CORRESPONDENCE

In defence of the external detector method of fission track dating

SIR – In their recent letter concerning the work of Ross *et al.* on 'Fission track dating of British Ordovician and Silurian stratotypes' (Ross *et al.* 1982), Gale & Beckinsale (1983) make a number of assertions concerning fission track dating (FTD) which are unwarranted, and give an erroneous impression of the current 'state of the art' in FTD. To begin at (or near) the beginning, I seem to be quoted as having published an opinion (together with Tony Hurford) that the study by Ross *et al.* was not a 'considerable advance in the numerical calibration of the Phanerozoic timescale'. This is not the case.

Most serious, perhaps, is the assertion, quoted from Storzer & Wagner (1982) that '...Ross *et al.* were apparently unaware of the efficiency problems in the external detector technique for zircons...'. It should be stressed once and for all that the majority of the fission track community believe that there are no efficiency problems in the external detector technique – or method (EDM). Careful measurements of geometry factors (Green & Durrani, 1978; Gleadow & Lovering, 1977; Naeser, Izett & Obradovich, 1980) all show that the 'ideal' value 0.5 is applicable to the EDM for apatite, sphene and zircon, respectively. Even if non-ideal values ($\neq 0.5$) are observed, the EDM could still be used if consistency is demonstrated over a range of age standards, relative to which unknowns are analysed (Hurford & Green, 1983; Green, unpublished results). The EDM is in common use throughout the fission track world. Indeed, there is compelling evidence (see, for example, Gleadow, 1981) that while the Population Method (PM) of FTD, and its variations, should not be used with sphene and zircon, the EDM may be successfully applied with confidence.

Practically the only active workers who do not use the EDM are Storzer and Wagner themselves. This prejudice seems to result from an early study of geometry factors (Reimer, Storzer & Wagner, 1970) in which comparisons of external crystal surfaces with internal surfaces showed departures from the theoretical 'ideal' values of 0.5. Although no external detectors were in fact investigated in that study, the authors concluded that both external surfaces and external detectors were to be avoided, as these introduced 'non-ideal' geometry factors. In fact, re-assessments of this early study (Gleadow & Lovering, 1977; Green & Durrani, 1978) showed that the problems lay in the internal surfaces rather than the external surfaces, due to the (inevitable) presence of short tracks which must be correctly identified and counted. External surfaces in fact offer the best chance of accurate track counting, because these short tracks are not present. (Other practical considerations preclude their routine use, however.) Track densities in external detectors are less susceptible to the problems associated with short tracks, as these can be identified with ease in muscovite (the usual external detector material).

Thus the dismissal by Storzer & Wagner of the EDM is totally without justification. For Gale & Beckinsale to assert that Ross *et al.* are unaware of problems associated with the EDM is indeed unfair, as it is largely the efforts of the USGS group which have shown the usefulness of the technique, particularly in zircon. The geometry factors close to 0.5 for zircon, measured by Naeser, Izett & Obradovich (1980) show that the problem of short tracks in zircon has been mastered, and that tracks are counted with equal efficiency in both zircon and external detector (muscovite). This is all that is necessary for the EDM to be successful. The record of successful age determinations by the USGS group (in the sense of agreement with other techniques) using EDM in zircon further strengthens the validity of the technique.

The achievement of proper counting of fission tracks in zircon is not something which is gained overnight, but takes years of experience. Thus in any fission track study using the EDM, it is necessary for the authors to have shown the necessary consistency on age standards before any results on unknowns can be believed. This is perhaps one point which many people within the fission track community do not adequately realize. Nevertheless, in the case of the USGS group, this evidence is there for all to see.

Returning to the letter by Gale & Beckinsale, the next unwarranted assertion comes in the statement that the ages of Ross *et al.* may contain '...apparently unassessed errors due to flux perturbation (see Storzer & Wagner)...'. This is not a subject which has blithely been ignored in all previous work. Several workers, including myself, have looked for effects due to flux perturbation, and no significant interferences have been observed, at all. We can fairly safely say that flux perturbation introduces no error into FTD. It may be noted that even if these effects were significant, then since the calibration of FTD is done by using age standards (explicitly or not), if unknowns and standards are of similar physical nature, then flux perturbation effects will be likely to be constant throughout, and still introduce no additional error.

Following on with Gale & Beckinsale, in the next sentence comes the assertion that 'the only way to avoid this inaccuracy' (in neutron dosimetry) 'is to use an age standard which is irradiated simultaneously with each...unknown ...'. Hurford & Green (1982, 1983) indeed claimed that errors in neutron dosimetry may be grossly underestimated. However, although the above strategy was suggested to eliminate these errors, it should be made clear that a long and detailed series of experiments (Hurford & Green, 1983; Green, unpublished results) has led to the conclusion that this is not the best approach (let alone the only approach, as Gale & Beckinsale assert) to FTD calibration. Instead, a series of age standards should be employed to yield a calibration baseline for a (uranium-bearing) glass dosimeter.

In effect, although not explicitly stated, this was the procedure followed by Ross *et al.*; the only difference being that they split the calibration baseline into a neutron fluence determination and a value for fission decay constant, as discussed by Hurford & Green (1982). Thus Ross *et al.* cannot be criticized for their methodology, although they have rightly been taken to task for not making clear the detail of their procedures, as noted by Gale & Beckinsale.

It is gratifying to receive support for my criticism of the statistical treatment of Johnson, McGee & Naeser (1979), although Gale & Beckinsale seem to have missed the point somewhat. This is that while there is indeed a correlation between variation in ρ_s and ρ_i caused by variation of uranium content from grain to grain, this is not what is assessed by the conventional error assignment in the EDM. Instead, as explained in more detail in Green (1981), this error assignment estimates how well the age (or ρ_s/ρ_i) can

be constrained from the numbers of tracks counted, as a result of Poisson variation, and the Poisson distributions (virtual, not real) of ρ_s and ρ_i can never be correlated. This notwithstanding, Gale & Beckinsale are correct in asserting that the errors quoted by Ross *et al.* are underestimated. The body of evidence and opinion against the analysis of Johnson, McGee & Naeser is now overwhelming. In fact the appropriate errors to be assigned to the ages of Ross *et al.* could be even larger than those recalculated by Gale & Beckinsale, if non-Poisson experimental errors are present in any of their analyses. Such errors can be detected using the χ^2 test of Galbraith (1981). However, in the study by Ross *et al.* this test was not applied, and the incomplete data presentation precludes application of this test by other interested parties.

Finally (from Gale & Beckinsale) we come to the mention of the comments by Storzer & Wagner 'pointing to the large variations in the estimates by fission track dating of the age of the KBS Tuff...'. Storzer & Wagner choose to use this point to illustrate the unreliability of FTD, but looked at objectively the message from the history of work on the KBS Tuff is completely to the contrary. The final and definitive study using zircon FTD, by Gleadow (1980), gave a result which has a precision of $\pm 5\%$ (2σ), and is indistinguishable from the K–Ar age obtained from a similarly definitive study (McDougall *et al.* 1980). In both pieces of work, great care was necessary before a reliable age estimate could be obtained from either method. The K–Ar age of this deposit was also in question for a long time, but no one used this to question the validity of the K–Ar method. Similarly for FTD, the early failures can now be understood, as the result of a failure to include zircons with zero spontaneous track density, and thus zero apparent age. This is the sort of problem which is not considered until it arises, the KBS Tuff being the first time zircons with low track densities and thus having a high proportion of zero apparent ages, were encountered in a rock whose age was well constrained by other techniques. Not surprisingly, perhaps, it failed the test at first, but as is common in most science, the new effect distorting the results has been identified, and allowed for. On re-analysing the sample, a precise and accurate value of the age of the KBS Tuff was obtained from FTD, using the EDM, with procedures successfully tested on age standards for validity.

The point of all this is to stress that, contrary to the impression given by Gale & Beckinsale, and Storzer & Wagner, fission track dating using the EDM is a useful, reliable and potentially precise method of geochronology, when performed properly. The only point at which it suffers by comparison to other radiometric methods is in precision, at present, and at least most of the comments by Gale & Beckinsale on this front are true. A reasonable figure for the precision of a fission track age is $\pm 10\%$ (2σ) at present, *for a single determination.* Multiple determinations, however, allow much greater precision to be obtained, as shown by the study of the KBS Tuff by Gleadow (1980). In addition multiple determinations on age-standard zircons by Hurford & Green (1983) give estimates of the calibration scaling parameter, zeta, to a precision of $\pm 2\%$ (2σ) for one sample, which is not significantly different from results from three other samples. Thus precision much better than $\pm 10\%$ is possible even now from FTD. The final level of precision ultimately possible is mainly limited by the time necessary for repeat analyses, and $\pm 1\%$ at 2σ will probably never be achieved. However, generally the situation is favourable.

There remain two points of weakness in the study by Ross *et al.* First, despite my comments above about the success of zircon EDM, it has yet to be shown that the method gives a consistent calibration scale for ages much in excess of 100 Ma, as acknowledged by Ross *et al.* This arises mainly from the lack of suitable age standards. Perhaps the British bentonite results of Ross *et al.* could be construed as providing evidence of this consistency, although the errors involved in single determinations are too large to establish real consistency to the desired level. Work is continuing on this front, however, and the question should eventually be resolved.

The second point of weakness in the work of Ross *et al.* was also brought out by Gale & Beckinsale, who refer fleetingly to the failure of Ross *et al.* to 'consider adequately the possibility of track fading in zircon...'. In fact this constitutes the most serious failing of the Ross *et al.* study. Both at Dob's Linn, Moffat, and in Central Wales, some zircons are claimed to give depositional ages, while others are acknowledged to be reset. Ross *et al.* try to explain this by appealing to variation in the annealing properties of different zircons, but no evidence has ever been published to justify this as a real possibility. Bearing in mind that a 'depositional' and a reset age were obtained from two samples in the same section at Dob's Linn, and thus the zircons in each sample are likely to be quite similar, it is hard to believe that the older age is a totally unaffected depositional age. Merely to select the oldest ages as being of stratigraphic significance is not good science. The burden of proof must lie with Ross *et al.* to show that certain of their results are thermally unaffected, depositional ages. As yet, they have not done so.

The present discussion is also relevant to another article in *Geological Magazine*, by W. B. Harland (1983). In his mostly excellent review of the recent publication by Odin, Harland refers to the chapter by Storzer & Wagner cited above as giving 'some needed cautions' and 'a convenient state of the art survey'. Unfortunately, this is just what is *not* given. Storzer & Wagner have done little work on zircon, and abandoned the EDM over thirteen years ago, without ever studying it in detail. Most of their work has dealt with apatite or glass, dated by the population method. Thus they have none of the relevant experience on which to base a state-of-the-art survey on the application of FTD to stratigraphy, which inevitably must deal with zircon and the EDM. The true state of the art is much more favourable than they (or Gale & Beckinsale) indicate and is developing rapidly. The technique has left its infancy and is progressing steadily towards maturity, as indeed all other geochronological methods have done, in their turn, in past years.

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