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COMMENT

Comment concerning the review by W. McKinnon and R. Korotev of P. D. Spudis' book, *The Geology of Multi-Ring Basins*

PAUL D. SPUDIS
Lunar and Planetary Institute, Houston, TX 77058-1113, USA

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The review of my book *The Geology of Multi-Ring Basins* (October 1994 issue of *Geochimica et Cosmochimica Acta* by William B. McKinnon and Randy L. Korotev) contains many factual errors, misrepresentations, and misunderstandings. I also detect within the review the expression of an attitude that I will charitably call a misconception: that is, that planetary photogeology is not a rigorous scientific discipline, but a field in which anyone is qualified to express an opinion, regardless of training, experience, and competence.

The review has two parts. The first part, presumably by McKinnon, is dominantly an extended criticism of the mapping of basin rings from photographs and the analysis of ring diameter measurements to empirically derive a general ring spacing relation for the planets. The review insinuates that because I choose to focus most (not all) of my research efforts in planetary photogeology, any conclusions I reach are suspect because photogeology is "a field that is sometimes dangerously unconstrained," an amusing observation coming from a physicist who models planetary features and processes that, presumably, are constrained in large part by planetary image data. Rather than documenting this dubious assertion, McKinnon claims expertise in photogeology and that he maps and interprets only those features that are "obvious, if not convincing." Obvious to whom? Convincing in what manner? The process by which the ancient multi-ring basins on the Moon were first recognized and then carefully reconstructed is discussed by Hartmann (1981). Degraded planetary surface features are difficult to delineate, particularly those that occur at regional to global scales, and long hours of careful mapping are required to elucidate these features. Planetary geologists are well aware of the dangers of merely drawing 'connecting' circles to terrain elements. Degraded basins are not mapped and unquestioningly accepted, but require a variety of corroborating evidence, both direct and indirect, to establish their existence, including the presence of structural and topographic lows, the infilling by mare (lava plains) deposits, circular ridges, and arcuate scarps that outline basin structure, gravity anomalies, and regional patterns of structural deformation. Few basins on any planet possess coverage by all of these diverse sources of information and thus, the relative certainty of basin presence and extent varies widely.

Although photogeology provides virtually all of the empirical constraints for large craters and basins used by impact theorists, an unsettling disdain for this technique pervades the review. For example, McKinnon questions the many multi-ring basins that I show on Mercury, partly on the grounds that the hemisphere mapped by Mariner 10 was seen only at resolutions comparable to Earth-based telescopic photos of the Moon. Is he truly unaware that, as regional features, basins are *easier* to detect in low to moderate resolution images? Virtually all the near side basins of the Moon were mapped and recognized (by Ralph Baldwin, Bill Hartmann, and many others) from telescopic lunar pictures, before the first probe ever returned lunar images. The review discusses the dangers of planetary scientists "fooling themselves" into believing in nonexistent structures and implies that I have invented many of these features. Of the 98 basins inventoried on the Moon, Mars, and Mercury in the book, I claim personal discovery of only 11; all of the others were first recognized by many different workers over three decades of research and dozens of papers (all carefully documented in the bibliography). It would appear that the ring mapping 'disease' is near epidemic.

The review is particularly fervent in regard to my discussion of basin ring spacing and my acceptance of the (in)famous "square-root of 2" spacing rule.* The review allows as how this rule might be "approximately true" for the lunar Orientale basin, but is not true for any other basin. I am criticized for "glossing over" the "manifestly obvious" rings of basins on Ganymede and Callisto; in fact, I *specifically* point out the obvious fact that the Asgard and Valhalla basins on Callisto do *not* follow the $\sqrt{2}$ rule (p. 217, emphasis as in the original). McKinnon claims that pristine double-ring basins, particularly on Mercury and Venus, do not follow the spacing rule. This is incorrect: as carefully documented in Pike and Spudis (1987), double-ring basins follow the same ring spacing (a 2:1 ratio, rim to peak ring) as do the main rim and peak ring equivalents (Cordillera and Inner Rook rings at Orientale) in multi-ring basins. The 2:1 spacing of double-ring basins is merely a *subset* of the general $\sqrt{2}$ spacing rule. The claim that these relations do not hold on Venus is con-

* Briefly, this "rule" states that basin rings are found at positions corresponding to multiples of integer powers of the square root of 2 ($N = (\sqrt{2})^n D$, where N is the diameter of any basin ring, D is the diameter of the basin topographic rim, and n is an integer). This rule, its history, and documentation of its applicability on the terrestrial planets is discussed at length in Pike and Spudis (1987).

traindicated by the work of the Magellan Science Team (Schaber et al., 1992), who found that both double-ring and multi-ring basins on Venus follow the same $\sqrt{2}$ spacing as basins on all of the other terrestrial planets. Perhaps the vigor of the review's denunciation of this (actually relatively minor) point in my book is related to the fact, the 'square-root of 2' spacing rule, if correct, causes difficulty for the reviewer's preferred mode of basin ring origin (cf. Melosh and McKinnon, 1977, and my discussion, Chap. 8, p. 180–188). An increasing weight of evidence supports my position on the existence of 'obscure rings' and the $\sqrt{2}$ spacing rule, including new data for the terrestrial Chicxulub impact basin (Sharpton et al., 1993), multi-ring basins on Venus (Schaber et al., 1992), and altimetry data from Clementine that depicts basin rings on the Moon as mapped previously on the basis of photogeology alone (Zuber et al., 1994; Spudis et al., 1994). This simple fact may be responsible at least in part for the vehemence of their denunciation.

The part of the review, presumably by Korotev, on lunar sample aspects of the book is a combination of a red herring, an outright misrepresentation, and a difference of opinion, in which apparently Korotev believes that some issues in sample science are settled facts, whereas in reality, few questions in lunar science are completely closed. The red herring is a passionate condemnation of the use of mixing models of chemical data. I am well aware of the limitations of this technique and in fact, point out (Chapter 1, p. 14) that mixing models do not indicate the rock type abundance on the Moon, but are merely *tools* for the visualization of chemical data—a handy technique for geoscientists more comfortable with rocks than elements. As the review points out, one could indeed tell from a glance at the chemical data that anorthosite is rare in the ejecta from the Serenitatis basin, but more subtle differences, such as those between Orientale and Crisium ejecta, for example, are not so apparent. The review discusses the nonuniqueness of mixing models at length, hardly a productive use of publication space.

The misrepresentation occurs in relation to my treatment of sample 68415, an aluminous impact melt collected at the Apollo 16 landing site. Korotev cites my discussion of this sample as an example of my "lack of attention to detail." He claims that in the chapter on the Orientale basin, I propose this sample as a piece of Orientale ejecta, while in the chapter on Nectaris, where I discuss Apollo 16 samples, I advocate a local origin for the melt group of which 68415 is a member. No one can eliminate conclusively an Orientale basin origin for this rock; the formation of millions of large craters on the Moon ensures that exotic samples from anywhere *could* be found at any site. However, in the Orientale chapter of my book (p. 63), I reject an Orientale basin origin for 68415, giving my *preference* for an origin within the melt sheet of a local crater at the Apollo 16 site. I brought up this issue only because such an origin has been advocated for this rock (Chao et al., 1975). I can only conclude that Korotev did not read this section completely.

Korotev's final complaint is that I make only a cursory examination of the data for samples of the Moon. I'll allow that it is not possible to do full justice to lunar sample science in a book of only 300 pages, especially a book not totally devoted to the Moon. Korotev implies that he has tried to 'educate' me on several occasions regarding the nature of the impact melt problem, but I perceive that his real problem is that I have not accepted *his* view of the origin of certain Apollo impact melt breccias. For example, Korotev believes that minute differences in the concentration of certain elements prove that Apollo 16 impact melts must be from multiple, local impact craters, whereas I believe that such differences can be understood and accommodated by their formation as the result of one or two very large (basin-forming) impacts. In contrast to Korotev's assertion that "no discussion or reference to such problems occurs in the book," I discuss the impact melts at the Apollo highland sites in five chapters, the impact melts of the Luna 20 mission in the chapter on Crisium, and the general problem of impact melting in chapter 8, including a lengthy discussion of the issue of melt heterogeneity (pp. 172–177). There is *not* unanimity of opinion on this issue within the community and it remains an area of active research. Apparently, these are sections that Korotev also did not read.

I do not think it critical that the reviewers believe or disbelieve any given photogeological study, but they ill serve the readership of GCA when they imply that I have somehow 'invented' features and skewed the referencing to myself and to my colleagues. Virtually all of the lunar and martian basins and most of the basins on Mercury and the icy satellites I discuss in the book were described first by other workers (all duly referenced). I attempted to credit all previous studies, but regrettably, in a field as rapidly changing as ours and with the long lag times of book production (2 years in this case), my reference list is indeed a little behind the literature. Even so, I contend that the bibliography of my book is the most comprehensive collection of references to studies of multiring impact basins in print. One of the review's off-center criticisms is that there are too many references to abstracts, gray literature, and my own work. Nearly all of the so-called gray literature and abstracts that I make reference to are those of the annual Lunar and Planetary Science Conference and a variety of associated topical conferences and workshops. This conference is the premier meeting of our profession, the source of many important first-order results, and both its proceedings and abstracts are available in virtually all university libraries. I do not cite my own work to corroborate any point that I make in the text, but do so merely to lead the reader to a more detailed exposition of a given case in point, a common and unavoidable practice in space-constrained review papers and books.

The funniest comment in the review states that I make many references to "a certain journal notorious for its crank papers." I must admit that this assertion puzzled me at first, so I decided to examine the reference list of my book. After pursuing the list and considering the quality of this book review, perhaps I have found the journal that they were talking about: the eleven references to *Geochimica et Cosmochimica Acta.*

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