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Reply to Enright and Osborne

Douglas G. Sutton

Anthropology Department, University of Auckland

If this debate is to be useful we must define our target. Specifically, what are the environmental impact or archaeological remains of the first people who lived in New Zealand going to look like in the field? We can infer the following.

- (i) There may have been no more than fifty of them (Kirch 1984: 83).
- (ii) Their intrinsic population growth rate need not have exceeded 1 percent per annum (Brewis *et al.* n.d.; Davidson 1984: 57).
- (iii) Their rate of population growth may have followed either a sigmoid or an exponential curve (Brewis *et al.* n.d.).
- (iv) Archaeological evidence of their settlements will be masked by a range of taphonomic factors.
- (v) Those sites need not contain extinct species (Sutton 1987).
- (vi) The non-archaeological evidence of first colonisation is very unlikely to be unambiguous (Enright and Osborne 1988).
- (vii) The first colonists may not have had an Archaic East Polynesian material culture, as defined by Golson (1959).
- (viii) People have been within less than 20 days sailing days of New Zealand (Babayan *et al.* 1987) for approximately 2500 years (Chikamori 1987; Kirch 1986). Therefore, evidence from the whole of that period must be considered.

These points constitute an initial definition of target in terms of its time frame and the possible scale of its impact. The target has not yet been identified in the field.

Several of Enright and Osborne's (1988) objections to my paper are justified only if one accepts their reading of it—as author I do not. Their points 1 and 2 are seriously at odds with what I wrote. Apparently, I must reiterate two points. The first is that the proposed downwards revision of McGlone's (1983) chronology does not depend on the results of Chester's (1986) analysis. The second is that the possibility that New Zealand was settled earlier than Davidson (1984) has suggested is in no way dependent on the real age or ages of the Kaharoa ash fall.

We are agreed that the evidence at Pataua is indeed worthy of investigation and that human agency in the event dated to *circa* 1550 years B.P. at Puketurua cannot be discounted.

Similarly, Enright and Osborne have not eliminated human presence as a possible cause of colour changes in the shell deposit at Stillwater. However, I am surprised to be told that the Waimakariri soils are not relevant. Enright and Osborne do not argue at all with my view that explanations of major depositional events which occurred from 2500 B.P. are dogmatic and, at the same time, profoundly contradictory (Sutton 1987). However,

despite that significant omission they insist that one principal piece of evidence from the time period in question is irrelevant. The reasons they give are insufficient.

Enright *et al.* (n.d.) have shown that forest reduction by fire and subsequent advancement of sand dunes occurred on the Aupouri Peninsula before about 2100 years B.P. These dates neatly match those recently obtained by Chikamori (1987) and Ottino (1985) for human presence in other parts of Polynesia (see also Kirch (1985) on Hawaiian chronology).

Millener (1981) and Taylor (1984) are cited by Enright and Osborne to thwart the possibility that this 2100 B.P. disturbance in the Far North was anthropogenic. Enright and Osborne feel a need to defend the short time scale of New Zealand prehistory. However, the evidence does not require or justify the effort. In fact, considerable license is taken with the evidence to make it support the short time scale. In particular, the use made of Millener's (1981) thesis is an unacceptable oversimplification of its contents. Taylor's (1984) interpretation of the chronological position of Twilight Beach midden is overextended.

Atheoretical empiricism and excessive intradisciplinary certainty will not help with the task of defining the date of first settlement. A shared theory of evidence and an ability to deal constructively with ambiguous field data are prerequisites to success. New research designs in both archaeology and the historical earth sciences are needed now.

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