# Forum

## Anomalous cold in the Pangaean tropics: REPLY

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We thank W. Hood et al. (2009) for their Comment on our *Geology* paper (Soreghan et al., 2008). We recognize that our hypothesis of episodic cold in the Pangaean tropics, which relies, in part, on our proposal of a late Paleozoic age and glacial origin for Unaweep Canyon, is radical. Hence, scrutiny of our data and reasoning is justified. Note that we addressed several of Hood's arguments about the canyon age in a separate Reply to a Discussion of Soreghan et al. (2007) (Soreghan et al., 2009; Hood, 2009).

Hood et al. (2009) question the age of diamictite found in an outcrop and core within Unaweep Canyon. We initially proposed a late Paleozoic age for the diamictite in outcrop on the basis of (1) late Paleozoic palynoflora, and (2) an exclusive Precambrian provenance, which contrasts with the presence of Paleozoic and Mesozoic palynoflora and Mesozoic clasts in an overlying (Quaternary) unit. Hood et al. argue that the palynoflora could be reworked. We agree, and acquired a core to further probe the age of sediment atop basement. Palynoflora from this core reveal recent pollen throughout, but late Paleozoic forms in the basal part of the core. Hood et al. asserted that strata ("…post-Oligocene lake beds…") overlying the diamictite in the core contain Paleozoic palynomorphs. To clarify, three palynologists have collectively examined ~45 core samples and found only Pliocene-Pleistocene forms, excepting four samples that yielded Paleozoic forms (C. Eble, 2004, personal commun.; J. O'Keefe, 2007, personal commun.; D. Willard, 2008, personal commun.). Those four samples all occur in the basal 31 m of the (320 m) core (Soreghan et al., 2007, 2009). Moreover, the appearance of late Paleozoic forms here overlaps with a shift to an exclusive Precambrian provenance, and the appearance of low paleomagnetic inclinations, which contrast with the mixed provenance and steep (modern) inclinations of overlying sediment. Hood et al. ignore our paleomagnetic inclination data, yet these data indicate that sediments of the basal core acquired a low inclination the last time that Colorado lay at low paleolatitudes—during late Paleozoic time.

Hood et al. (2009) suggest that Mesozoic strata should thicken into Unaweep Canyon if the canyon filled partially with Mesozoic strata. Such thickening is not needed if, as we hypothesize, Unaweep Canyon was buried by late Paleozoic time. Indeed, partial (>350 m) late Paleozoic burial is documented (Cater, 1955; Moore et al., 2008). Cater (1970, p. 78) recognized this: "After the [Uncompahgre] highland attained its maximum height and while the Cutler was being deposited, the highland began sinking…." Thickening of the Mesozoic strata is unnecessary if the canyon was infilled prior to the Mesozoic.

Hood et al. claim that there should be other late Paleozoic canyons that sourced other "fans" of the Uncompahgre, but that these fans show no evidence for canyons. This comment is odd: insufficient exposures of Precambrian basement near the proposed source areas preclude any investigations of preserved paleorelief. Testing for the possible presence of buried canyons awaits future geophysical analyses.

Hood et al. comment on the shape of Unaweep Canyon, beginning with the statement that our suggestion of a U shape is not indicative of a Permian age. We agree, and have not argued for age from shape. In noting Cole and Young's (1983) suggestion of a Quaternary glacial origin for Unaweep Canyon, Hood et al. (2009) state "These [glacial] features are sharp and well-defined, which is inconsistent with a late Paleozoic origin because the area was subjected to significant erosion prior to deposition of the Triassic-age Chinle Formation atop the Precambrian." Two aspects of this statement are puzzling. Firstly, they contradict Cole and Young (1983, p. 78), who stated "Most observers have noted the U shape of Unaweep Canyon, but few have attributed it to modification by glacial ice. Instead, other explanations have been popular, perhaps because the canyon *does not exhibit the sharp, fresh glacial features* seen in areas of Wisconsin alpine glaciation" (emphasis added). Are the features fresh, or subdued? Secondly, Hood et al. invoke the planar nature of the Triassic–Precambrian nonconformity surface to justify their inference of significant erosion prior to the Triassic; but the timing of the erosion is debatable, and has no bearing on the age of the canyon.

Hood et al.'s final arguments on canyon shape invoke the geophysical work of Oesleby (1978), who suggested a V shape, which Hood et al. take as evidence against Paleozoic glaciation. Oesleby's (1978) primary intent was to gauge fill thickness, and his assertion of a thick fill was, 25 years later, confirmed by drilling (Soreghan et al., 2007). However, as Oesleby (1978) himself stated, and Cole and Young (1983) reiterated, the limitations of his data and methods, including the longitudinal orientations of his profiles, precluded an accurate assessment of the transverse basement profile.

Hood et al. state that our evidence for high-stress fractures on quartz from our diamictite could support Cole and Young's (1983) hypothesis of Cenozoic glaciation in the canyon. This is an interesting departure from Hood (2009), who attempted to refute a glacial origin for these strata, preferring an origin as a talus deposit. Regardless, Cenozoic glaciation here is inconsistent with its low elevation (Soreghan et al., 2007).

Hood et al. (2009) conclude by stressing their view of a Cenozoic age for the canyon, citing as evidence the influence of Laramide-age faults, and the "bowl-shaped" longitudinal profile of the (bedrock) canyon. We cannot test the validity of these suppositions; no one knows the earliest ages of movement on any faults that may have influenced the canyon, and the longitudinal shape of the bedrock canyon is unknown because the basement location and relief is obscured by a fill now known to (locally) exceed 320 m depth and 2 km width (Soreghan et al., 2009).

We appreciate the comments of Hood et al. (2009), but find that they stem from either misrepresentations or suppositions. Our hypothesis is testable. Hood et al.'s discussion is a healthy step toward such testing. We hope our work will spur additional research aimed at investigating the idea of episodic cold in the late Paleozoic tropics, and the deeper understanding of both the geology of this region and the climate of the deep-time Earth that should result regardless of the outcome of such tests.

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