

## Comment on "The global resurfacing of Venus" by R. G. Strom, G. G. Schaber, and D. D. Dawson

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### Background

The distribution of impact craters on Venus has been the subject of a great deal of analysis since the return of Magellan data. *Phillips et al.* [1992] performed Monte Carlo two-dimensional (2-D) modeling of the areal distribution of craters, and the results of that exercise allowed a restricted, but still quite large, range of possible planetary resurfacing histories, including the possibility that the craters were emplaced on a geologically inactive planet. However, the nonrandom distribution of embayed and deformed craters [*Phillips et al.*, 1992], the hypsometric distribution of craters [*Herrick and Phillips*, 1994], the varied degradation states of craters [*Izenberg et al.*, 1994], their nonrandom distribution with different geologic terrain types [*Namiki and Solomon*, 1994; *Price et al.*, 1994], and three-dimensional resurfacing modeling [*Bullock et al.*, 1993] all seem to argue against that particular possibility. In contrast, *Strom et al.* [1994] have collected a refined and more comprehensive data set of impact features, and they input these data into more sophisticated 2-D Monte Carlo modeling and statistical analyses of the areal distribution of craters, the hypsometric distribution of craters, and the number of embayed craters. They concluded that "Venus experienced a global resurfacing event about 300 m.y. ago followed by a dramatic reduction of volcanism and tectonism. This global resurfacing event ended abruptly (<10 m.y.). The present crater population has accumulated since then and remains largely intact . . . only about 4%-6% of the planet has been volcanically resurfaced since the global event . . ." If these conclusions are well-founded, this work certainly represents a significant advancement in restricting the number of plausible resurfacing histories for the planet. If *Strom et al.* [1994] are correct, it would also mean that all of the other aforementioned works are in error to various degrees, or at least represent overzealous interpretation of the data. However, we have identified apparent flaws in the observations, modeling, and interpretations presented by *Strom et al.* [1994] that lead us to question whether their conclusions are warranted. We limit our comments to three areas of their analysis: (1) Observations pertaining to the number and area of disrupted and pristine craters and crater-related features, (2) modeling of the areal and elevation distribution of craters, and (3) interpretations of resurfacing models.

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### Observations

In any modeling exercise perhaps the most valuable component is the data set being modeled. As input to their areal resurfacing models, *Strom et al.* [1994] count 932 craters (with ejecta blankets), 401 craterless splotches, and 58 parabolic features. Thirty-three of the craters are considered embayed, and none of the splotches and parabolic features is considered embayed by volcanism. Their resurfacing models use as an input constraint the area covered by unmodified crater-related features relative to the area covered by embayed crater-related features. Their figures seem to overestimate the number of unmodified features and underestimate the number of embayed features.

In their resurfacing modeling, the 401 craterless splotches are counted as part of the normal crater population. This inclusion requires that all of the splotches are formed by bolides, and that they are removed by exactly the same processes and in exactly the same way as craters. If the latter condition is not met, then the production rate of splotches must be known (at least relative to craters) before constraints can be placed on their resurfacing, and even then the splotches would have to be treated separately from the general crater population in a resurfacing model. There is insufficient evidence to conclude that all splotches are formed by bolides, there is compelling evidence that they are not removed in the same manner as craters, and their production rate is not well constrained. It is important to note that the only confirmed observation of a meteoroid exploding in a planet's atmosphere and visibly affecting its surface is the Tunguska event [*Turco et al.*, 1982]. The argument that splotches result from atmospheric explosion of meteoroids [*Phillips et al.*, 1991] is compelling, but the possibility that some of the splotches are of endogenic origin cannot be ruled out. Radar-dark volcanic materials from an unseen vent or simply a slightly different composition for a particular region of plains are possible alternative explanations. While most of the splotches have no obvious volcanic source, neither do most of the surrounding plains.

It is unlikely that splotches are removed in the same fashion as impact craters. *Strom et al.* [1994] neglect the possibility that weathering and/or wind erosion eliminates the splotches. Dark haloes around impact craters are similar in appearance to many of the splotches; they may be of similar origin and subject to similar removal processes [*Phillips et al.*, 1991]. Studies of dark haloes surrounding craters indicate that they are removed by both weathering processes and by volcanic processes [*Izenberg et al.*, 1994; *Phillips and Izenberg*, 1995]. Only about 40% of otherwise pristine-appearing impact craters have complete dark haloes around them. *Phillips and Izenberg* [1995] showed that

craters without accompanying dark haloes are prevalent (compared to the planetary average) in areas of very low and very high crater density, which they interpret as evidence that volcanic and weathering processes, respectively, are dominating halo removal in these regions. *Phillips et al.* [1994] found that twice as many embayed craters than would be expected with statistical independence have no dark haloes. Further, the remaining, noncoincident craters have a decidedly nonrandom spatial correlation. *Phillips et al.* [1994] proposed that low-level volcanism might operate in a region to remove the relatively thin dark haloes without an observable effect on the accompanying crater and its continuous ejecta blanket. In other words, if volcanism plays a role in removing dark haloes and splotches, then it must be much easier for volcanism to remove a splotch or halo (which has little topography) than an ejecta blanket or rim. Furthermore, around craters are the known places to search for degraded dark haloes; thus there is some hope that evidence for a partially embayed dark halo can be identified (e.g., lack of annular completeness of deposit). *Strom et al.* [1994] state that "Virtually none [of the craterless splotches] have been clearly embayed by lavas." They do not make the case that a partially embayed splotch could even be recognized as a splotch; indeed, they did not identify a single embayed splotch. It seems highly unlikely that 33 craters would be identifiable as embayed but no splotches if these two populations are resurfaced in the same manner.

The production rate of splotches cannot be derived from the crater production rate as *Strom et al.* [1994] have attempted to do. Their basic logic is that: (1) if we could see all the splotches, there would be about 1100 of them, making the total number of impact-produced features equal to 2000; (2) if Venus had no atmosphere there would be about 2000 craters > 8 km in diameter; (3) therefore splotches must be formed by meteoroids that would have formed 8-km (or greater) craters on an atmosphereless Venus, and we can estimate the number of such incoming meteoroids. This logic, however, could be valid only if it is already known that splotches are resurfaced in exactly the same manner as craters. The size-frequency distribution of these features bears little resemblance to that of the impact craters [*Strom et al.*, 1994, Figure 9]. To determine the production rate of craterless splotches requires knowledge of the relationship of bolide diameter to splotch diameter and the number of incoming bolides per year. The former must be completely based on highly speculative numerical models and the latter is based on a great deal of extrapolation from observations of Venus-crossing asteroids. In summary, the craterless splotches may not all be attributable to impact processes, and their production and elimination rates are so poorly constrained that it seems to be an intractable problem to use them in the type of modeling presented by *Strom et al.* [1994]. If the Venusian surface is all one age, then the 401 splotches formed over the same time period as the 932 craters and the production and elimination rates are known; however, the splotches cannot be included in a model that attempts to prove this point.

Inclusion of the splotches, all unembayed, added ~50% to the area of the planet covered by pristine impact features by *Strom et al.* [1994] with no increase in the area covered by embayed features. Aside from the craterless splotches, *Strom et al.* [1994] may have underrepresented the amount of volcanic embayment of the crater population. They cite 33 craters as having "some part of their rim materials embayed by lava flows." However, the remainder of the paper discusses these 33 craters as the only craters or crater-related features that have in any way been

affected by volcanism, and we question this assumption. In works we have been involved in [*Phillips et al.*, 1992; *Izenberg et al.*, 1994; *Herrick and Phillips*, 1994] ~60 craters have been identified which have either a rim breached by volcanism or obvious volcanic embayment of a substantial portion of the ejecta blanket, a number confirmed by others who have used our data in their own works [e.g., *Price and Suppe*, 1994; *Namiki and Solomon*, 1994]. This number is only approximate because, as with any data interpreted from the geologic record, there are differences of opinion reflected as discrepancies between databases maintained by two of the co-authors (R. H. and N. I.). More importantly, even this number is much less than the number of craters affected in less obvious ways by postimpact volcanism. One of us (N. I.) has used detailed examination to identify ~40 more craters with more subtle embayment of the ejecta blanket and crater-related features such as parabolas and dark haloes.

Combining high-resolution topographic data with imagery suggests that many craters have been volcanically modified in ways that are not obvious in the image data alone. *Sharpton* [1994] found that bright-floored craters are typically a few hundred meters deeper than similar-sized dark-floored craters. The strong correlation of bright-floored craters with parabolic features and radar-dark haloes indicates that they are young craters [*Campbell et al.*, 1992; *Herrick and Phillips*, 1994], leading to the conclusion that dark floors represent postemplacement volcanic filling of the floor [*Phillips et al.*, 1992; *Sharpton*, 1994]. These observations are not consistent with the notion that radar-dark floors result from radar-smooth cooled melt sheets in craters. If, for example, dark-floored craters were a result of impact into a particular terrain type, then bright-floored craters should not be found among dark-floored craters, as is observed. Or, if dark-floored craters resulted from some peculiar property of the incoming meteoroid, then there should be no correlation of bright-floored craters with parabolic features or dark haloes, as is observed. As another example of how subtle crater embayment can be, consider that Mead, the largest crater on the planet and a feature examined by dozens of researchers, was only revealed to be embayed after high-resolution stereo topography showed that a high-standing volcanic feature NW of the crater produced a flow that covered a low-standing part of the ejecta blanket [*Herrick and Sharpton*, 1995].

What are appropriate numbers to use for modeling of the resurfacing history of Venus? While every researcher (including the co-authors of this comment) will have different opinions for specific craters, some generalities can be made. The above discussion indicates that 30-60 of the ~900 craters on Venus are embayed to the point where a substantial portion of the ejecta is missing or the rim is breached. Thus, for modeling purposes, one can assume that a crater is substantially embayed if a lava flow is thick enough and extensive enough to cover part of the rim or come very close to it. It is extremely difficult to model or determine accurate numbers for "subtle" embayment; consequently, the area encompassed by dark haloes and parabolic features, in our opinion, should probably not be included in the type of models *Strom et al.* [1994] used. It is also our opinion that, currently, craterless splotches are too poorly understood to be included in this type of resurfacing modeling.

## Modeling

*Strom et al.* [1994] performed modeling exercises designed to reproduce the elevation and areal distribution of craters.

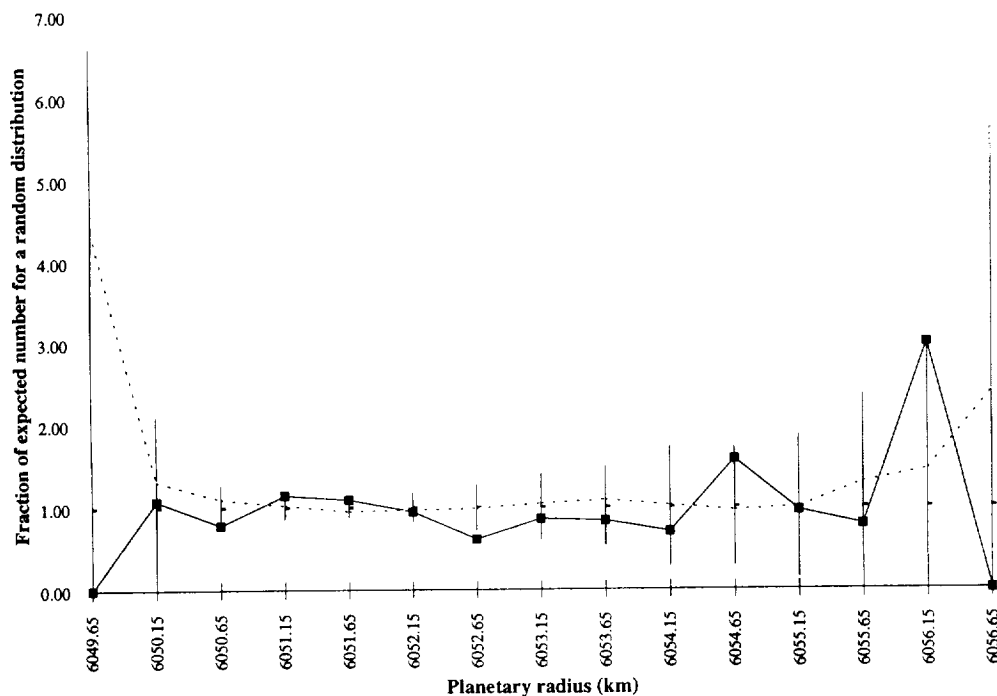
However, apparent flaws in their modeling techniques lead us to question whether they are truly superior to other approaches. For example, they have used a multinomial chi-square test to determine that the distribution of craters with elevation cannot be distinguished from a random population. However, to perform a chi-square test requires comparing the observed elevation distribution of craters with the expected elevation distribution of a random crater population. Yet they have used a histogram of the global topography as the expected elevation distribution. If the elevations of the craters are collected in a different manner than the data for the global topographic histogram, then *Strom et al.* [1994] are not comparing "apples with apples." For example, if the histogram of the topography was calculated by simply counting the number of pixels within each elevation band in the Global Topographic Data Record (GTDR), but the elevation of a crater was calculated by averaging the elevation of the surrounding terrain, then the two data sets could not be legitimately compared. We cannot assess whether this is a significant effect for the work of *Strom et al.* [1994] because they do not state how they obtained the elevation of a crater.

*Herrick and Phillips* [1994] produced a true expected elevation distribution by running 100 Monte Carlo simulations placing simulated craters randomly on the surface and then measuring their elevations using the same technique as that used for the observed distribution. Figure 1 shows data used in Figure 5b of *Herrick and Phillips* [1994] replotted with the addition of a plot of the topographic histogram. In this case had the topography been used as the mean of a set of random populations, the data would have appeared more nonrandom. *Herrick and Phillips* [1994] performed a chi-square test and determined that the observed distribution was nonrandom at a 97.5% confi-

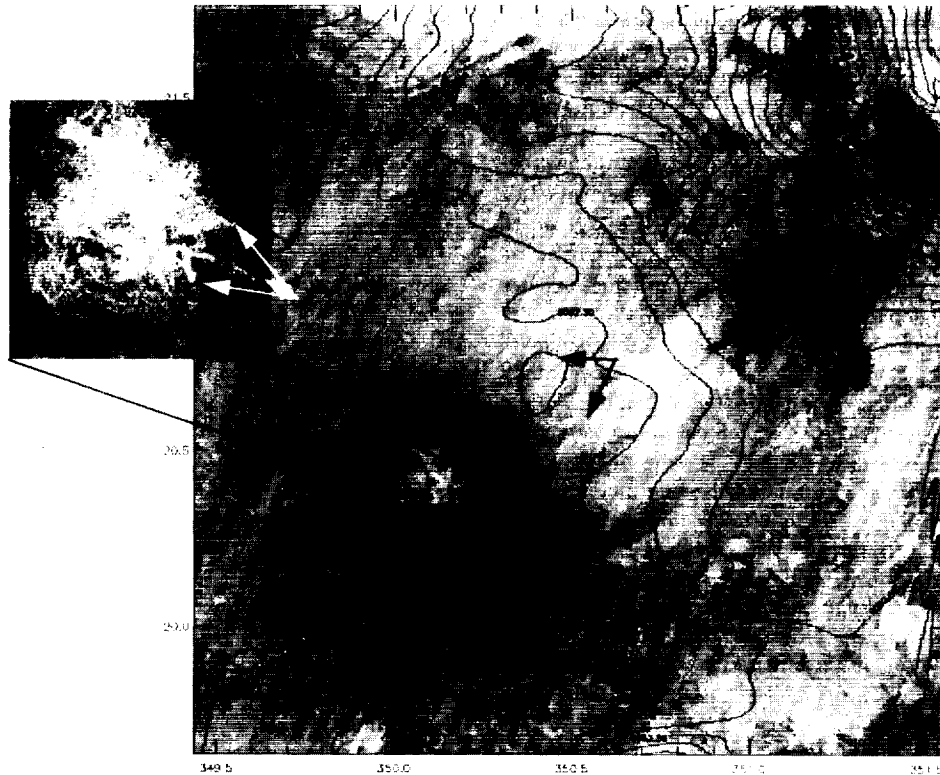
dence level. The *Herrick and Phillips* [1994] technique also allowed a standard deviation to be calculated for each elevation bin of the expected distribution. This allowed these authors to show that four 500-m bins of the observed distribution deviate from the expected distribution by  $>1\sigma$  and that one of these bins (6052.4- to 6052.9-km elevation) is  $>2\sigma$  from the expected distribution. Even Figure 3 of *Strom et al.* [1994] shows  $\sim 10\%$  excesses and deficits in elevation bins with 50-250 craters.

We also note that the crater elevation distribution becomes more random appearing if volcanic features are misclassified as craters, as the elevation range that volcanoes are predominant in is exactly the range *Herrick and Phillips* [1994] observed a crater deficit. *Herrick and Phillips* [1994] assigned a confidence level for each feature that might be an impact crater, and then created the elevation histogram using only those features that were classified as being of certain impact origin. Of the 932 craters *Strom et al.* [1994] used, we could not rule out volcanic origin for  $\sim 150$  features, and  $\sim 50$  additional features are almost certainly of volcanic origin. For example, the 8.5-km diameter feature at 20.4N, 350.1E (Figure 2) was included in the *Strom et al.* [1994] calculations but not in *Herrick and Phillips* [1994]. While an impact origin cannot be ruled out from radar imagery alone, this feature is located very near a local topographic high on the flanks of Sif Mons and appears to have volcanic flows emanating from it. Regardless, as we discuss later, the critical issue is not so much whether or not the elevation distribution of craters is barely within the range of possible random distributions, but instead how this observation is interpreted.

A large portion of *Strom et al.* [1994] is devoted to resurfacing models designed to test the circumstances under which the observed areal distribution of pristine and embayed



**Figure 1.** Histogram of observed craters (connected squares) with elevation compared with results from 100 Monte Carlo simulations of a random population (vertical line segments represent  $2\sigma$  bounds). Data are binned at 500 m increments and normalized so that the mean of the simulations equals unity. Dashed line shows histogram of topography for comparison. Because of different measurement techniques, topographic histogram does not exactly overlie the mean of the simulations.



**Figure 2.** A feature (20.4N, 350.1E) classified as an impact crater by *Strom et al.* [1994] which can alternatively be interpreted as a volcano. Image is 225 km x 225 km and located on the SW corner of Sif Mons (22N, 351E). Contour interval is 100 m. Feature is on the side of a relative high (~25 km from the crest, areal resolution of topography ~20 km), and the distortion of the contour line associated with the feature indicates it is elevated relative to its immediate surroundings. Black arrows point to features that appear to emanate from possible volcano. In inset, radar-bright material does not have the same texture as typical crater ejecta, particularly to the east of the two central depressions. White arrows point to features that may be flows down small valleys near the possible calderas.

craters is reproduced. As discussed above, their model appears flawed by overrepresentation of the area covered by pristine versus embayed craters. This overrepresentation dramatically increases the odds that lava flows in their models will embay more craters than they observed. Also, a starting assumption of all of their models is that resurfacing occurs in spatially random patches. However, *Strom et al.* [1994] clearly states that "Most of Venus' recent volcanism occurs in the Beta-Atla-Themis region," not in randomly placed patches. In fact, they criticized the end-member equilibrium resurfacing model of *Phillips et al.* [1992] for using randomly placed resurfacing events [*Strom et al.*, 1994, p. 10,912], and then used the same assumption themselves. What is the relevance of a set of models where one of the fundamental starting assumptions is admittedly incorrect? Such models are perhaps adequate to get a rough feel for possible resurfacing scenarios (as done by *Phillips et al.* [1992]), but it seems inappropriate to use them for placing rigid constraints on time and volume history of volcanism and tectonism on a planet. For example, consider a model where a 1000-km-wide equatorial band (8% of the Venusian surface) is continuously resurfaced (i.e., kept craterfree) while the remainder of the planet is not resurfaced. This model produces a statistically random crater population (using the statistical tests defined by *Phillips et al.* [1992]) with one embayed crater (one crater will fall on the edge of the band and be embayed using the *Strom et al.* [1994] criterion of an embayed crater). This model is no

more realistic than one with randomly placed resurfacing events, but it does illustrate how calculations regarding the percent of area resurfaced and the number of embayed craters change dramatically if resurfacing is allowed to occur in a spatially nonrandom fashion in the model.

Finally, we feel the criteria for whether a crater is embayed or destroyed are unrealistic. The *Strom et al.* [1994] model is 2-D and does not consider whether a flow is thick enough to cover the ejecta blanket or breach the rim, but instead considers whether the center of the crater is covered or what fraction of the surface area is covered by a circular lava flow on a flat (zero-relief) planet. Although more elaborate, the *Strom et al.* [1994] models seem no more realistic than those used two years earlier by *Phillips et al.* [1992]. While the 3-D modeling of *Bullock et al.* [1993] is not perfect, it does represent a substantial improvement in realism over existing 2-D resurfacing models.

### Interpretation

The philosophy used by *Strom et al.* [1994] in interpreting the cratering record on Venus is best summarized by the following quote from their paper:

Because impact craters on Venus cannot be distinguished from a random distribution, both spatially and hypsometrically, relative and absolute dating of local or regional terrains based solely on crater densities is statistically impossible. This is especially true due to stochastic effects in a random crater distribution of low density, as

discussed above. Thus the cratering record along with an appropriate cratering rate can be used only to estimate the average absolute age of the entire planetary surface. [Strom *et al.*, 1994, p. 10,903]

In other words, these authors imply that if distinct geologic units show statistically significant different crater retention ages (or crater modification states), this geological information should be ignored because the cratering record by itself appears to be random. At the heart of our difference of opinion with Strom *et al.* [1994] is our belief that if there is compelling geologic evidence supporting the observation of a crater-deficient (or otherwise anomalous) region, then this crater-deficient region is probably not just a stochastic effect.

It is not terribly important whether the distribution of craters with elevation is just inside or just outside a particular measure of randomness. What is important is how this distribution is interpreted. Strom *et al.* [1994] interpret the result of the elevation distribution being just inside the possibility for random distributions as a supporting piece of evidence for a geologically static planet. We, however, think it is significant that an excess of craters exists in the elevation range dominated by plains, that a deficit exists in the elevation range dominated by volcanoes and coronae, and that the mean elevations of embayed and deformed craters are in the elevation range of the deficit [Herrick and Phillips, 1994]. Thus the geologic record suggests that the excesses and deficits are real in at least some places, and the deficits represent resurfacing of at least 15% of the planet (>30% for a constant resurfacing rate) over the time period that the cratering record was emplaced [Herrick and Phillips, 1994; Herrick, 1994].

Similar arguments are involved in the interpretation of the areal distribution of craters. If the interpretation of a statistically random areal distribution of craters is that all deficits and excesses of craters must be stochastic effects, then one does not even need the modeling of Strom *et al.* [1994] to conclude that the craters were emplaced on an essentially geologically static surface. If, however, the possibility exists that geological units of different ages could combine to produce an areal distribution of craters that cannot be distinguished from a spatially random population, then the geologic evidence suggests that a significant amount of resurfacing has taken place over time. Phillips *et al.* [1992] found that volcanically embayed and tectonically deformed craters occurred preferentially around areas with a low crater density, suggesting that these areas lacked craters as a result of tectonic and volcanic processes. Phillips *et al.* [1992], Izenberg *et al.* [1994], and Phillips and Izenberg [1995] observed correlated areal variations in crater floor deposits, crater surroundings, areal crater density, and modification of craters by volcanism and tectonism, suggesting that the surface of Venus exhibits a range of geologic ages. Namiki and Solomon [1994] found that the density of impact craters on large volcanoes is half the average density of the planet, requiring resurfacing of at least 20% of the planet's surface area. Price and Suppe [1994] also found significant differences in the areal density of craters for different geologic units on the surface.

In summary, it is our opinion that the modeling of Strom *et al.* [1994] is too flawed to be used as supporting evidence that there was a global resurfacing event that ended in less than 10 m.y. followed by resurfacing of only 4% - 6% of the planet. Their proposed geologic history of tessera formation, subsequent global volcanic flooding, and later limited resurfacing is actually quite similar to that proposed by Herrick [1993; 1994].

However, Strom *et al.* [1994] contend that the planet was frozen in place in a time span of less than 10 m.y. with only negligible changes to the landscape since that time, a geologically implausible scenario that is inconsistent with observations of the cratering record and its relationship to the surface geology.

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