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# Reply to comment on ‘Scaling factors for production rates of in situ produced cosmogenic nuclides: a critical reevaluation’ by Darin Desilets, Marek Zreda and Nathaniel Lifton

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## 1. Introduction

In their comment Desilets et al. [1] claim that several assumptions made in my paper [2] are false and that consequently the scaling factors derived in [2] are not an improvement on Lal’s scaling model [3]. The comment of Desilets et al. [1] covers several points; my reply will follow their sequence of points raised for discussion. In part the assertions of Desilets et al. [1] arise from the brevity of some of the discussion in [2] that was demanded from me during the review process of [2]. I welcome the opportunity to expand on the reasons which led to some choices I made.

Desilets et al. [1] reach some of their conclusions by giving more significance to results they cite than the authors who originally produced them. I will show that the ‘false assumptions’ as characterized by [1] are safe approximations/simplifications that have no disadvantageous effect on the reliability of the scaling factors as presented in [2] and that the concerns of Desilets et al. [1] are largely unfounded. The relevance and correctness of some of my choices in [2], as for

example the value of the high latitude attenuation length  $I$  use and the utilization of atmospheric depth rather than elevation, have been confirmed in the mean time by the work of others [4,5].

## 2. Neutron monitor data

Desilets et al. [1] repeatedly state that I used data unavailable to Lal [6]. It seems they [1] missed that I did not reevaluate the PhD thesis of Lal from 1958 [6] but his paper of 1991 [3] (see e.g. abstract and introduction of [2]). The neutron monitor and other data available to Lal in 1991 [3] were essentially the same as those available to me, but he chose not to use these data.

I disagree with Desilets et al. [1] that the three sources they cite [7–9] provide more accurate information on the nucleon attenuation length, as used for scaling factors, than the sources used by myself. I maintain that the procedure I used, i.e. linking of latitude curves, is still necessary. The results of the three studies cited by Desilets et al. [1], [7–9], give values for *local*  $\Lambda$  as a function of altitude and cutoff rigidity. At pressures exceeding  $\sim 600$  hPa ( $\sim 4000$  m to sea level) the muon correction, as is considered necessary by [1] (see Section 3), introduces a  $\leq 5\%$  uncertainty on the local values of  $\Lambda$  (see figure 4 in [9]).

For scaling factors that relate production rates

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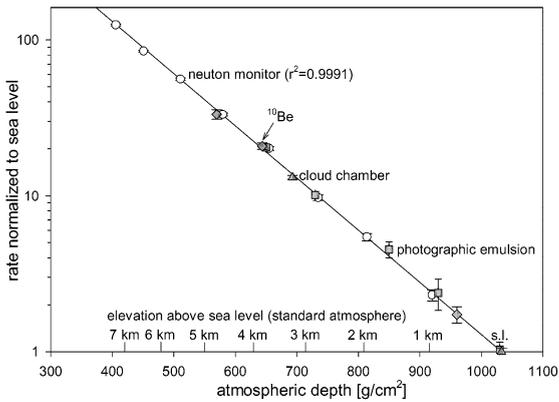


Fig. 1. The normalized counting/production rates of neutron monitor, photographic emulsion, cloud chamber and water target experiments are shown as a function of atmospheric depth (function of elevation), open circles [39], gray squares [25], gray diamonds [4], gray triangles [26]. The normalization is relative to sea level. The value of attenuation path length  $\Lambda$  is equal to the slope of the data array. The highly linear array demonstrates that a single value  $\Lambda$  describes the observational data perfectly well. The error estimates on [26,39] are based on counting statistics. The cutoff rigidities at the time and location of the experiments were  $\sim 0.5$  GeV [39],  $\sim 4$  GeV [4,25,39]. The solar modulation parameter  $\phi$  [17] at the time when the experiments were performed was  $\sim 700$  MeV [25],  $\sim 670$  MeV [39],  $\sim 550$  MeV [4], and  $\sim 500$  MeV [26], respectively. The values of  $\phi$  for [4,25] are estimated values based on the assumption that the experiments were performed in the year prior to submission of the paper as the exact timing of the experiments is not given by the authors. Further discussion is given in the text. The significance of  $\phi$  is discussed in Section 5.

to sea level the *integrated* attenuation of the cosmic ray flux between the sampling location and sea level is important, which is not necessarily equal to the local attenuation. While the decrease of the cosmic ray flux below 7000 m is not perfectly exponential [2,7–9], currently available experimental data do not allow or require a more detailed description of altitude integrated  $\Lambda$  as a function of atmospheric depth (Fig. 1) as relevant for exposure age dating (see also figure 8 in [10], [11]). The conservative error estimate for using my ‘poor assumption’, i.e. linear approximation, is  $\sim 3\%$  [2]. This is by no means perfect but it is better than the  $\leq 5\%$  of the ‘adequate characterization’ as preferred by Desilets et al. [1]. Moreover, the value of  $\Lambda$  for high latitudes as used in

[2] relies on three different instrumental approaches to determine  $\Lambda$  and is therefore more robust with respect to instrumental biases than using neutron detectors only (see Section 3).

### 3. Instrumental biases

Citing [8] Desilets et al. [1] claim that background correction and correction of muons will affect the sea level neutron monitor attenuation length by about 7%. The monitors used to collect the sea level neutron flux data used in my scaling factors had an estimated background of 0.25% ([12], p. 5). Not correcting for this background will therefore introduce a  $\sim 0.2\%$  error for the *normalized* neutron flux curve. Following Rose and Katzman [12] I took the liberty of neglecting this error. For the muon correction I follow the judgement of Raubenheimer and Stocker ([9], p. 5072) who conclude that “since the error introduced by the muon correction is of about the same magnitude as the correction itself, we may conclude that this correction is very uncertain and statistically insignificant”, and maintain that a satisfactory correction is, with the level of accuracy of present data, not possible.

Desilets et al. [1] are worried that the neutron monitor data used for my scaling factors are “biased towards the high end” of the nucleon energy spectrum. About 90% of all events recorded in a IGY neutron monitor (a design similar to those used for neutron surveys used in [2]) are produced by nucleons of energies  $\leq 1$  GeV, the lowest energies recorded are 50 MeV [13]. Within this energy range there is an energy bias in the neutron monitor counting rate. This bias affects the counting of incoming nucleon by factors 1, 2, 3 and 4 for mean energies of 110, 240, 520 and 1000 MeV, respectively [13], i.e. the response is stronger for higher energies. The median energy of nucleons contributing to the counting rate of a IGY neutron monitor is  $160 \pm 40$  MeV ([14], p. 74).

From the little we know about the reaction cross sections for neutrons producing cosmogenic nuclides it seems that reaction cross sections for target nuclides relevant for in situ produced nu-

clides mostly peak at energies between 100 MeV and 1 GeV (e.g. [15–17]) (those cross sections are published for  $^3\text{H}$  (precursor of  $^3\text{He}$ ) and  $^{10}\text{Be}$ ; judging from the consistency (within errors) of  $^{10}\text{Be}/^{26}\text{Al}$ ,  $^{10}\text{Be}/^{21}\text{Ne}$ ,  $^{21}\text{Ne}/^{22}\text{Ne}$  production ratios in quartz [18–20], measured at different altitudes and cutoff rigidities (i.e. different nucleon energy spectra), we can assume that the same is true for at least  $^{21}\text{Ne}$ ,  $^{22}\text{Ne}$  and  $^{26}\text{Al}$  produced in quartz). The response of neutron monitors is therefore favorably biased to the energy region where most of the reactions take place. The inaccurate knowledge of the reaction cross sections ( $\sim \pm 30\text{--}50\%$  [17]) does not yet allow full exploitation of our knowledge of the energy response of neutron monitors [13]. Hence we cannot yet obtain tailored scaling factors for each in situ produced cosmogenic nuclide. Correcting for undercounted neutrons of energies  $< 100$  MeV, as suggested by [1] (here I assume that [1] would want to be consistent and correct neutron monitor data in a similar fashion as photographic emulsion and cloud chamber data, see also below), however, will not improve the scaling factors as they are not, or are less relevant for most cosmogenic nuclide production. This correction would rather add an unwarranted systematic error to the scaling factors for the aforementioned cosmogenic nuclides.

The cross sections of reactions producing  $^{36}\text{Cl}$  and  $^{14}\text{C}$ , however, peak at energies below 100 MeV [15]. Production of  $^{36}\text{Cl}$  occurs at very low energies (thermal neutrons  $\sim 80$  MeV, ENDF, IAEA online data service, telnet: IAEAN-D.IAEA.OR.AT), which is in turn strongly dependent on target chemistry [15]. At this low energy end neutron energy modulation by protons contained in soil moisture and snow cover [14,21] will probably be of similar importance as the ‘right’ scaling factors. Therefore, I must conclude that for  $^{36}\text{Cl}$  my scaling factors might not be suitable (N.B. the same applies to those of Lal [3]). Studies utilizing in situ produced  $^{36}\text{Cl}$  most probably need an own and unique set of scaling factors, the same might be true for  $^{14}\text{C}$  studies.

Similar to the neutron monitors photographic emulsions record mainly events in the energy range between 100 MeV and 1 GeV (when count-

ing only stars with  $\geq 3$  prongs [22], as done in the studies used in [2]). Unlike the neutron monitor there is no energy bias in this energy range (every spallation event is counted as one event). An important feature of the photographic emulsions is that the target nuclei in the emulsion that can produce three-pronged and larger stars (C, N, O, S, Br, Ag, I, tables 2 and 3 in [23]) have a much lower mass than the lead neutron producer in the neutron monitors (produces 95% of the neutrons in a IGY monitor [14]). This is important as reaction cross sections for nuclear evaporation induced by cosmic rays are highly dependent on the mass of target nuclei ([14], pp. 34–37). Also the photographic plates on which the emulsions are fixed, and which are a source of secondary neutrons traced in the emulsions (usually thick stacks of plates are exposed, as in [22,24,25]), have a relatively low mean atomic mass (O, Si, Na, Ca; assuming a soda–lime glass composition). The mean atomic mass of the photographic emulsions and plates is thus close to the mean atomic mass of rocks we study for exposure age studies. Therefore it may be expected that the response of nuclear disintegration rates in rocks to changes in cosmic ray energy spectra/flux is similar to the response in photographic emulsions fixed on glass plates. The target (argon) in the cloud chamber experiment of Brown [26] (Fig. 1) has a similar atomic mass as the mean atomic mass of the target nuclei in the emulsion, and is therefore assumed to have a similar energy response (especially if one- and two-prong star events are undercounted, as claimed by Desilets et al. [1]). Furthermore the photographic emulsions and cloud chamber data of Fig. 1 and used in [2] record the omnidirectional cosmic ray flux.

Based on the discussion above I claim that the photographic emulsion and cloud chamber data are highly relevant for the description of the average nuclear disintegration rate in rocks as induced by cosmic rays. It is therefore reassuring to note that the altitude response of the neutron monitor used for the high altitude neutron flux curve in [2] and depicted in Fig. 1 is indistinguishable from the response of the photographic emulsion and cloud chamber experiments, despite the different

nature of the biases in those three cosmic ray flux recorders.

Recent experimental data confirm the conclusions of the discussion given above. Brown et al. [4] report an attenuation length for production of  $^{10}\text{Be}$  in water targets of  $130 \pm 4 \text{ g/cm}^2$ , at high latitude and elevations between 620 and 4745m (Fig. 1). This value is identical to the attenuation length for high latitudes derived in [2], i.e.  $130 \pm 4 \text{ g/cm}^2$ .

#### 4. Inclination versus effective cutoff rigidity for ordering neutron monitor data

My choice of the inclination for ordering neutron flux data and later scaling factors is less unique than Desilets et al. [1] imply. Equation 10 in [2] makes it clear that inclination and geomagnetic latitude are equivalent for sorting neutron flux data. Lal, in 1991 [3], uses the geomagnetic latitude as a sorting criterion for his data and scaling factors. I did not use the geomagnetic latitude, however, as in the way this term is usually used it implicitly contains the dipole assumption. The inclination on the other hand is simply an observational value that is free of such an attached and unwanted meaning. The inclination as used for my scaling factors can take any value that is observed (or reconstructed) as a result of the dipole *and* non-dipole components of the field.

It is clear that the cutoff rigidity  $P$  is in principle the most complete parameter for description of the cosmic ray flux, as can easily be seen from equation 2 in [2], which contains inclination and horizontal field strength. By using the inclination alone, intensity variations are neglected. The utilization of the inclination, or geomagnetic latitude for that matter, is therefore a simplification. I chose this simplification as the goal of my paper was to derive scaling factors that can be easily related to geographic coordinates ([2], p. 164) (over long time scales inclination and geomagnetic latitude are equivalent). If one takes a sample somewhere on the globe it is far easier to determine the average inclination (geomagnetic latitude) for the last  $n$  ka, than to determine the

average cutoff rigidity (this is, however, eventually necessary if secular intensity variations are to be taken into account [27,28]). However, my mind was not fixed on using the inclination as a sorting parameter when I started the reevaluation. The observed relationships between inclination, horizontal field strength, cutoff rigidity and neutron flux (figure 3a–c of [2]) led to contact with the USGS National Geomagnetic Information Center to find out whether the deviations from smooth fits are real or artefacts of the geomagnetic models used. I was informed that for example deviations of  $\sim 10^\circ$  between real and modelled values of inclination can be expected in areas with minimal data coverage (e.g. South Atlantic and Antarctic Sea; John Quinn, personal communication, April 1999). Based on that information I decided to ignore the data in those regions where the observed deviations can potentially be fully explained as an artefact of the geomagnetic models used. This data treatment remains justified. Sources for the discussion of geomagnetic models are given in [2]. The remaining data fit equally well to cutoff rigidity as to inclination [2], therefore I maintain that the simplification of using the inclination alone (see above) to describe the present-day neutron flux is valid.

Desilets et al. [1] present a fit of the sea level neutron flux data used in [2] against the cutoff rigidity (figure 1a in [1]). Their fit is somewhat smoother than the cutoff rigidity fit in figure 3c of [2]. Data from the South Atlantic and Antarctic Sea scatter more in my plot, for the rest the fit is comparable. The smoother fit in figure 1 of [1] is probably the result of the different geomagnetic model [29] used by Shea et al. [30] to calculate the vertical cutoff rigidities (the ones used by [1] for figure 1; Desilets, personal communication, December 2000; Drgf-50 and 55 were used in [2]).

My citation of Shea et al. [31] (in [2], p. 158) is partially incorrect. While equation 2 of [31] is essentially the same as equation 1 of [2], i.e. as cited in [2], it is not the same as ‘trajectory tracing’. However, Shea et al. [31] suggest that their equation 2 is a good approximation for cutoff rigidities as calculated by trajectory tracing. I am sorry for any confusion my inaccurate citation might have caused. Desilets et al. [1] are right that

the equation 2 used in [2] is a modified analytical solution to the dipolar equation, which is also clearly stated in [2] (p. 159). They are wrong, however, in their assertion that it cannot account for the effects of areal geomagnetic field. This field, a combination of dipole and non-dipole fields, results in a horizontal field strength and inclination of the magnetic field at the surface that are unlike values that would result from a geocentric or eccentric dipole. Observational or reconstructed values of the real geomagnetic field are the values used in equation 2. Thus fed with the proper observational or reconstructed values equation 2 as used in [2] can account for the effects of the real geomagnetic field. Here I also want to note that the potential benefits of using trajectory tracing only exist for recent times where a full description of the geomagnetic field is available. Approximations that would be necessary for going into the geological past, such as the eccentric dipole field approximation [32], give rise to errors of the order of 20% when determining the cutoff rigidity for some locations ([32], p. 737). Thus, for the description of the geological past, which is required for exposure age dating, trajectory tracing does not have any advantages over utilization of equation 2 of [2], as accurate geomagnetic models are generally not available. It is therefore of little use to fit present-day neutron flux data to cutoff rigidities calculated by trajectory tracing if similar calculations cannot be made sensibly for the duration of the exposure of a sampling locality (N.B. this limits the value of neutron monitor surveys that only provide the calculated cutoff rigidity and not the locations of the measurements). The geomagnetic parameters as used in equation 2 of [2], however, can be reconstructed to a large extent from local paleomagnetic records [27].

Strictly speaking Desilets et al. [1] are correct that inclination fitted curves should only be combined if they cross the geomagnetic equator at the same cutoff rigidity. The effect they describe, that low latitude counting rate appears to increase with a greater  $\Lambda$  than the true  $\Lambda$ , is, however, small. Fitting the data used in [2] against cutoff rigidity instead of inclination the results in ‘true  $\Lambda$ ’ at low latitude  $\sim 1.5\%$  lower (i.e.  $147 \pm 5$  g/

cm<sup>2</sup>) [27] than the low latitude  $\Lambda$  obtained from the inclination fits ( $149 \pm 5$  g/cm<sup>2</sup>) [2]. This deviation is well within the error estimate of 3% that I give for my derivation of  $\Lambda$  ([2], p. 166). If this is “the main problem with ordering neutron monitor data according to geomagnetic inclination” [1], I presume that in this respect my scaling factors are safe to use (see also Section 5).

## 5. Solar activity and the latitude effect

The work of Lockwood [33], which I used in [2], provides indirect evidence for the shape of the latitude curve. Lockwood [33] compared the responses of four standard IGY neutron monitors that are all located at cutoff rigidities between 1 and 2 GV, between sea level and  $\sim 2000$  m. Within  $\pm 2\%$  the monthly mean nucleonic intensities of these monitors co-varied in the period between a solar minimum and a solar maximum. Based on this finding I concluded that the *integrated* attenuation length between sea level and  $\sim 2000$  m remains virtually unchanged during a solar cycle, i.e. the relative distance between altitude curves will remain unchanged. The location of the monitors above the latitude knee (i.e.  $< 2$  GV) makes them particularly sensitive to changes in the cosmic ray modulation (e.g. see figure 2 of [1]). Therefore, I assumed that if there are no changes in the energy dependent altitude relationship for standard IGY monitors at low cutoff there will be no changes in the energy response at high cutoff. The data sources presented by [1] provide little reason to change that view at this moment, although at first glance (figure 2 of [1]) one might reach another conclusion. I will discuss the sea level curve and the high altitude curves of figure 2 of [1] separately.

One problem I see with the sea level data used by Desilets et al. [1] is the sea level curve at solar maximum [11]. Aleksan’yan et al. [11] only report a formula with fitting parameters that approximates their data but no actual data. The calculated counting rates deviate from the measured counting rates at low cutoff rigidities [11]. The discrepancy is not quantified by [11] but termed “considerable” for  $\leq 2$  GV (“Note that, for small

hardness  $R \leq 2$  GV, considerable discrepancy is observed between the initial and calculated data, which increases with altitude.” [11], p. 43). Desilets et al. [1] therefore did not plot data in their lowest curve in figure 2, but calculated counting rates that do not describe the neutron monitor response well at low cutoff rigidities, if we believe the authors that report the fitting parameters [11]. Since the deviation of the two sea level neutron flux curves plotted in figure 2 of [1] occurs at low cutoff rigidities I cannot follow Desilets et al. [1] who claim that the deviation between the ‘calculated’ data [11] and the measured data [34] is significant. I also do not see how [1] did the normalization of the two sea level curves, as two different and definitely not cross calibrated NM64 neutron monitors, both with unspecified reflector thickness and different numbers of counter tubes, were used. Citing Forman [35] in this context as done by [1] helps little as she reports on response of *local* values for  $\Lambda$  (see also Section 2) during a solar cycle and not on counting rates (N.B. the results for local changes of  $\Lambda$  of [35] do not agree with the *integrated*  $\Lambda$  for  $P \leq 2$  GV [33] discussed at the beginning of this section). Thus both the shape of the solar maximum curve and the relative difference between the two sea level curves remain unresolved.

The sea level data of [34] agrees well with that of [36] as used for my scaling factors [2]. Both ship based surveys show the same latitude effect in the same cutoff energy range ( $< 0.5$ – $15$  GV, intensity varies by a factor of  $\sim 1.77 \pm 0.5\%$ ). Both studies [34,36] also agree well (within  $\pm 1\%$ ) with an overland neutron flux survey of [8] (p. 2083 in [8]).

Low energy protons that are modulated by solar activity are quickly absorbed in the atmosphere and the effects of solar modulation mostly take effect at high altitude [17,37]. Therefore as long as we have no reliable sea level neutron flux data for solar maxima we may assume that there is only a minimal effect at sea level. In case this assumption turns out to be incorrect it is important to note that the solar modulation parameter  $\phi$  was 400–600 MeV when the sea level data of [8,36] were obtained [17]. The long term mean solar modulation parameter  $\phi$  is  $\sim 550$

MeV [17]. Therefore we may conclude that the sea level data of [8,36] were obtained in periods that were rather representative of the long-term average solar modulation (range realized in the last four solar cycles is 400–1200 MeV [17]). Consequently the same applies to my scaling factors at sea level [2] that use the data of [36].

The high altitude surveys [10,38] shown in figure 2 of [1] use the same neutron monitor and are directly comparable. This neutron monitor has, however, a different design as compared to the neutron monitor of [39] as used in [2]. The former is more sensitive to low energy neutrons as the shielding at the end of the pile used by [10,38] is less than half of that of [39] (figure 1 in [40], p. 264 of [39]). Therefore the surveys of [10,38] will react more strongly on solar modulation than that of [39]. For the influence of shielding thickness on the energy response of neutron monitors please see [14]. (N.B. the shielding thickness of the neutron monitors of [36] and [39], the ones used in [2], are virtually identical, i.e. 12–13 cm [39], p. 264, [14], p. 9–10).

The solar minimum and maximum surveys are normalized [10,38] to periods where the solar modulation parameter  $\phi$  was 450 and 1250 MeV respectively [17]. The high altitude survey of Sandström [39] was conducted in a period where the solar modulation parameter was  $\sim 900$  MeV [17], and thus should take an intermediate position between the surveys of [10] and [38] if the monitor responses are more or less comparable. It turns out that the monitors are comparable as the latitude effect observed in the Sandström survey (factor 2.74) [39] is intermediate between those found in the surveys of [10] and [38], i.e. 2.92 and 2.63, respectively. Previously it was mentioned that the long-term mean solar modulation is  $\sim 550$  MeV [17], thus the ideal curve for long-term scaling factors should lie somewhere between those of [38] and [39]. Due to the different design of the neutron monitors used [38,39] (see above) it is, however, impossible to locate its position with accuracy.

The experiments utilized to derive the attenuation pathlength I use to link the altitude curves [2] and the  $^{10}\text{Be}$  experiment of [4] (figure 1) were conducted in periods where the solar modulation

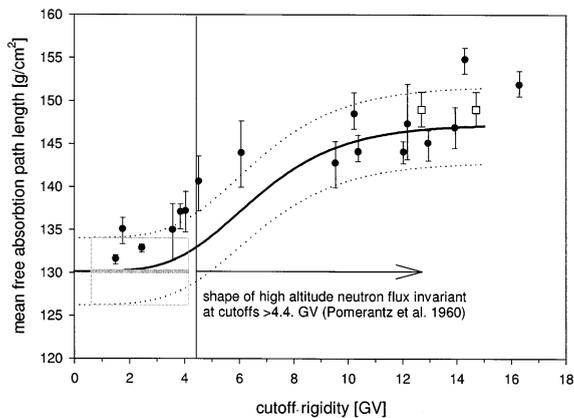


Fig. 2. The mean free absorption pathlength  $\Lambda$  as a function of cutoff rigidity. The solid line is calculated by linking the sea level survey of [36] with the high altitude survey of [39] with a mean free absorption pathlength  $\Lambda$  of  $130 \text{ g/cm}^2$  [2,27] at 0.5 GV. The stippled lines denote the  $1\sigma$  error envelope of  $\pm 3\%$  [2]. The gray box gives the constraints of the data shown in Fig. 1. The open squares denote the results of the photographic emulsion studies of [22,24]. The solid circles are the values of  $\Lambda$  obtained by [38] during a solar minimum. The data of [38] were obtained by the same neutron monitor as used for the high altitude curves in figure 2 of [1] that are discussed in the text. The line at 4.4 GV and the arrow indicate the region where the survey of [40] (same monitor as used by [38]) showed an identical shape of neutron monitor response as compared to that of [39] (figure 3 in [40]), despite the 300 MeV difference in solar modulation between the two surveys. The shape of  $\Lambda(P)$  is therefore probably fixed for  $P \geq 4.4$  GV irrespective of the solar modulation. Kent and Pomerantz [38] do not report the exact altitude range over which  $\Lambda$  was determined (“centered about a mean pressure of 500 mm of Hg [ $680 \text{ g/cm}^2$  or  $\sim 3.5 \text{ km}$ ]”, legend of figure 2 in [38]). Some of the scatter in their data could be the result of different altitude ranges over which the measurements were taken. There is also the possibility of an offset to higher values between the values of [38] and my  $\Lambda(P)$  simply due to a potentially higher minimum elevation of the measurements taken by [38] (see also discussion in [2], pp. 160–161).

was between 400 and 700 MeV (see legend of figure 1). Thus they are probably representative of the long-term mean solar modulation. Below I will show that using the mean absorption pathlength  $\Lambda$  of  $130 \pm 4 \text{ g/cm}^2$  [2] (figure 1) to link the solar minimum sea level curve of [36] with the high level curve of [39] yield values for  $\Lambda$  that are as reliable as currently possible.

In Fig. 2 the values for  $\Lambda$  as a function of the

cutoff rigidity are shown. For comparison I display the values that were derived by [38] (the source for the high altitude curves in figure 2 of [1]) during a solar minimum next to the calculated curves of  $\Lambda(P)$  [27]. Also a box depicting the constraints placed by the data in Fig. 1 and low latitude photographic emulsion data [22,24] are given. I plot  $\Lambda(P)$  because  $\Lambda(I)$  (as used in [2]) cannot be reconstructed for the values of [38] as the locations of the measurements are not revealed by [38]. A plot of  $\Lambda(I)$ , however, would probably result in even a ‘better’ correspondence between the data of [38] and the calculated  $\Lambda(I)$  as the curve would be steeper in the middle range in order to reach greater values of  $\Lambda$  at small  $I$  (large  $P$ ) (plateau at 149 instead of 147  $\text{g/cm}^2$ , see end of Section 4). All but one point of the data of [38] agree with my calculated  $\Lambda(P)$  within  $1\sigma$ , all agree within  $2\sigma$ . Therefore it is impossible to conclude which data set is ‘better’. I prefer the curve presented in Fig. 2, because of confidence in the constraints given by the data in Fig. 1 (see discussion above and Section 3). Desilets et al. [1] may prefer [38], because of their stated dislike of linking altitude curves. In conclusion, I maintain that we do not know  $\Lambda$  better than  $\pm 3\%$ , as is stated in [2]. The values of  $\Lambda$  that underlie my scaling factors [2] are therefore as good as they can be at present. The discussion above should have made it clear that both  $\Lambda$  and the sea level neutron flux curve used in [2], and therefore my scaling factors that rely on them, describe the cosmic ray flux representative of long-term mean solar modulation.

## 6. Measurements of cosmogenic $^3\text{He}$

The corresponding section in [1] is a comment on Dunai and Wijbrans [41]. I will nevertheless reply to it here. I agree with Desilets et al. [1] that testing scaling factors with geological samples is non-trivial. However, I believe that eventually it can and should be done. Various studies utilizing cosmogenic  $^3\text{He}$  (e.g. [41–43] and references therein) give production rates with error estimates between 2.5 and 4.5% ( $1\sigma$ ). If we believe the error estimates of these studies [41–43], then these and similar studies in the future can be used to test

scaling factors, as differences between scaling factors are in places as large as 20–30% [2,5]. The spatial and temporal spread covered by calibration sites that are presently available does not yet allow us to perform an ultimate test of scaling factors [5,27]. The results presented in our paper [41] are therefore also not such an ultimate test. They were simply the first time my scaling factors [2] were applied, and the results indicated that a better agreement between calibration sites could be obtained with the new scaling factors [2] than could be obtained with those of Lal [3] ([41], p. 154). I am unaware of any passage in [41] that claims more.

The erosion rate estimates used in [41] are based on the preservation of flow top features, which would disappear by the erosion of more than 2–4 mm. I agree with Desilets et al. [1] that these samples would be ideal for calibrations of production rates of other cosmogenic nuclides  $^{36}\text{Cl}$  and invite interested researchers to work on the same material.

Contrary to Desilets et al. [1], I recognize that the Monte Carlo simulations of Masarik and Reedy [21] are sufficiently accurate to make a reliable statement on the neutron flux at the air–surface interface. Other aspects of these [21] and similar calculations [17] fit experimental/observational data to better than  $\pm 10\%$ . Therefore I assumed that the same applies to the shape of the neutron flux at the air–surface interface and decided not to correct for shielding for the first  $10 \text{ g/cm}^2$ .

It is clear that over the time period of 150 or 280 kyr and longer as covered in [41] there cannot be absolute certainty of the absence of temporary soil or ash cover. Taking more samples as suggested by Desilets et al. [1] is better, I am grateful for that advice. As beneficial as sampling over a wider area might be, it entails the danger of ending up unnoticed in a lava flow of a different age. That was the reason I decided against sampling wider areas for each sample cluster. In the end, the identity of production rates obtained at two sites that: (i) are some 30 km apart, (ii) are on a flat plain (AFB [41]) and on a sloping mountain side (TA [41]), (iii) are located in the arid south and the slightly more humid north, (iv) are pro-

tected from easterly winds in one case, from westerly winds in the other, and (v) have age differences of a factor of  $\sim 1.9$ , implies that temporary soil or ash cover was probably not a problem. Each of the various points (i–v) would have a strong influence on timing and duration of soil generation and preservation as well as on ash deposition and preservation. It is extremely unlikely that both sites would be affected by the same (volcanic) event(s) to the same degree. Therefore, even though I cannot prove with 100% certainty that there was no temporary soil or ash cover, it is unlikely that varied depositional events would result in the observed identical production rates.

I cannot really discuss Ackert's new interpretation of his samples at this stage because I was less privileged than Desilets et al. [1] and was denied access to relevant data (Ackert, personal communication, September 2000). I can only say that it is interesting to note that the change in age (preferred age is now  $\sim 12\%$  lower, Brad Singer, personal communication, July 2000) and that of the omitted correction of implanted  $^4\text{He}$  ( $\sim 10\%$  [1]) have the same order but opposite effect on the production rate. Therefore, I tentatively state that I have my reservations about the revised, 25% higher, production rate.

## 7. Conclusions

The approximations I made in deriving the scaling factor for in situ produced cosmogenic nuclides [2] have no detrimental effect on their reliability. The differences between Lal's scaling factors [3] and mine [2] are therefore real and of the magnitude discussed in [2]. Recently some of my discussions were significantly expanded by Stone [5] who convincingly demonstrated that, by using realistic estimates for sea level temperature and the muon contributions to cosmogenic production (see also [2], p. 165) alone, 25–30% systematic error can be avoided in some areas. In the light of the discussion given here and in [2,5] I can see no reasons to keep on using the scaling factors of Lal [3]. The scaling factors as suggested in [2] are, however, also subject to fur-

ther improvement. A better knowledge of neutron reaction cross sections and direct measurement of the relevant cosmogenic nuclide production in natural and artificial targets will further improve the reliability of scaling factors. Furthermore, secular intensity variation of the geomagnetic field has to be incorporated to better describe cosmogenic nuclide production in the geological past [27,28]. [RV]

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