

## LETTER TO THE EDITOR

### Reply to Comment by C. Rowland Twidale

The tone and content of Twidale's comment suggest that there are irreconcilable differences between our isotopically based interpretation of Australian landform development and his interpretation based on field evidence. However, examination of his assertions, the isotopic data, and our interpretations suggests that the fundamental conclusions of Twidale's work and ours are, in fact, similar.

Some of Twidale's complaints appear to stem from philosophical differences between his field-based and stratigraphic model for landscape development and our process-oriented, geomorphic interpretation of the nuclide data (e.g., our interest in erosion rates versus his emphasis on surface "age"). However, most of Twidale's complaints are semantic, perhaps reflecting cultural differences (e.g., *denudation* versus *erosion*: a synonym for us; vastly different implications for him). A few of Twidale's assertions, such as the claim that half lives limit the application of cosmogenic nuclides for dating, are common misconceptions and are simply wrong, as discussed below.

#### DO COSMOGENIC NUCLIDE ACCUMULATIONS PERMIT DATING OF LAND SURFACES?

This section of the comment contains inaccurate statements. For example, despite Twidale's claim, nowhere in our paper do we use the word *date*. Furthermore, Twidale's assertion that, "The half lives of the isotopes used impose severe limitations on the applicability of the method . . ." is wrong. To date, the only place in the world where the half lives of  $^{10}\text{Be}$  and  $^{26}\text{Al}$  limit cosmogenic dating is Antarctica (Nishiizumi *et al.*, 1991). In every other terrestrial sample analyzed so far, including all 68 Australian samples that we have analyzed, neither  $^{10}\text{Be}$  nor  $^{26}\text{Al}$  is saturated (Bierman and Caffee, 1996 and in review). Such undersaturation unambiguously indicates that nuclide abundance is limited by exposure duration, erosion, or burial, not by the half lives of  $^{26}\text{Al}$  or  $^{10}\text{Be}$ , 0.7 and 1.5 My.

This section of the comment repeatedly cites our words out of context, obscuring our original intent. For example, in our paper, we used the word *age* in quotations and with significant qualifiers such as *minimum limiting* and *assuming*. We encourage the reader to reexamine the last two paragraphs of our paper in which we make careful distinctions between the limiting *age* of a sampled surface and the

*age* of the landform. These paragraphs make it clear that in no way do we claim the "high accuracy" repeatedly inferred by Twidale on the basis of our use of the verb *measure*.

We agree with Twidale that the inselberg forms of the Eyre Peninsula predate the Pleistocene and we stand by our earlier interpretations of the isotope data (Bierman and Caffee, 1994; Bierman and Turner, 1995; Bierman and Caffee, 1996) reasserting that no currently exposed inselberg surface is Mesozoic in age. Erosion, even at the exceptionally low rate of  $0.7 \text{ m myr}^{-1}$  would have removed 40 to 50 m of rock from the inselbergs since the late Cretaceous when Twidale suggests that their ancestors were first exposed. As we stated in our paper, such erosion ensures that cosmogenic nuclide measurements will never allow one to verify nor to falsify Twidale's claim that the inselbergs we sampled are direct descendants of Cretaceous landforms. The answer to Twidale's question is clear; cosmogenic nuclides will not date land surfaces that have experienced such significant erosion, although nuclide abundances can be interpreted as limiting ages.

#### UNWARRANTED OR UNJUSTIFIED CLAIMS AND REINVENTING THE WHEEL

We thank Twidale for pointing out earlier works that suggest the persistence of land surfaces, some of which we cited in our paper and others of which were unavailable to us when we submitted our paper in 1994. In Bierman and Turner (1995), we use the word *denudation* three times and each time with the qualifier, *rate of*. On the first usage, we explicitly indicated that we consider *denudation rate* synonymous with *erosion rate*, a usage supported by the American Heritage Dictionary (Morris, 1978).

Twidale accuses us of failure to acknowledge earlier work and of making unwarranted and exaggerated claims. We thank him for correcting the error concerning the height of Mt. Wudinna but wonder how he could possibly suggest that our conclusion of unprecedented stability for Australian inselbergs was, ". . . not backed by comparative data." When making this conclusion in our paper, we cited Bierman (1993 and 1994) and Nishiizumi *et al.* (1991). In Bierman (1993) there are extensive isotopic data from three sets of inselbergs in very different climates; in Bierman (1994) there is a large figure comparing nearly 30 different estimates of

bare-rock weathering, many of which are cosmogenic data. Twidale's assertion that Bierman and Turner (1995) did not cite comparative data for inselbergs in other parts of the world is simply false.

#### GEOMORPHOLOGICAL BACKGROUND

Cosmogenic nuclides provide the first *quantitative* confirmation of Twidale's repeated assertion that the landforms of the Eyre Peninsula are ancient. We agree with Twidale that the two-stage model for inselberg development is supported by field evidence. Our data add to this model by providing constraints on the rate of rock surface lowering and by suggesting that the exposure history of these bare rock surfaces is likely more complex than previously believed, as we consider below.

We stand by our isotopically based conclusion that the exposure history of Australian inselbergs is complex. We based this conclusion on six samples for which the relationship the ratio of  $^{10}\text{Be}$  to  $^{26}\text{Al}$  is not consistent with continuous surface exposure (Fig. 3, Bierman and Turner, 1995). Such isotopic evidence of burial during or after initial cosmic-ray exposure cannot be interpreted in a unique fashion (Bierman *et al.*, 1996) but unambiguously indicates that the sample exposure history is complex and has included at least a period of burial after or during initial exposure. For example, on the basis of our current measurements, we cannot conclude whether cosmic-ray exposure of now-bare bedrock surfaces took place under a recently stripped cover of *in situ* weathered granite (as Twidale suggests) or whether now-bare rock was once buried by windblown sand and pedogenic carbonate after the weathered granite was removed.

The first case, recent stripping of a continuously shielded surface, is consistent with Twidale's model of episodic stepped inselberg exposure from beneath a cover of regolith but is inconsistent with his assertion that the inselbergs are direct descendants of features first exposed in the Cretaceous. Specifically, if the *in situ* cover under which the inselbergs supposedly formed was just now being eroded, how could the form of the inselbergs be more than 65 Myr old? The second scenario could be consistent with much older bedrock forms but implies transport and deposition of at least decimeters of material to which Twidale objects. In either case, the isotopic ratios are telling us something about inselberg history (burial during and/or after cosmic-ray exposure) that our eyes cannot. Nuclides produced in Eyre Peninsula rocks by cosmic-ray bombardment integrate hundreds of thousands of years of exposure and burial history, complimenting Twidale's 30 years of field observations. The isotopic data are themselves primary evidence for burial and exposure, with or without other field evidence.

Twidale's suggestion that our measurements would be

affected by previously acquired nuclides in the dust flux or vegetation betrays a shallow understanding of the isotopic techniques involved. We made our measurements on 250 to 750  $\mu\text{m}$  quartz separated from rock, not regolith or dust, and ultrasonically etched this quartz in acid at least four times to remove any meteoric  $^{10}\text{Be}$  adhered to the mineral surface. Vegetation can provide only several  $\text{g cm}^{-2}$  shielding, insufficient to change measured nuclide abundances more than a percent or two, particularly in sparsely vegetated south Australia. Interception of radiation by a cover of regolith is exactly what we have detected by making multiple isotope measurements and Twidale is correct when he concludes that, "it is difficult to determine when the surface began to accumulate a full dosage of nuclides." This is exactly the argument we put forth in the concluding paragraphs of Bierman and Turner (1995). Moreover, interpretation of measured abundances is made even more uncertain by erosion of the bare rock surfaces during their current period of exposure. It is for all of these reasons that we couched our arguments in terms of maximum limiting erosion rates and minimum limiting exposure ages.

#### CONCLUSION

We fully agree with Twidale that cosmogenic nuclide measurements must be placed in a geological context and we have strived to do this, in part by inviting Twidale to participate in the writing of Bierman and Turner (1995), the paper he now criticizes. Our data and approach are different from his and, in our view, complimentary. We have taken a dynamic and isotopic rather than a historical and stratigraphic approach to understanding the Eyre Peninsula landscapes. Our data show that exposed bedrock on the Eyre Peninsula, while exceptionally stable, is indeed part of a dynamic landscape. Such dynamic behavior calls into question the entire concept of dating landscape surfaces. How can one define the age of a continuously changing surface? To use Twidale's example in the beginning of his comment, the time at which a stratum was first deposited is definable and discreet even if we do not yet have the means to date it exactly. The same cannot be said for many landscapes. How could one define the moment in time when an inselberg landscape first took form? How could one define when such a landscape ceased to exist?

We disagree with Twidale that cosmogenic nuclide data in every case, "must be consistent with stratigraphic and geomorphologic ages and concepts at both the regional and local scales." New methods and new data do not always support existing paradigms. We cannot accept Twidale's claim that, "At present, stratigraphic methods are secure and widely applicable to the dating of old land surfaces." Specifically, Twidale's assertion that Eyre Peninsula insel-

TABLE 1  
Corrected Cosmogenic Minimum Model Ages for Granitic Inselbergs<sup>a</sup>

Sample	Site	Location	Minimum age ( <sup>10</sup> Be) <sup>b</sup> (ky)	Minimum age ( <sup>26</sup> Al) <sup>b</sup> (ky)
Y-12	Yarwondutta	top	691 ± 165	573 ± 166
Y-10	Yarwondutta	just below top	708 ± 170	496 ± 132
T-1	Turtle Rock	top	850 ± 213	543 ± 148
LW-2	Little Wudinna	top	784 ± 192	645 ± 186
P-1	Pildappa Rock	top	898 ± 225	625 ± 178
MW-10	Mt. Wudinna	top	785 ± 192	632 ± 182
MW-9	Mt. Wudinna	upper side	379 ± 85	347 ± 85
Y-7	Yarwondutta	mid flare	338 ± 75	283 ± 69
Y-6	Yarwondutta	mid flare	201 ± 44	180 ± 41
Y-5	Yarwondutta	low flare	118 ± 26	119 ± 26
LUI-7	Enchanted Rock	top	243 ± 53	219 ± 50
AHI-20	Alabama Hills	top	326 ± 71	308 ± 74

<sup>a</sup> AMS Measurements made at Center for Accelerator Mass Spectrometry, Lawrence Livermore National Laboratory

<sup>b</sup> Calculated using constant sea-level, high-latitude production rates for <sup>10</sup>Be and <sup>26</sup>Al of 6.08 and 36.8 atoms g<sup>-1</sup> yr<sup>-1</sup>, respectively; propagated uncertainty includes: measurement uncertainty, production rate (±20%), density (2.7 g cm<sup>-3</sup> ± 5%), attenuation (165 g cm<sup>-2</sup> ± 5%), half-lives (1.5 my and 0.7 my ± 5%), Al abundance (±5%), Be abundance (±2%).

bergs are Cretaceous in age is not supported by explicit evidence either in his comment (where the claim goes un referenced) or in his other papers. The rationale for stratigraphic dating of Eyre Peninsula surfaces has not been clearly and unambiguously spelled out in print. As best as we can tell, this Cretaceous age is assigned by Twidale on the basis of assumed dates for duricrust surfaces, hundreds of kilometers distant, that he has correlated with flared slopes and platforms on the Eyre Peninsula inselbergs (Twidale and Bourne, 1975; Twidale *et al.*, 1985).

Despite the differences that Twidale's comment and our reply appear to illustrate, our work unequivocally supports the most fundamental tenet of Twidale's research. Specifically, isotopic data demonstrate that bare rock surfaces in Australia are extremely stable, eroding at rates slow enough that the gross form of the landscape likely changes little over millions of years. Such low rates of erosion, if extrapolated, are consistent with the persistence of inselbergs over millions of years, a conclusion for which Twidale has argued tirelessly in over 30 years of publications.

In our view, the only substantial point over which Twidale and we disagree is philosophical. We, unlike Twidale, do not believe that one can assign a meaningful age or age range to the continuum represented by a dynamic, eroding, and ever-changing landscape.

#### ERRATA

A sign change in the equation we used for calculating ages in Table 1 of Bierman and Turner (1995) resulted in

underestimation of minimum exposure ages calculated from nuclide abundances. We inadvertently used Equation (1) in our data reduction spreadsheets whereas Eq. (2) is the correct equation:

$$t = \frac{1}{\lambda} \ln \left( 1 + \frac{N\lambda}{P} \right) \quad (1)$$

$$t = \frac{-1}{\lambda} \ln \left( 1 - \frac{N\lambda}{P} \right). \quad (2)$$

Correctly calculated minimum exposure ages (Table 1, this reply) are 9 to 65% greater than originally reported. The change is greater for Al minimum ages than for Be minimum ages because the half life of Al is shorter, and the corresponding decay correction is larger. No other results are affected. This revision does not affect the conclusions of Bierman and Turner (1995) because our arguments were based on erosion-rate estimates and <sup>26</sup>Al/<sup>10</sup>Be ratios, both of which were correctly calculated in the original table.

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