

The enigmatic rise of the Colorado Plateau

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How and when the Colorado Plateau attained its current mean elevation of ~2 km has puzzled scientists for nearly 150 yr. This problem is most dramatically manifest when standing on the rim of the Grand Canyon, viewing the extraordinary 1500-m-deep gorge carved into nearly horizontal sedimentary rocks that were deposited during the 500 m.y. prior to plateau uplift when the region resided near sea level. What caused the elevation gain of this previously stable cratonic region in Cenozoic time? Did the source of buoyancy for plateau uplift arise from the crust, lithospheric mantle, or asthenosphere, or through some combination of the three? Why did this low-relief plateau escape significant upper crustal strain during uplift, in contrast to the Cenozoic surface deformation that is so strikingly apparent in the high-relief landscape of the surrounding Rocky Mountain, Rio Grande Rift, and Basin and Range provinces (Fig. 1)?

The answers to these contentious questions are significant for understanding how deep-seated processes control the elevation change and topographic evolution of Earth's surface. These relationships are particularly cryptic within continental interior settings like the Colorado Plateau. Although there is a first-order understanding of vertical motions in areas close to plate boundaries, there is comparatively little consensus on the causes of such motions distal from these margins. The Colorado Plateau exemplifies this problem. The protracted history of Cordilleran orogenesis affords numerous opportunities for how and when uplift of the Colorado Plateau might have occurred. The

region is last known to have been at sea level in Late Cretaceous time, based on the widespread occurrence of marine sediments of this age. Elevation gain could have occurred in Early Tertiary time associated with Sevier-Laramide contraction, mid-Tertiary time synchronous with the proposed demise of the Laramide flat slab, or Late Tertiary time coeval with regional extensional tectonism in adjacent provinces. Hypothesized mechanisms include partial removal of the lithospheric mantle (e.g., Spencer, 1996), chemical alteration of the lithosphere owing to volatile addition or magma extraction (e.g., Humphreys et al., 2003; Roy et al., 2004), warming of heterogeneous lithosphere (Roy et al., 2009), hot upwelling within the asthenosphere (Parsons and McCarthy, 1995; Moucha et al., 2009), and crustal thickening (McQuarrie and Chase, 2000). It is clear that there is no shortage of mechanisms that could explain the plateau's origin. The core challenge is determining which mechanism, or combination of mechanisms, is indeed the cause.

Crucial for solving this problem are constraints on the plateau's elevation history, erosional evolution, magmatism patterns, and modern lithospheric structure. Liu and Gurnis (2010, p. 663 in this issue of *Geology*) and van Wijk et al. (2010, p. 611 in this issue of *Geology*) both present models for elevation gain of the plateau, and use such constraints from a variety of recent studies to better restrict and assess their models. Liu and Gurnis focus on an explanation for the Late Cretaceous through mid-Cenozoic uplift history of the southwestern plateau, whereas van Wijk et al. explore a mechanism for late Cenozoic elevation gain and differential uplift along the plateau edges.

In the first study, Liu and Gurnis employ an inverse mantle convection model to compute the changing dynamic topography, the vertical motion of Earth's surface in response to mantle flow. This model of dynamic uplift linked to Farallon slab evolution predicts an initial phase of subsidence associated with flat slab development, followed by two phases of uplift in Late Cretaceous and Eocene times due to progressive slab removal. Liu and Gurnis invoke mantle upwellings to induce the remainder of the plateau's uplift in the Oligocene. A distinctive result of the model is the prediction that the plateau was tilted to the northeast in Late Cretaceous–Early Tertiary time, with later dif-

ferential elevation gain of the plateau interior that diminished or reversed this tilt. The authors note that their results compare favorably with the conclusions of two recent investigations on the southwestern plateau (Flowers et al., 2008; Huntington et al., 2010). In the second study, van Wijk et al. examine the consequences of late Cenozoic edge-driven convection along the plateau margins induced by a step in lithospheric thickness between the plateau and the adjacent Rio Grande Rift and Basin and Range provinces. The lithospheric thickness contrast is observed seismically and attributed to Cenozoic extension of the adjoining regions. This work finds that asthenospheric upwelling and lithospheric mantle removal along the plateau edges can account for Neogene–Quaternary patterns of magmatism, distinctive shallow mantle seismic anomalies, and several hundred meters of differential uplift along the plateau margins.

One question arising from these two studies is: are their conclusions compatible? The proposed dynamic topography model can account for ~1.2 km of uplift in the southwestern plateau in Late Cretaceous through Eocene time. This portion of the history does not preclude later uplift, although Liu and Gurnis propose mid-Tertiary mantle upwelling to account for much of the remaining elevation gain to generate the modern ~2 km plateau elevation. Van Wijk et al. can explain several hundred meters of differential plateau margin uplift by edge-driven convection in the late Cenozoic. Their results do not exclude earlier uplift. Thus, to first order, the results of these two studies do not appear to be mutually exclusive. When integrated, they would predict a complex spatio-temporal progression of uplift migrating from southwest to northeast in Late Cretaceous through mid-Cenozoic time, with a late Cenozoic uplift phase and development of differential topography along the plateau edges.

The other obvious question that emerges from these efforts is both more important and far more difficult to answer. Do the proposed models accurately describe the true origin and evolution of Colorado Plateau elevation? Both studies are significant in advancing potentially important and viable mechanisms to explain key features of the plateau. They are therefore serious contenders among the suite of competing models for plateau elevation gain. However, determining the extent to which these models

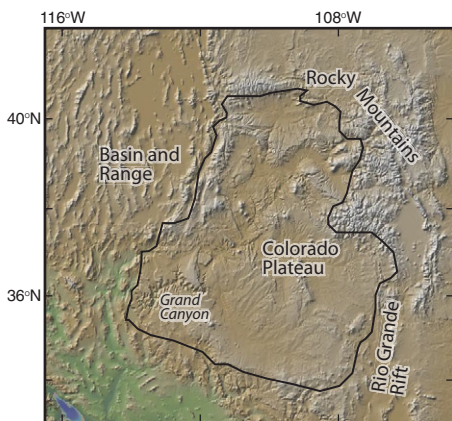


Figure 1. Topographic map of the Colorado Plateau and adjacent provinces.

approximate reality must in part await additional constraints on the uplift history with which to further test the predictions of each study.

One reason why resolving the cause of plateau uplift is such a tough problem is that deciphering the paleoelevation of continents is extremely difficult, and the plateau's elevation history is critically important for isolating the correct uplift mechanism. Dated marine deposits can determine when an area was at sea level, but no direct, reliable proxy yet exists for the past altitude of an elevated region. Strategies for addressing this problem commonly involve estimating paleotemperature, constraining paleorelief, deciphering paleohydrology, and reconstructing erosional and depositional histories. Paleotemperature estimates for inferring paleoelevation are typically made by stable isotope and paleobotany studies. Application of the new clumped isotope thermometer on the Colorado Plateau (Huntington et al., 2010) and paleobotany efforts in the Rocky Mountain region (e.g., Wolfe et al., 1998) are noteworthy examples. The past amount of relief in a landscape imposes a minimum constraint on paleoelevation, and resolving the carving of the Grand Canyon is an obvious target for constraining Colorado Plateau paleorelief. Recent approaches to determine the history of canyon incision include U-Pb dating of carbonate cave deposits (Polyak et al., 2008), $^{40}\text{Ar}/^{39}\text{Ar}$ dating of canyon basalts (Karlstrom et al., 2008), and (U-Th)/He thermochronometry of plateau surface and canyon samples (Flowers et al., 2008). The reorganization of plateau paleodrainage systems in part reflects the plateau's topographic development; deciphering this history has been the target of numerous stratigraphic, geochronologic, and isotopic studies (e.g., Young, 1979; Davis et al., 2008; Pederson, 2008). Stratigraphic and thermochronologic efforts have been used to resolve the plateau's erosional and depositional evolution that is linked with its history of elevation change (e.g., Dumitru et al., 1994; Cather et al., 2008). A basalt vesicularity study differs from the investigations above in its attempt to directly constrain paleoatmospheric pressure, and therefore paleoelevation, from the size of vesicles in plateau lavas (Sahagian et al., 2002), but these results are widely debated (e.g., Libarkin and Chase, 2003). Not surprisingly, contradictory interpretations regarding the uplift history of the Colorado Plateau often arise from the diverse information yielded by the many studies in this region.

The two geodynamic studies in this issue of *Geology* underscore the probable complexity of the plateau's history. They especially highlight

the unlikelihood of the entire plateau undergoing a single spatially uniform phase of surface uplift, and emphasize the potential for significant geographic and temporal heterogeneity in elevation gain. Such a history would only exacerbate the challenge of accurately reconstructing the plateau's evolution from the geological record. Perhaps some of the geological data that seemingly conflict in the context of simpler uplift models can be reconciled when evaluated in the framework of the more complex patterns of elevation gain predicted by these geodynamic studies. The inventive new approaches for deciphering the plateau's history coupled with testable predictions from geodynamic models are yielding fresh insights into the perplexing story behind the topographic rise of the Colorado Plateau.

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