Discussion of

Venus' coronae: impacts, plumes, or other origin?

by

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3rd December 2006, Warren B. Hamilton

Thousands of old mostly-circular structures with impact-compatible morphology, and rim diameters from 3 to 2500 km, saturate large tracts of both uplands and lowlands of Venus. Most of these old structures are ignored in conventional work, including that by Jurdy and Stoddard (this volume), and the small fraction that are considered at all are deemed endogenic and young. The well-preserved structures typically are circular-rimmed shallow basins with gentle debris aprons, often with gentle circular moats, and often with internal central peaks or peak rings. Basins are superimposed with the cookie-cutter bites required by impacts but incompatible with endogenic origins. Doublets and composite shapes attest to disruption of many bolides by gravity and dense atmosphere. Superpositions and analogy with the dated youngest large lunar impact basin indicate most of these structures to be older than 3.85 Ga if they record impacts: the venusian landscape is relict from late-stage planetary accretion. See Hamilton (2005, this volume) for evidence, illustrations, and discussions. Superimposed on the old structures are ~1000 small craters, rim diameters of all but a few <100 km, their scarceness reflecting atmospheric destruction of most small bolides, acknowledged by all as impact structures and very conservatively designated.

Jurdy and Stoddard (this volume) minimally address these matters in their presentation of numerical arguments for the conventional assumption that the old circular structures formed during the last billion years by endogenic processes. From the thousands of these structures, they consider only a small, arbitrary subset, particularly conspicuous in radar backscatter imagery of uplands, of "coronae." These mostly are circular rimmed depressions with rimcrest diameters between ~70 and 500 km. They include less than half of the large quasicircular structures, and fragments thereof, visible in uplands; perhaps one-twentieth of the large variably-filled structures, seen primarily in radar altimetry, of the twice-as-extensive lowlands; and none of the several thousand additional small old circular structures in both uplands and lowlands. Jurdy and Stoddard (this volume) select "coronae" by arbitrary criteria that limit them mostly to uplands, hence err in reasoning that uplands and circular structures must be products of plumes and diapirs. Further, they wrongly deny the existence of structures transitional between the ~1000 small young impact structures and their selected structures, and of old, small structures; see my papers for illustrations of both.

Jurdy and Stoddard (this volume) depict coronae as elongate and aligned within rift zones, and therefore endogenic. The conflict between these conjectural elongate structures and the actual mostly-circular ones is clarified by their Figure 4. Their “coronae” are elongate blobs drawn arbitrarily outside those structures most obvious in low-resolution reflectivity, or even drawn to enclose several such structures each. Their analysis of these blobs is irrelevant. They dismiss, without discussion, the circular and composite structures actually at issue as “calderic depressions,” despite lack of dimensional and geometric similarity to any modern terrestrial magmatic features.

Jurdy and Stoddard (this volume) present stacked radial topographic profiles through five young impact craters accepted by all, and through six coronae. They acknowledge that four (I say five) of these six coronae have impact-compatible topography, but argue that because five of the six profiles have lower circular symmetry than do those of the accepted impact structures, all but one are endogenic. (The one they accept as an impact is indistinguishable visually from hundreds of other “coronae.”) The perceived distinction reflects flawed methodology, and the greater age and modification of coronae. The Magellan Stereo Toolkit software, presumed source of the profiles, adds artifacts and digital noise, which cannot be interactively edited, where radar-brightness contrast is low and features are large, the common case with old structures (C.G. Cochrane, 2005, and written communications, 2006). Jurdy and Stoddard (this volume) magnify these defects with vertical exaggerations up to 160:1. The extreme exaggerations of their Figures 3 and 8 negate conclusions derived from those also.

The Jurdy and Stoddard (this volume) statements regarding distribution of accepted “young” impact craters are based on an early tabulation that undercounted craters in bedrock terrains. Their confident statements regarding ages and origins of surfaces, landforms, and coronae are merely intertwined assumptions that are false if the structures at issue record impacts. Their deductions regarding geoid and uplands are false if uplands include impact-melt constructs, as I advocate. Their references to my work and predictions are mis-citations.

22nd December 2006, Claudio Vita-Finzi

To the outsider it must seem that we are going round in circles, or perhaps ovoids. For example, Jurdy and Stoddard suggest that interpreting coronae as impact craters would result in a crater density for the BAT region of Venus indicative of a 'somewhat older than average surface age' whereas they know it is relatively young because it lacks impact craters and boasts many rifts which are also poor in impact craters. Such arguments cut no ice with those who draw no distinction between coronae and craters. Even the statistical dialogue is not conducted on a single wavelength: measures of circularity are neither here nor there if you have decided that craters may be deformed by erosion or tectonics.

Perhaps we should move on to more productive matters. Three come immediately to mind. First, we need to know the varying atmospheric and geological conditions under which Venusian impact craters formed, and this we can do only by dating them using criteria other than crater density. Second, the isotopic evidence points to a wetter past. When was it, how wet was it, and did impact history have anything to do with its demise? Third, if some, several or all the coronae originated in plumes or diapirs, can we construct a model for the interior of Venus that can support them simultaneously or serially? But the first step is surely to follow W. Hamilton in looking at all quasi-circular structures on Venus and not just those we call coronae, whereupon – witness the fine image by S W Tapper in Vita-Finzi et al. (2003), Figure 11, p. 821 (reproduced here as Figure 1) – we find a landscape as lunar as the Moon's.

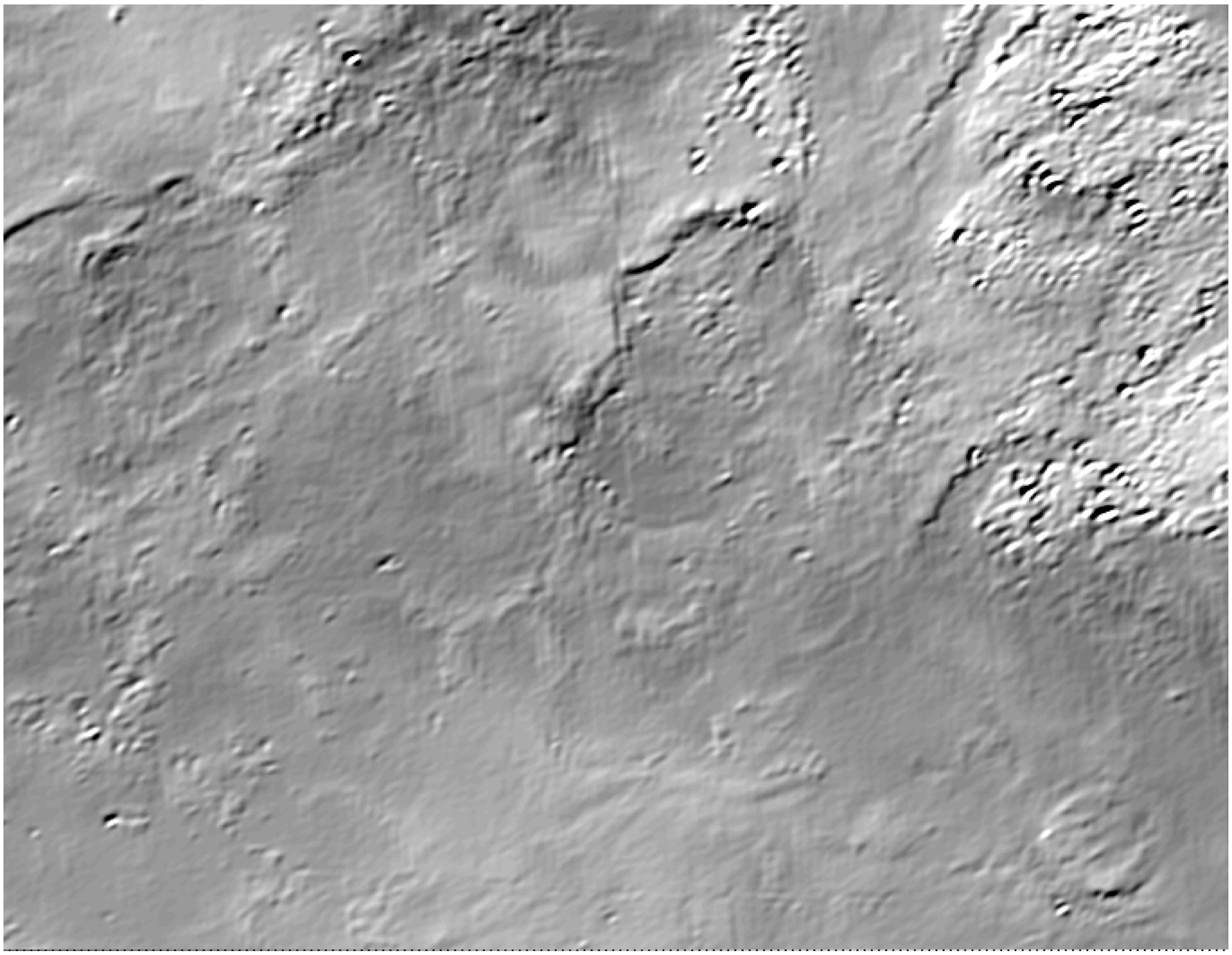

Figure 1. Shaded relief image generated from Magellan altimetry of part of Scarpellini quadrangle illustrates subtle multicratered topography. The crater at bottom right has a diameter of ~ 200 km.

27th December 2006, Richard J. Howarth

At the end of the 1st paragraph of their section on testing the impact hypothesis, Jurdy & Stoddard's quotation from Fisher is outdated – the approximation to a Normal distribution is quite inadequate for small samples. The authors don't even say what their “count” of 4 is supposed to be – it must be the number of occurrences in so-many sampled units. Even if these are implicit, and they ought to be explicit, *e.g.,* in *n* cells of X by X km on the surface of Venus you only find 4 instances, etc. (See Howarth 1998 for a survey of recent results on this topic),

Jurdy & Stoddard talk about a "random" distribution without specifying what they mean by it. In fact they are almost certainly talking about a Poisson distribution. In the second paragraph of their section on circular symmetry, they write of "profiles cross-correlated at only 25-30% of perfect cross-correlation." This kind of statement is inadequate. The sample size (i.e. number of data points), the type of metric used to assess the "cross-correlation", and the results of a statistical test of significance, should all be given.

Even had these topics been adequately addressed, Jurdy & Stoddard's statistical assertions seem to me to add little weight to the key arguments, which are essentially geological, and have been addressed *e.g.*, by Vita-Finzi et al. (2005).

9th January 2007, Ellen R. Stofan

Jurdy and Stoddard (this volume) describe coronae, a feature type that has been recognized in the peer-reviewed literature since their first identification in Venera radar images of the Venus surface (e.g., Basilevsky et al., 1986; Barsukov et al., 1986; Pronin and Stofan, 1990; Stofan et al., 1991; Head et al., 1992; Squyres et al., 1992; Janes et al., 1992; Namiki and Solomon, 1994; Stefanick and Jurdy, 1996; Hansen, 2003; Johnson and Richards, 2003; Hoogenboom et al., 2004). The majority of the community working with Venus data clearly differentiate between impact craters and coronae, as each has distinct morphologies and distributions. Jurdy and Stoddard (this volume) provide a clear and concise overview of the characteristics of coronae, and why an impact origin is not consistent with these characteristics.

Their paper provides clear evidence as to why their origin is related to endogenic processes. Endogenic processes are capable of producing depressions, and several models have been discussed in the peer-reviewed literature that are consistent with the observed range of corona topography (< -1.0 km - > 2.0 km) (e.g., Koch and Manga, 1996; Smrekar and Stofan, 1997). The impact cratering process, on the other hand, dominantly produces highly circular depressions. Clearly some coronae are depressions and some coronae are circular, but it is important to understand that both exogenic and endogenic processes can produce circular depressions, and to focus on the evidence that allows one to determine which cause is more likely. As Jurdy and Stoddard (this volume) conclude, the data clearly support an endogenic origin.

18th January 2007, Warren B. Hamilton

Just the place for a Snark! I have said it twice:

That alone should encourage the crew.
Just the place for a Snark! I have said it thrice:
What I tell you three times is true.
Lewis Carroll

Vita-Finzi et al. (2005) and I (Hamilton, 2005, and this volume) argue that the thousands of circular venusian structures, mostly rimmed depressions that reach giant sizes and that predate obvious small, young impact structures, are products of ancient impacts. Although early interpreters of low-resolution venusian radar imagery recognized this possibility, Ellen Stofan (e.g., Stofan et al., 1985) and a few others speculated, years before detailed imagery became available, that Venus is too earthlike to preserve a primordial surface, hence the circular structures must be young and endogenic. This conjecture soon became dogma, and the impact option has seldom been mentioned since. Broadly conflicting young-endogenic classifications and genetic rationales for some of the circular structures have been presented in hundreds of papers by Stofan and by many others (e.g., Jurdy and Stoddard, this volume), but the great majority of circular structures that are candidates for old impacts have been ignored by Stofan and most other venusian specialists. Many of these structures are huge and, if products of impacts, must, by analogy with dated lunar structures, be older than 3.8 Ga. The structures are mostly or entirely older than the small impact structures, as accepted by all observers, that conventionally have been assigned maximum ages of somewhere between 0.3 and 1.5 Ga on the basis of poorly constrained calculations of the effect on fragile bolides of the extremely dense venusian atmosphere; ages reach 3.8 Ga in my terms.

Few post-1988 mainline papers mention the impact option, and none have rigorously evaluated it, but Stofan here implies that because a number of papers, including five of her own and the paper in this volume by Jurdy and Stoddard, agree that the circular structures are endogenic, and because the manuscripts were peer-reviewed [by other specialists who also assume endogenic origins], endogenic origins should be accepted. That the authority-by-repetition thus appealed to is poorly supported by evidence is shown by Stofan's many papers, and that the implied consensus does not exist is shown by the mutual incompatibility of her evolving speculations with those by others including Jurdy and Stoddard (this volume).

Stofan's papers deal with various venusian features that she regards as products of endogenic magmatism, mostly intrusive, despite their complete dissimilarity to any modern terrestrial structures. Her papers have concentrated on 400 or so "coronae", an arbitrary subset of conspicuous midsize circular structures. She has presented conceptual and numerical models wherein assumed parameters and unearthly processes enable endogenic results, but the models do not explain the characteristic circularity of the rimmed depressions except as local coincidences. Stofan's statement here that "endogenic processes can produce circular depressions" is but wishful thinking with regard to the thousands of examples with rim diameters up to 2000 km. Her papers have not addressed the common impact-compatible morphology, whereas even the anti-impact Jurdy and Stoddard paper in this volume acknowledges that three of the six coronae whose morphology it considers in detail "appear crater-like" and that one of those three likely is an impact structure and the other two may be. The great majority of the old circular structures in the plains, which comprise two-thirds of Venus, are excluded from Stofan's studies, so her published statements about restricted distribution of coronae are invalid with regard to the broad population of structures at issue, and her global-dynamic speculations cantilevered from those statements also are invalid.

Stofan here notes, correctly, that "The impact cratering process … dominantly produces highly circular depressions", but then implies ("some coronae are circular and some coronae are depressions") that such structures are but a minor venusian type. The reader need look no further than the plains image in Claudio Vita-Finzi's preceding discussion to see that circular depressions are the rule. Stofan and Smrekar (2005) themselves stated that "the most typical shape for a corona is a depression or rimmed depression." The several papers by Stofan that mention the impact option dismiss it with arguments that I refuted in my papers as irrelevant because most of them (like most arguments in the Jurdy and Stoddard paper in this volume) address only a biased subset of the circular structures, chosen by criteria that limit their geographic and size distributions, and further deal not with the circular rimmed depressions at issue but with imagined boundaries far outside them or even outside groups of the structures.

10th February 2007, Donna M. Jurdy & Paul R. Stoddard

In his discussion, Hamilton criticizes our data set and definition of the outlines of the features we analyze as arbitrary. Both statements are inaccurate. As we stated in our paper, we used the data sets of Stofan et al. (1992), Magee Roberts and Head (1993), and Price and Suppe (1995), which in turn were not picked arbitrarily, but based on a defined set of observed topographic, structural, and volcanic features. We used outlines that were mapped by Price and Suppe (1995) based on volcanic flows related to the structures, as interpreted from the radar imagery. We documented our use of the subset of the classified corona catalogue that could be matched with those 669 distinct features mapped by Price and Suppe (1995) as "coronae," which they defined in their catalog as "circular to irregular volcanic-tectonic features characterized by an annulus of concentric deformation."

Hamilton suggests that if we were to pick the outlines of the features "correctly", we would find most to be circular to near-circular and cites our Figure 4 as evidence. Even if we were to accept this criticism, many of the internal structures in Figure 4 are still clearly non-circular. Dhorani and Ludjatako are clearly oblong, and Atete, Krumine (the northern part - as this is one that may have had "several such structures"), and Javine are all irregularly shaped. Others, such as Dilga, may be more circular, although Dilga was fairly round even as mapped by Price. Furthermore, our quantitative analysis assesses circularity, not based on the mapping by Price, but on Magellan topography. Finally, it must be pointed out, again, that circularity does not necessitate impact origin, but non-circularity requires a much more complex history, with many other implications, than a simple impact.

The remaining major concern Hamilton raises is the inappropriateness of the Magellan Stereo Toolkit software for features of the size we analyzed. We agree, which is why we did not use it for the study we report in this volume. (However, we did reference other studies that employed stereo-imaging; this may be where the confusion arises.) Stereo imaging for high-resolution topography can only be attempted for about 10% of Venus' surface with multiple radar coverage. For our topographic analysis we used the nearly-global Magellan altimetry data.

In his discussion, Claudio Vita-Finzi suggests that we engage in circular reasoning. While we admit much of our arguments are based on the circularity of craters and lack thereof of many coronae, we disagree that our BAT region argument is circular. The BAT region has many indications of being more active, and therefore younger, than the average age of the Venusian surface - uplifts with associated high geoid values, extensive rift systems, and the high concentration of tectonized, embayed craters, as well as volcanic activity - all independent of raw crater counts. Crater Uvaysi retains its parabolic halo, indicative of the most recent 10% of craters. The severe modification of this parabola-associated crater dates tectonic activity as having occurred recently. Furthermore, independent stratigraphic study of the BAT region to neighboring areas also shows it to be young (Basilevsky et al., 1997).

Vita-Finzi correctly points out that tectonics can change the shape of craters, and erosion can modify, to some degree, the circularity as measured in our paper. However, particularly for tectonized features, one would expect all features in a region to be elongated in more or less the same direction. Even with multiple tectonic events, the latest will be imprinted on all features. Analysis of corona orientation does not support this.

The correlation percentage referred to by Howarth is the standard cross correlation between the test and average profiles, divided by the autocorrelation of the average profile. The number of data points per profile was uniform for each feature, ranging from 41 for the smallest to 108 for the largest features.

Howarth has commented that our use of Fisher's description of sampling errors is out-dated. However Fisher's discussion of sampling errors in counting objects (Fisher, 1973l sect. 15, p. 57) is both clear and understandable, as well as being familiar to Earth scientists. It is, of course, based on assuming a Poisson distribution. We had noted the limitations of small sample size for some counts. We maintain that the statistical analysis of craters has utility in establishing of the relative ages of planetary surfaces.

Stofan in her discussion gives a historical perspective on studies of Venus' coronae. We thank her for comments and appreciate her insightful summary of past work.

None of the criticisms have seriously addressed our distribution arguments: Why are coronae, on average, less heavily cratered if they are indeed older features? Why do coronae tend to be more densely distributed in and near rift zones? When classifying coronae as craters, the BAT region with its surplus of coronae would then appear older than the average surface of Venus. What independent evidence exists for this?

We have presented a technique designed to quantitatively analyze features for crater-like morphology. Certainly this technique could be refined to consider better such factors as diameter/depth ratios, but we feel that even this rudimentary approach is much better than the eyeball method – with the associated subjectivity – for determining features' origins. Finally, we reiterate that only a complete, quantitative analysis of all mapped features identified as coronae would most conclusively address their origin.

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