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LETTERS TO THE EDITOR

Dear Editor,

Comment on "A production trend in AMS ages on Rattus exulans bones"

The June 1998 issue of *Archaeology in New Zealand* carried a report of a paper by Athol Anderson (Anderson, 1998) read at the NZ Archaeological Association Conference at Picton in April 1998, which claimed to detect a "production trend" in ages measured for kiore bones at the Rafter Radiocarbon Laboratory from 1995 to 1997. This paper was surprising to us for a number of reasons, not least of which was that there should be views presented which must reflect on the reputation of our laboratory without a prior opportunity to comment on those views. As a result of the presentation, we have been getting messages back from a variety of quarters that it has been established that bone gelatin dates have been "discredited", and that Rafter radiocarbon dates are "unreliable". These claims are quite incorrect. Of course, such stories always come at second or third hand, and it is impossible to pin down the sources, but they are no less damaging for that. I am quite sure that this result was unintended, but nevertheless, it appears that, in some minds at least, there is now a question mark over the integrity and competence of the Rafter Radiocarbon Laboratory.

Before considering the Picton paper, I want to make some general comments. As a radiocarbon laboratory, the Rafter Laboratory does not have a view on questions about late vs. early colonisation of New Zealand, or distinctions between transient contact and settlement, or the question of how kiore arrived here in the first place. Our first task is to provide the best possible data to the researchers investigating these matters. This involves applying our professional judgement to considerations of sample quality, handling and

processing, the radiocarbon measurements, and analysis of the data they produce. Procedures for processing various sample types are documented, and the documented procedures are followed to ensure uniformity of treatment methods. We also need to be aware of any improvements possible in the procedures we use, and where appropriate, such improvements are implemented and documented. Finally, we should be expected to stand behind the data we provide, which means that, in our best professional judgement, the number we supply is a true estimate of the parameter that it represents. I use the word 'judgement' here advisedly. We are scientists, we do not claim absolute knowledge. We must interpret what we observe in the laboratory in light of the knowledge and skill we bring to the task, and make decisions accordingly.

At the same time, we have to be prepared to re-examine our procedures and assumptions. Thus, when an authority such as Athol Anderson questions some results we have supplied, based on his experience and knowledge, we would be foolish not to take those objections seriously. In the present instance the questions raised have led to a prolonged methodological review and re-examination, consultation with recognised bone dating experts and intense discussion within the laboratory. If, after such a process, we have not identified a clear, substantive reason for questioning the measured ages, we have no option but to record that fact. This should not be mistaken for intransigence or defense of entrenched positions. It simply means that we do not see a resolution of the problem in the radiocarbon process, but that does not, and must not, close off further discussion and investigation.

Now let me turn to the specifics of the paper presented in Picton. The suggestion of a production trend has serious implications beyond the kiore situation. The claim was made in respect of a particular set of specimens of a particular species from a particular site. But since the procedures followed for processing bones are standardised, any "trends" introduced by those procedures should apply to all bones, and in fact Anderson drew the inference that other radiocarbon ages measured on kiore bones should be regarded as questionable. The existence of a global trend would have serious consequences for those researchers who have submitted significant numbers of bones for dating at the Rafter Laboratory over the years, and by now one might expect that problems would have arisen in the interpretation of the body of data that the laboratory has supplied. I am not aware of such problems, and I have been unable to ascertain that they exist. There have been several occasions when it has been possible to obtain independent checks

on bone ages that we have measured, and the agreement in nearly every case has been very good. Included in this is the series of 10 measurements we have made on the Third International Radiocarbon Intercomparison (TIRI, Gulliksen & Scott, 1995) whalebone over the period July 1994 to August 1995. Another example is given in Anderson's paper, and it is rather ironic that this agreement between the Rafter and Oxford laboratories is also used to question the reliability of our measurements.

Significant checks on bone age measurements are listed in the table. Some of these data have not been published previously, while the Pauatahanui results are discussed in detail in a forthcoming publication (Beavan *et al.*, 1998). Apart from Pauatahanui, the only kiore measurements shown are the bone powder intercomparison with the Oxford AMS laboratory (Anderson, 1998). This is because these measurements were made on a split homogenised sample, so that the two laboratories can reasonably be guaranteed to have measured the same material.

Sample	Bone CRA	Check
TIRI whalebone (x10) 1995	12640 ± 72	TIRI Consensus 12788 ± 30
TIRI whalebone (x2) Nov. 1995	12780 ± 66	TIRI Consensus 12788 ± 30
Piopio March 1996	2995 ± 72	Associated eggshell 2905 ± 88
Kiore bone powder (x6) 1997	656 ± 52	Oxford AMS (x2) 694 ± 57
Pauatahanui 1997 (x6)	519 - 434 cal BP	Shell 459 - 379 cal BP (x6)
Finsch's Duck 1998	1897 ± 70	Taupo 1820 BP

Numbers in parentheses indicate the number of measurements used to obtain the average in the middle column. The two TIRI whalebone measurements in line 2 of the table were carried out by Nancy Beavan in November 1995 specifically to check the processing methods used on the previously measured kiore bones reported by Holdaway (Holdaway, 1996). It is important to stress at this point that all the bones dated at the Rafter Laboratory over this period were processed identically. The only substantive changes to procedures concerned a tighter monitoring of the progress of the sample processing, and the addition of extra analyses to aid the evaluation of the sample quality. This resulted in better control on sample weights and yields, but did not affect the outcome of the measurements. I must also re-iterate that each time there is an explicit check available on the bone age, good agreement is obtained, and this performance spans the whole period of concern in the present discussion. When the whole collection of bone ages is reviewed it becomes difficult to

argue that those for which checks are available just happened to come out right while the others must be wrong.

So what of the "production trend" postulated by Anderson? As I discuss above, the existence of such a trend must have consequences going beyond the immediate kiore question, but such consequences do not seem to be apparent. Bone ages that can be independently verified do not display a trend. I have examined all the bone ages measured in the Rafter AMS laboratory and have been unable to discern either long or short term trends in the data. The simplest explanation for the graph presented in the Picton paper is that the bones measured in 1995 were older than those measured in 1997. This is not to trivialise or dismiss the problem. We clearly still have a situation of conflicting evidence, but I think that the solution will be more subtle than simply inadequacies in the dating laboratory.

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References

Anderson, A.J. 1998. A production trend in AMS ages on Rattus exulans bones. *Presented at* NZ Archaeological Association Conference, Picton, April 1998

Beavan, N.R., McFadgen, B.G. and Sparks, R.J. 1998. Reliability of bone gelatin AMS dating: *Rattus exulans* and marine shell dates from Pauatahanui midden sites. (*To be published*)

Gulliksen, S., Scott, E.M. 1995. Report on the TIRI workshop. *Radiocarbon* 37(2):820-821

Holdaway, R.N. 1996. The arrival of rats in New Zealand. *Nature*, 384:225-226

Dear Editor,

Reply to comments on "A production trend in AMS ages on *Rattus exulans* bone."

My NZAA conference paper, made no remarks on the integrity or competence of the Rafter Laboratory. I should also like to point out that I have never been sent a pre-publication draft of any paper from the Rafter

Laboratory, including of that which offered gratuitous critisizm of my AMS samples. Nevertheless, I promised Rodger Sparks a copy of the NZAA conference paper when I had a complete set of the ancillary data, including from Rafter, and I would have preferred comment on that instead of upon reports of its presentation.

Now to more important matters. I am pleased that my presentation prompted a "prolonged methodological review" of their AMS operation by the Rafter Laboratory. However, as the results of that engage rather obliquely with the central and substantive problem of contradictory AMS dates on prehistoric rat bones which was the point of my paper, and as that is unpublished, I shall have to summarise it here (see also Anderson 1996, 1998, Smith and Anderson 1998) in order to take up the issues raised by Rodger Sparks' commentary.

I have three archaeological sites which are well-dated by conventional radiocarbon on multiple samples of different type, charcoal, shell, moa eggshell etc., to the period 500-800 b.p. approximately - many of the dates are from the Rafter Laboratory. From the same dated contexts I took multiple samples of *Rattus exulans* bone and sent them to two laboratories, Rafter and Oxford, between 1995 and 1998. Throughout that period the Oxford results came back as expected or not much earlier (500-950 b.p.). However, the Rafter results fell into the range 1500-2000 b.p. in 1995, 1600-900 b.p. in 1996 and 750-600 b.p. in 1997. In other words, over three years the Rafter results moved progressively towards the expected date range, a trend missed in the analysis described by Rodger Sparks because it lumped all AMS bone ages together indiscriminately. It should also be noted that since the Oxford AMS results generally agree with the conventional radiocarbon chronology it is the Rafter results which are anomalous.

In accounting for their unusually old determinations, Rafter have traversed mutually contradictory explanations - the samples were suitable for AMS and the dates are right (the initial laboratory position, and that now adopted by Rodger Sparks), and the samples were degraded and the dates are wrong (Beavan and Sparks 1997). In fact, there are no laboratory data indicating that any of the samples were degraded. In addition, various sources of evidence from four radiocarbon laboratories show that archaeological samples of *Rattus exulans* bone from Shag River Mouth are as a whole well-preserved and therefore suitable for accurate AMS dating.

However, there are also serious objections to the proposition that the dates are correct (see also arguments in Anderson 1996), one fundamental problem being the implication that successively younger samples were sent to Rafter during a period when only material of more-or-less expected age was selected from the same sample contexts and sent to Oxford. Given that the age of a sample is unknown in advance, such a consistently patterned selection of material is deeply improbable and, in any event, results from both laboratories cannot be correct.

Assuming that the unusually early Rafter determinations are wrong, we have to consider alternative explanations, one of them being that there might be something amiss in the processing of samples. Rodger Sparks details recent intercomparisons (of limited extent it must be noted, and some of them unpublished data from my homogenised bone powder experiment currently underway at three laboratories), which show a gratifying consistency but few of these results are on rat bone and none from before 1997. This is really the crux of the problem: the production trend that my NZAA conference paper identified predicts that intercomparison of results obtained in 1997 or 1998 would disclose no significant differences, which is indeed the case. However, it also suggests that massive inconsistencies between Rafter and Oxford rat bone dates from the same sample contexts in 1995 and 1996 would have been confirmed by intercomparison experiments performed at that time. I had drawn attention to the need for these tests, advocating additional dating of material from all contexts which provided early results in comparative experiments between different laboratories. Regrettably they were never carried out.

Consequently, if the problems lay in some aspects of sample processing it is now almost impossible to go back and document them. We know that there had been some other AMS bone-dating difficulties at Rafter in the early 1990s which might be relevant to the rat bone results. For example, Pietrusewsky, Galipaud and Leach (1996), describe a case involving human bone in which Rafter results were 1400 years adrift of expected ages and Oxford determinations. It is interesting to note that most of the startlingly early results on rat bone, on both natural and archaeological samples, were also advised by the Rafter laboratory (along with some disconcerting remarks about the reinstatement of pre-Maori Moriori) prior to a change in the operating personnel in late 1995. So, the potential for operator-variation in processing, especially in pretreatment, needs to be minutely scrutinized.

Inter-laboratory variation in pre-treatment strategies involving very small samples of bone could also be important, as I have suggested before. The Oxford system, in which gelatin is purified by ion exchange, is generally considered an improvement on the standard method used at Rafter. My small-scale experiments concerning this matter should have been overtaken long ago by a full laboratory programme. Essential to it, would be inter-laboratory comparisons using natural rat bone samples from contexts which gave early ages to see whether they reveal the same pattern of conflicting age determinations manifested in the archaeological data. I have urged this basic test for nearly four years, so far to no avail.

I do not say that answers to current problems in dating rat bones will necessarily point to laboratory inadequacies. I agree that the solution could turn out to be complex or subtle, but regard that as all the more reason to take a broader view of potential difficulties. The recent intercomparison data reported by Rodger Sparks may be reassuring for the current clients of the laboratory, but they issue from a review conceived in terms which simply fail to confront the basic problem that serious contradictions, created almost entirely by anomalous Rafter Laboratory results, remain in the body of evidence upon which the substantive question of early rat colonisation is debated. These have still to be explained and resolved.

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References

Anderson, A. J. 1996. Was *Rattus exulans* in New Zealand 2000 years ago? AMS Radiocarbon ages from Shag River Mouth. *Archaeology in Oceania* 31: 178-184.

Anderson, A.J. 1998. Rating the Dating? *New Zealand Science Monthly* 9:2.

Beavan, N.R. and Sparks, R.J. 1997. Rating the Dating. *New Zealand Science Monthly* 8: 7-8.

Pietrusewsky, M., Galipaud, J-C., and Leach, B.F. 1998. A skeleton from the Lapita site at Kone, Foue Peninsula, New Caledonia. *New Zealand Journal of Archaeology*, (forthcoming).

Smith, I.W.G. and Anderson, A.J. 1998. Radiocarbon dates from archaeological rat bones: the Pleasant River case. *Archaeology in Oceania*, (forthcoming).