UTILIZING MARTIAN CRATERS TO DERIVE CHRONOLOGIC INFORMATION. W. K. Hartmann, Planetary Science Institute, 1700 E. Ft. Lowell Rd., Ste 106, Tucson, AZ 85719-2395 USA, hartmann@psi.edu .

Various criticisms have been aimed at cratering chronology studies in general, including mine. Many of the critiques misstate the methods and/or do not apply to my isochron system. The technique appears more reliable than many critics imply.

Malin and Edgett [1] stated that "...it is impossible to date Martian surfaces from impact craters...given the problems of burial and exhumation," and that [2] a Mars with young volcanism "is not the planet we think we see...." This ignores that the present techniques not only made a correct pre-Apollo 1965 prediction of "about 3.6" Gy [3] for typical lunar mare ages, but also correctly predicted from Mariner 9 data in the 1970s that widespread areas of Martian lavas are only a few hundred My old [4,5]. This was confirmed in the 1980s-2000s by basaltic Martian meteorites (MMs) from all but one of 5 to 9 MM source regions on Mars. This critique does not suggest any alternative chronology, nor does it note that the technique can be used to give rough dates for exhumation or erosion events, from the numbers of fresh craters on relevant surfaces [6]. It was for this just reason that, building on work of Öpik [7], I defined "crater retention ages" (CREs) in 1966, to distinguish between actual age of rock unit formation and the survival or retention time of craters of specified size on such a surface, under the influences of erosion, exhumation, deposition, etc. [8]

McEwen et al. [9,10], Bierhaus [11], Chapman [12], and McEwen and Bierhaus [13] (usually in their first few sentences) have all stated that crater count methods depend on an assumption that small craters are primary impact craters, suggesting that any failure of this assumption destroys the whole method. In first-order terms this is incorrect as applied to my method (there are second-order issues, e.g., treatment of impact velocity, which vary with ratio of secondary/primary origin). As stated in my lunar papers, "I have avoided, so far as possible, dividing craters by supposed modes of origin..." (1967 [14]), and "The craters smaller than 2.8 km diameter...are probably a mixture of primary, secondary, and endogenic craters" (1970 [15]).

During the 1960s Ranger program, I determined the size-frequency distribution (SFD) of the total mix of all apparently randomly-scattered impact craters in lunar maria (excluding obvious clusters of secondaries). McEwen and Bierhaus [13] describe a "40-year controversy" during which Shoemaker's theory of secondaries was allegedly abandoned by most workers, who assumed small craters were primaries. However, precisely because of the concerns about the numbers and clustering of secondaries, as stated in my papers, I limited dating procedures to craters of D ≥2 km [15,16]. When Mars Global Surveyor pioneered studies of Martian crater SFDs down to D = 10 m, our first team paper on this subject [17] (Hartmann et al., including McEwen) stated "...our procedure is to count all craters but avoid areas with obvious clusters of small secondary ejecta craters." Contrary to repeated assertions, our method was to count the total of primaries + distant or "field" secondaries, and is not limited by assuming "primaries only."

An example of compounded misinterpretation comes from [10], where the authors read numbers from my isochrons and interpret them as primary craters, then stating that the "HPF" (Hartmann production function) has a "disparity" of a factor ">2000". Inconsistent with this comment, they then make a new estimate of the age of Athabasca Vallis ("between 1.5 and 200 My") that is virtually identical to that published three years earlier by Berman and Hartmann [18], but not referenced ("a few megayears or less" to "≤200 Myr"). Nonetheless, the statement that our "production function" "overpredict(s)" primary crater densities is repeated in [13], in spite of the fact that our isochrons do not attempt to predict primary crater densities, and (correctly interpreted) are consistent with their interpretations.

References [10] and [11] emphasize that since distant secondaries are statistically clustered in time and (to lesser extent) space, they can carry no useful chronologic information. Three lines of evidence are more positive. First, the statistics cited in [10, Table 3], indicate that after about 10-20 My, a Zunil-sized crater will broadcast small distant secondaries over most of Mars; my isochron for 10-20 My, instead of being off by ~2000, roughly matches their predicted SFD (which, incidentally, is too steep). Their data and my data agree that surfaces with virtually no 20-m craters are probably < few My old, and surfaces saturated with such craters are probably ≥100 My old (see Fig. 1). Second, Malin and Edgett (www.msss.com) proposed detection of a new small crater on Mars since the Viking period and calculated a crater production rate at 25 m ≤ D ≤ 100 m; that rate matches my isochrons within a factor of about 4 (see Fig. 1). Third, small, young lava flows [6] and landslides [19] nearly always show lower small-crater densities than underlying units, proving that small-crater densities do retain chronologic information and are not dominated by random clustering.

Plescia, in 2005 [20], argued that few if any sites on Mars show the predicted size distributions or isochron shapes. However, his comparison was with my 1999 "first iteration" of the isochrons. My "2004 iteration" [6] shows a much better fit between the isochron shapes and SFDs on young, pristine plains of Mars.

In summary, our current isochron system, while subject to improvement, may be stronger than portrayed. It has predicted ages that match MM ages; fits observed SFDs in pristine areas; matches predictions of the McEwen et al Zunil work; matches Malin-Edgett production rates for very small craters, and fits observations on small stratigraphic units such as landslides.

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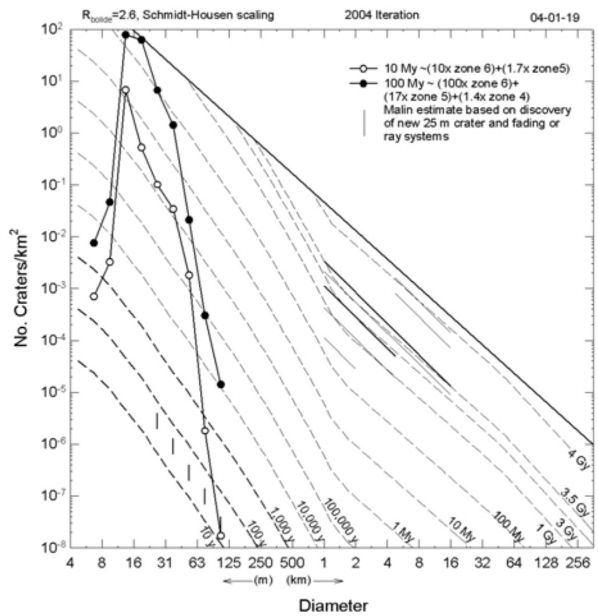

Figure 1. Progress in studies of small craters: two new approaches to understanding accumulation of very small craters, plotted on my latest ("2004") isochron iterations [6]. First, the two upper curves show estimates of the total accumulation of secondaries from ten Zunil-sized craters over ~10 My (open circles), and from 100 Zunil-sized craters of ~100 My (solid circles), all based on data in Table 3 of McEwen et al. [10]. The inference is that predicted numbers of secondaries after some tens or 100 My begin to approach the craters numbers actually observed (see text), and that small craters do contain chronologic information. Second, the tick marks in lower left show the estimate of 20-100 m crater production by Malin (www.msss.com), distributed into our diameter bins. Again, the estimate is close to our isochron for 100 years (see text).