alternations of open work gravels and sandy (matrix supported) gravels reflecting the fluctuating energy levels of the depositional environment.

(*d*) The 'unexpected conformity' of channel depth reported by the authors is surely a function of maximum scour within a multi-channel stream system. Such uniformity is a fundamental characteristic of braided channels independent of the climatic environment. Similarly the dense channel network can be ascribed to another basic property of such networks without recourse to pore ice.

(*e*) Box-shaped forms. The reference to French (1976, p. 175) is inappropriate since he referred to an overall cross-valley profile, with an essentially flat floor flanked by abrupt bounding valley slopes giving an *overall* shallow box-like form to the valley bottom. Indeed French (caption to fig. 8.4) referred to stream *incision* in a terrace. No data is presented relevant to the interpretation of the palaeochannels under discussion.

3. Local evidence for former periglacial environment

First, we accept that the 'poorly sorted and unstratified debris' which overlies the channel fills is a head (solifluction deposit) but its presence is solely witness to former slope instability and is not diagnostic of permafrost. Indeed a major component of the pioneer case study of solifluction by Andersson (1906) related to the Falkland Islands where permafrost is absent. Clearly the head-gravel unconformity may mask a major hiatus. Secondly, we are perplexed over the relevance of the reference to disturbed gravels described by Dines and Edmunds, for even if the same gravel unit as that infilling the palaeochannel is involved, the fabric modification is obviously epigenetic.

From the foregoing discussion it is clear that in our view no evidence has been presented by Fisher & Rolls which conclusively supports their case for thermal erosion. Rather the evidence presented is totally consistent with processes associated with a braided river environment but the presence of permafrost is unproven. We urge caution in accepting the periglacial control on valley floors as inferred by recent authors. There is a great danger of slipping into the reinforcement syndrome unless unequivocal evidence for the former presence of permafrost is presented. Usually this latter evidence is lacking and even in areas of present day permafrost the frozen ground has limited influence on the sedimentary structures produced in fluvial systems.

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Sir – We welcome the interest generated by our interpretation of the Wrecclesham channels. Our replies to what we believe to be the more important of the points raised are broadly arranged in the sequence adopted by Worsley and Bryant.

1. Palaeochannels

(i) We separate the main fluviatile event (associated with the planing of the Folkestone Sands) from the incision of the palaeochannels on the ground of contrasting morphology and, by inference, of contrasting process. It is difficult to see how flowing water could plane an extensive surface and at the same time dissect that surface. Consequently, as the channels are incised into the surface they are seen as younger and produced under different circumstances.

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(ii) It is by no means clear, as asserted by Worsley and Bryant, that the channels are braided in the normally understood meaning of that term. A modern view suggests that braided streams 'consist of a series of rapidly shifting channels and mid-channel bars' (Miall, 1977, p. 3). The Wrecclesham channels we have described are separated by interfluvial zones consisting of *in situ* Folkestone Sands, and not bars.

(iii) Worsley and Bryant argue that the morphology of the channels is similar to that developed by braiding under non-permafrost conditions. A study of the literature suggests that this is doubtful. For example, Williams & Rust (1969) in their study of the R. Donjek, Yukon, Canada, described channel scours as being 'elliptical in outline' (p. 662), referred to the cross-sectional profiles of main channels as being 'almost invariably asymmetrical' (p. 660), and presented mean width/depth ratios varying from 21.8 to 40.8 (p. 663) for the channel types they studied. None of these descriptions fits the Wrecclesham examples we have discussed.

(iv) Comments about the likely depth of freezing in the light of contemporary environments are difficult to discuss. Williams (1975, p. 117) drew attention to the problem of identifying modern analogues of past conditions.

(v) Worsley and Bryant suggest that the open fabric of the channel gravels reflects a high energy environment. This view must surely diminish the plausibility of non-permafrost braiding as it involves high shear stresses and consequently disturbance of the (unfrozen) channel wall material. Such disturbance is not visible in the channels studied.

(vi) We agree that orthodox braided channels would, because of size similarity, be associated with roughly uniform depth. However, the channels we have measured show appreciable variations in size (our fig. 1) and we could therefore expect similar variations in depth. That these do not occur argues against the non-permafrost braided origin and is consistent with our model.

(vii) We drew attention to French's description in order to emphasize the angularity that appears to be characteristic of thermoerosional profiles irrespective of scale.

2. Evidence for former periglacial conditions in the local area

We accept that the local occurrences of head and frost-disturbed material are not of themselves proof of former permafrost. However we merely stated that 'local evidence is consistent with a former periglacial environment' (p. 317).

The essence of Worsley and Bryant's argument is that the channels result from braided river activity in a non-permafrost environment. We believe that the evidence derived from channel position and morphology, from the nature of the interfluves, from the character of the contained sediment, and from the likely behaviour of saturated sand, is inconsistent with orthodox braiding but supports a model of thermal erosion for an area of known periglacial activity.

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