is the time interval in which several large lunar basins probably were formed by impact (Stöffler and Ryder 2001)…The Vesta cataclysm (assuming the term applies) appears to have begun ~4.1 Gyr ago and lasted until ~3.4 Gyr ago. Fewer eucrite ages occur in the time interval of 4.1–4.4 Gyr than in the interval 4.0–3.4 Gyr, and this suggests that the impact cataclysm **did involve a significant increase in impactor flux**. {MN note: see also Turner 1979 PLPSC mentioned later regarding the significance of this observation] This observation is consistent with the lunar impact cataclysm, as its upper time bound is not well constrained." And they offer various physical explanations for the age distributions: "the late stages of the bombardment may have consisted of smaller impacting objects than those which formed the lunar basins—smaller impactors which may have persisted in the inner solar system until ~3.4 Gyr ago. Presumably, the flux of later arriving objects was not high, or we would observe more ages of

<3.7 Gyr among lunar highland rocks. The third and related explanation is the possibility that both eucrites and returned lunar highland rocks represent an incomplete sampling of the full range of reset ages on these bodies."

The diagram from Bogard and Garrison should replace that of Bogard 1995.

The point of view being promoted here is only sustainable by requiring a strict definition of the LHB and an unrealistically narrow view of lunar cratering history. The manuscript lacks a consideration of other relevant views from the literature. It needs to confront these contrasting interpretations directly and convince the reader by careful argument rather than just assertion.

Line 1217: The reference to Becker et al here is peculiar because that study concerned only the Earth's mantle as sampled by periodite xenoliths. Those authors do note that their patterns of highly siderophile elements are similar to those of Apollo impact melt rocks (referencing Norman, Bennett, and Ryder 2002 EPSL) but they do not provide data on lunar melt rocks directly. Perhaps the author is thinking of Gleissner and Becker 2006 GCA but again that refers mainly to trace element patterns rather than isotopic properties, and they find that the HSE signatures of most lunar melt rocks do not match well with any class of meteorites. Recognising that the author is not a geochemist, the manuscript still needs to be a little more careful in its use and citation of geochemical data.

Line 1239: Please clarify what is being referred to by "these problems". As noted above, this statement is contradicted by the interpretation of the meteorite data in terms of impacts because most of those samples are fragmental, annealed, or in some cases partially molten breccias and are therefore petrologically distinct from the crystalline lunar melt rocks that defined the 'terminal cataclysm'. There are a few chondritic impact melt rocks (noted in Bogard 1995, 2011) but these are very rare and some are very young (e.g., Y-790964 is ~ 1.2 Ga and Chico is 0.5 Ga).

The argument made here extends Ryder's rule to other bodies without adequate justification or consideration of differences in petrology of the samples and size of parent bodies, i.e., crystalline impact melts are not expected on asteroid-size parent bodies. Ryder applied his Rule only to the Moon, and only to basin-scale impacts capable of producing coarse-grained, crystalline melt rocks. The author's attempt to extend it to asteroids is not adequately justified.

Line 1243: This process of destruction by mega-regolith evolution evidently does not apply to Vesta (eucrite parent body) because 4.5 Ga primary basaltic crust is still preserved and is being delivered to Earth as eucrites.

Line 1250: References to 'clusters' of melt rock ages at 4.21 and 4.33 Ga should be given. These are very precise ages and no such clusters are shown on the recent compilation of melt rock ages from Michael et al. (2018). Rather, some of these ages might refer to disturbed zircons, fragment-laden clasts such as those sampled repeatedly at North ray Crater (e.g., Maurer et al., 1978 GCA; Shuster et al., 2010 EPSL; Norman et al., 2010 GCA; Fernandes et al., 2013 MAPS), or to metamorphic granulites. If so, none of these types of materials would be analogous to the ~3.9 Ga crystralline melt rocks, and the significance of those ages relative to size of the impact is unclear. Most likely they would NOT represent basin-scale events. This relationship of age to scale of event is critical for any evaluation of the LHB as not all ages are created equal. Alternatively, the 4.33 Ga age may refer to zircons in an A17 granitic clast dated by Grange et al. 2009 Ga and the 4.22 (not 4.21?) Ga age might be referring to 67955 dated by Norman and Nemchin (2014 EPSL) and confirmed by Norman et al. (2015 GCA), although without references it is hard to guess. If they latter, then referring to individual samples as 'clusters' seems misleading.

To the best of my knowledge, only one sample of coarse-grained melt rock with an age of 4.2 Ga has been established in the literature, and that is 67955 (Norman and Nemchin, 2014 EPSL; Norman et al., 2016 GCA). Whether one would characterise this 163 g, 4 cm long piece of a larger clast entrained in the Outhouse Rock as "tiny" would be a matter of opinion.

Dot points 1, 2, and 5 after line 1254 are all speculative suggestions from the same group at Curtin University about the relationship of impacts to magmatic systems. Virtually all of these zircons they dated have been interpreted as magmatic although a few appear to have been disturbed by LATER impacts. Any initiation of the magmatism by impacts is entirely speculative and does not add much in the way of real evidence. I suggest condensing these three dot points into a single one that says something along the lines that impacts into the early lunar crust may have initiated episodes of magmatism (e.g., Pidgeon, Nemchin, Grange) but any genetic relationship between impacts and magmatism remains speculative.

The best reference for an old impact melt zircon would be Grange et al. 2009 GCA, who dated a zircon in what they interpret as a rapidly quenched granitic impact melt at 4.33 Ga. However, this acicular melt appears to have formed in a relatively body of melt (rapidly cooled, similar to those dated at ~4.2 Ga by Fernandes et al. 2013 MAPS) compared to the crystalline melt rocks, and – importantly – other impact-affected zircons in this sample give an age of 3.9 Ga. So based on this sample there does seem to be a hiatus in impacts between 4.3 and 3.9 Ga.

This is important because Turner (1979 PLPSC) and Turner et al. (1981 R Soc London) applied statistical tests to show that a resolvable gap between impact ages of 3.9 Ga and 4.2 Ga would be consistent with a discrete pulse at 3.9 Ga, i.e., a Terminal Cataclysm. Based on the available data, that does appear to be what the lunar data are showing (e.g., compilation of Michael et al., 2018), and also for Vesta as referenced earlier (Bogard and Garrison) and highlighted by Bottke and Norman (2017). If there is a hiatus in ages, then it not sufficient simply to point to the existence of melt rocks with older ages, rather the existence (or not) of statistical gaps between the age groups needs to be considered.

Line 1268: This dot point mis-states the conclusions of Fischer-Godde & Becker and is misleading. As noted above, the authors provide a scholarly discussion that considers the relationship of this sample to Nectaris but then conclude that any relationship to a specific basin remains ambiguous. More specifically, they state "In the case of 67935 [the sample for which they obtained the 4.1 Ga age], a relationship …to Nectaris seems **unlikely** because it contains a moderate KREEP component" (p. 149). So the dot point is not only misleading but **factually incorrect**. Those authors do, however, argue for at least two impact events at 4.1-4.2 Ga based on the distinct siderophile element signature of 67955 and 67935, and this is something that the manuscript could mention. Personally, I am sceptical of this Re-Os isochron (this sample has complex and partially re-equilibrated metal and no isochron was obtained on much simpler 3.9 Ga melt rocks), but it is in the literature so fair to cite.

Line 1270: This dot point is simply NOT TRUE. As mentioned above, the 2012 Bombardment workshop abstract by Norman and Nemchin did not make any mention of Nectaris, and the 2014 EPSL paper by those authors on the same sample made only a passing mention of Nectaris to say that its age was unknown. Perhaps the author is confused about the citation (perhaps Fernandes et al. 2013 MAPS who does consider the 4.2 Ga ages as linked more directly to Nectaris based on a geological interpretation of the Descartes Fm.), but this kind of error makes me very concerned about the veracity of the presentation based on arguments derived from the literature, especially if based on petrological or geochemical data. The author appears to be playing really fast and loose to get to the answer that he wants. As mentioned above, I think this is fair if presented as a personal perspective and not as a balanced review.

Line 1294: Note that the 4.2 Ga melt rock 67955 is 160 g and 4 cm, and that it is a split of a larger clast that was entrained in Outhouse Rock. The A16 astronauts could only collect a small bit of the clast. Although we might argue over what constitutes the subjective term 'tiny' (the Earth is tiny at the scale of the solar system), at face value this sample seems to contradict the argument presented here in the manuscript.

Line 1295: The "rapidly accumulating evidence" cited previously in the manuscript is mostly speculation and does not address the key question of pre-3.9 Ga basins. In any case, the conclusion that the 'strong form' of the TC is untenable has already been reached by a number of previous studies (e.g., Bottke and Norman 2017) and in fact was used as justification for this study at the beginning of the manuscript so this statement is redundant and can be deleted here.

Line 1411: A better reference would be: Norman MD and Lineweaver CH (2008) New perspectives on the lunar cataclysm from pre-4 Ga impact melt breccia nd cratering density populations. Proc. 7th Australian Space Conf. (Wayne Short and Iver Cairns, eds.). National Space Society of Australia, Sydney. ISBN 13: 978-9775740-1-8 (reviewed conference proceedings).

Line 1429: This dot point somewhat mis-states what Norman &Nemchin (2014) actually say, which is:
" Our result for the crystallization age of 67955 extends the lunar basin-forming epoch back to at least 4.2 Ga. Although the concept of a short-lived terminal cataclysm is not universally accepted (see Chapman et al., 2007), **for the first time** we now have direct, sample-based geochronological evidence for a basin-scale impact melting event on the Moon prior to 3.9 Ga. This provides a more definitive framework for the interpretation of other evidence for pre-cataclysm impacts on the Moon. Multiple impact events with ages ranging from 3.9 to 4.3 Ga have been inferred from brecciated and annealed zircons found in a variety of Apollo breccias, lunar meteorites, and regolith samples (Gnos et al., 2004; Pidgeon et al., 2007; Grange et al., 2011; Liu et al., 2012; Zhang et al., 2012). These zircon ages reflect thermo-mechanical events of sufficient magnitude to reset the U–Pb system in recrystallized or

plastically deformed igneous zircon although the P-T-t conditions, and therefore the magnitude of the impact events, that generated these effects are poorly constrained (Grange et al., 2011; Timms et al., 2012). Some lunar metamorphic breccias and fragment-laden melt-breccia clasts yield apparent ^{40}Ar-^{39}Ar plateau ages of 4.1-4.3 Ga (Stöffler et al., 1985; Hudgins et al., 2008; Norman et al., 2010, Fernandes et al., 2013), but the question of partial equilibration of inherited clasts in these breccias complicates a straightforward interpretation of those data (Norman et al., 2010). Recently, an impact event at 4.21 $\pm$ 0.13 Ga was proposed for lunar melt breccia 67935 based on ^{187}Re-^{187}Os isochron data (Fischer-Gödde and Becker, 2012), but the Re-Os isochron approach does not appear generally applicable to dating lunar impact melt rocks (Fischer-Gödde and Becker, 2012) and the geochronological significance of that result has not been independently verified. Fernandes et al. (2013) reported ^{40}Ar-^{39}Ar plateau ages of 4.21 $\pm$ 0.05 Ga and 4.29 $\pm$ 0.04 Ga for two melt breccia fragments extracted from regolith collected at the rim of North Ray crater. Based on the petrography of these samples (Fernandes et al., 2013), they appear to represent relatively small impact events compared to 67955."

And

"The ages of lunar zircons and metamorphosed breccias also suggest that multiple large impacts occurred on the Moon at around that time, but the sizes of those events is difficult to establish."

Note we go out of our way to specifically NOT ascribe various 4.1-4.2 Ga ages to basin-forming events.

Paragraph 1546: With respect to the author, in my opinion this manuscript has the potential to contribute further to the confusion through its insistence that the TC and the LHB are and always have been identical in concept and definition. As shown above, the literature does not support this view, which is actually a re-statement of Ryder's 2002 Strong Version. I do agree that many papers in the 2010-2012 window were using the term LHB (incorrectly) in this restricted sense, but disagree that Bottke and others who studies the Archean spherule layers contributed significantly to further confusion by arguing that largish impacts continued on the Earth well after Orientale. In retrospect, this is a reasonable argument based just on probability and it reflect more poorly on earlier work that this possibility was not recognised sooner.

Line 1554: As noted several times above, equating Wetherill's use of the LHB and Tera's use of the term Terminal Cataclysm is not an accurate reading of the literature. To paraphrase Ross Taylor, whether a few basins over 200 m.y. constitutes a 'cataclysm' or not is just an argument over words. Certainly even just one Imbrium-size event on Earth may have been viewed as a cataclysmic catastrophe by anything living in the target zone. In any case I would be happy to exclude Ryder's Strong Version, as we have said in print several times now so this should not be presented here as a new conclusion, but without firm ages on any basin other than (perhaps) Imbrium and Orientale, we just cannot be sure what actually happened (see Bogard quotes above).

Line 1815: equating the TC and the LHB is inconsistent with Levison's quote just following because he refers only to the "lunar basins with known dates". In even the most generous reading of this, e.g., including Nectaris, that encompasses only about 1/3 of the lunar basins. However, the following quote from Bottke very clearly demonstrates the complicity of the dynamicists in re-defining the LHB as = TC, which in my opinion is a scholarly injustice to the original definitions of these terms. That's why Bottke & Norman separated the LHB from the TC, although Bottke has now fallen back into his old ways of equating the two concepts.

Line 1821: Note that the Norman and Nemchin 2012 abstract cited in the ms was at one of the earlier Bombardment workshops, but my LPSC abstract on 67955 was out in 2007, our 2014 EPSL paper had already been published as had Morbidelli's 2012 EPSL paper on the sawtooth bombardment as well as Fernandes et al. 2013 MAPS at the time of the 2015 workshop. So I think that referring to the 2015 workshop as 'revisionist' is a little behind the timeline. The wave was already in motion by at least 2012.

Line 1895: The manuscript again explicitly equates the cataclysm with the LHB.

Section beginning at line 1908: The modelling of Michael et al. (2018 Icarus) on destruction of older impact melt rocks could be referred to here.

Line 2066: This argument continues to ignore the preservation of primary crustal rocks with ages of ~4.3 Ga and that formed in the uppermost 1 km of the crust. E.g., McCallum and O'Brien 1996 AmMin document cooling depths of 0.2-0.5 km for gabbronorites from Apollo 17, a qtz monzidiorite from A15, and a sodic ferrogabbro from Apollo 16. There is also the 4.4 Ga ferroan anorthosite clast from 67215 with an age of 4.44 Ga and a cooling depth of <1 km (Norman et al., 2003), and aluminous basalts from A14 with ages of ~4.3 Ga (Neal and Kramer 2006 Am Min). The manuscript needs to address this apparent contradiction between the preservation of impact melts vs. primary igneous rocks that also formed in the uppermost crust. Are all of these existing samples irrelevant and the only relevant ones are the older melt rocks that do not exist? An alternative explanation might be that impact melts were relative scarce in the uppermost crust prior to about 4.3 Ga. The preservation of primary basaltic crust with an age of 4.4-4.5 Ga on Vesta also seems inconsistent with such a scenario.

Line 2169: As shown above, this assertion that Wetherill 's definition of the LHB and Tera's definition of the TC were not the same, so this assertion is in correct. Wetherill defined the cluster of impact melt rock ages at 3.9 Ga as the END of the LHB marked by a sharp drop in the number of basin-forming impactors hitting the Moon. He NEVER defined the LHB as a spike and in fact the literature cited above clearly shows just the opposite, that he adopted a declining bombardment similar to Hartmann's as the baseline for his models, while acknowledging that the flux could have been episodic to accommodate Tera et al.. In my opinion, the manuscript seriously mis-states the literature on this particular topic.

Line 2177: While there have been two reports of melt rock ages of 4.33 Ga (Grange et al 2009 GCA zircon) and 4.22 Ga (Norman and Nemchin 2014 EPSL), I am not sure this can be characterised as "an increasing number of reports".