*D. L. Linton R. S. Waters*
*Birmingham — Sheffield*

# THE EXETER SYMPOSIUM

## DISCUSSION

*Tuesday, 14th July, 1964*
*Chairman:* Professor Linton

Professor Linton — Three papers were originally submitted that were to have been read this morning as part of a session on tors. Professor Jahn was to have described granite tors in the Karkonosze Mountains in Poland, Dr Demek was to have discussed tors in the Bohemian Highland (Czechoslovakia) and Dr Bird would have described tor-like sandrock features in the central Weald in the neighbourhood of Tunbridge Wells. Now, since none of these gentlemen can be with us, we are deprived of three papers which would I am sure have led this morning to a discussion of tors in general and left us this afternoon for general discussion. Now since there are no papers to be read it is suggested that this morning's session be devoted to discussion of points that arise out of our two days in the field.

In the field it is not possible to discuss matters as fully, or in some cases as explicitly, as is desirable. Field discussion is most stimulating and a source of delight I think to all persons minded as we are, but it is also desirable to have the opportunity to recollect in tranquillity and reconsider some of the things that are said in heat in front of the exposures and to adopt views as sober and conciliatory as possible. Perhaps we may attempt such a discussion this morning, and take up in turn four broad questions that arise from what we have seen in Dartmoor and in North Devon concerning the solifluxion deposits that in the southwest of England are known as the *head.*

First there is the question of their origin and their relationship to earlier phases of deep weathering including also the significance of the tors that you saw on Dartmoor. Next we may discuss some of the climatic implications of the structural features that we have seen in the head. Thirdly we may consider the age relationships of the head — whether the successive heads belong to different ages, and their relations to the raised beaches. Finally, the raised beach itself poses a very severe problem with its association of a high sea level and a cold environment. Here I am very happy to welcome Professor Rhodes Fairbridge to our gatherings. He was fortunately able to join us last evening and we look to him for some contribution to our discussion on this point.

I propose now to initiate the discussion myself by making, as it were, a Chairman's contribution that may be rather more extended than those that follow, and will introduce our first question. It is quite clear that features due to solifluxion and associated processes did not arise *de novo* on a landscape of bare rock. There was, of course, a pre-existing landscape. And since the Quaternary period followed a long period of mild, temperate climate, it would be mistaken to imagine that pre-glacial landscape as other than one that had suffered a good deal of weathering. It is possible that the immediate pre-Pleistocene conditions in Britain were warmer than those of today, but it is not necessary to assume very much in this direction. Perhaps the climate may have resembled that of present day Portugal, an Atlantic climate with possibly rather higher summer and higher winter temperatures than Devonshire now enjoys. Be this as it may, what seems more certain is that the period was one of stability of base-level and therefore that weathering had been acting for a long time. It is probable that a good deal of the landscape had been weathered to a state in which a full weathering profile had been developed. Here we may profitably recall the sequence proposed by Ruxton and Berry in their studies of the weathering of granite in the tropics [1]. They recognised four weathering zones going down from the surface. In their zone IV, their lowest zone, the alteration of the rock is confined to the penetration of weathering solutions along joint planes leading to some modifications of the surfaces of the joint blocks but leaving these in their original positions. Near the ground surface weathering may be complete in the sense that no original rock is left. So their highest zone is defined as the layer in which the products of weathering exceed 90% of the total. Conversely in the lowest zone (IV) the products of weathering are less than 10%. In between, zone II is characterised by predominance of the products of weathering, but the pattern of joints is still represented by the existence of stones embedded in those products. In zone III, products of weathering are subordinate to core stones, which are visibly in their original relationships and may exhibit a sort of jig-saw fit. Now though this classification was devised in relation to weath-

[1] B. P. Ruxton & L. Berry 1957 — Weathering of granite and associated features in Hong Kong. *Bull. Geol. Soc. America,* vol. 68; p. 1263—1292.

ering in the tropics and in relation to a particular rock, namely granite, it is obviously of general application and can, with suitable modifications, be applied to any rock. The only change which I would suggest making in this zonal nomenclature concerns the numbering of the zones. Ruxton and Berry numbered the zones in the order in which they encountered them in digging downwards from the surface. But if we are thinking genetically this is the wrong order. Weathering is a process that proceeds downwards and there is, as Mabbutt[2] suggested, an advancing front to the zone of weathering. At an early stage in the process this weathering front might be the only form of weathering to be found. At a later stage when the front had advanced somewhat deeper the products of weathering in the upper parts of the profile would gradually come to exceed the sound rock in amount, but not until weathering had been in progress for a considerable time — how long would depend on both climate and the nature of the rock — would zone I appear. It would therefore seem better to adopt a nomenclature which is genetic and name the zones in a way that indicates the order in which they arise, as A, B, C and D, and this I have done in a recent paper in the *Zeistchrift für Geomorphologie*.[3] I hope that this modification of Ruxton and Berry's nomenclature will commend itself to you and to other geomorphologists, because I think that as far as possible when the zones are really genetic zones we should attempt to adopt a nomenclature that expresses the genetic relationship.

Now if, as is likely, the pre-Pleistocene landscape of southwest England had suffered weathering for a considerable period, we may imagine that it would be covered by a regolith or mantle of weathered rock of considerable thickness and that all four of these zones would be present over a good deal of the landscape. Here, I am presenting this as a hypothetical assumption, but we can recall that we have some evidence that suggests that the assumption is valid. You will recall the tors on Dartmoor and the hypothesis that I put forward that they represent sound rock that had survived such a period of weathering because the joint spacing was so far apart that the amount of weathering was minimal, and that in different parts of the landscape where the joint spacing varied, quite clearly the weathering front would advance to greater depths where the joint spacing was close than in the areas where it was wide. If we think of the height of the present tors, it can be said that they are normally some tens of feet above the bedrock at their base. Vixen Tor is the loftiest of the Dartmoor tors and on its

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[2] J. A. Mabbutt 1961 — "Basal surface" or "Weathering front". *Proc. Geol. Assoc. London,* vol. 72; p. 357—358.

[3] D. L. Linton 1964 — The origin of the Pennine tors. An essay in analysis. *Ztschrft. f. Geomorph.*, N. F. Bd. 8; p. S*—24*.

up-hill side has an elevation of about 50 feet. All the tors are loftier on their downhill side, and on this side Vixen tor rises 80 or 90 feet. Now this implies a weathered mantle of the order of some tens of feet in thickness in which the four zones A, B, C, D, would be present in varying proportions.

The tors commonly arise from rigdes, spur ends or summits, and this suggests that the whole topography represents a measure of adjustment to structure related to the joint frequency in the granite. Now imagine such a landscape subjected to soliflual processes. If we presume a cold period without change of precipitation the first results of a measure of refrigeration would probably be to diminish the efficacy of evaporation and therefore to increase runoff, rendering the weathered slopes that had long been stable under the existing climatic conditions unstable. They would be out of harmony with the changed climatic conditions. Such increased runoff would probably remove zone D altogether because this material is mobile. To a large extent it can be expected to be composed of the products of weathering. These are in fact chemical precipitates and being of fine silt or clay size are readily mobilised and carried away. They may be redeposited in concavities in the landscape or they may be carried by the rivers as suspension load and disappear to the sea. It is possible therefore that very little trace would be found at any later stage of zone D. Increasing refrigeration would make it possible to move the remaining material as a sludge and this is it seems what happened. But the finer part of this material, including some un-weathered or partially weathered felspar crystals and other relatively coarse materials, might find its way downhill by washing and accumulate in concavities. The sludge would flow downhill bringing coarse material weathered from granite as core stones of varying size embedded in a porridge which would spread over any earlier deposited wash materials. Increasing refrigeration or continuation of the cold period would next lay zone B under contribution, with probably a good deal of it being shattered by frost and the proportion of rock to weathering products increasing.

Professor Waters has drawn for you a section (fig. 1) which is based on a carefully measured section in Shilstone Pit (SX/659902) and supplements admirably the evidence you have already seen in the field [4]. You will notice that the lower part of the section represents the lower part of an undisturbed weathering profile. Sound granite passes up into flaggy broken granite and this into undisturbed growan, sometimes revealing its undisturbed character by the fact that running through it are fine quartz veins, or sometimes tourmaline veins that have been largely replaced by

[4] R. S. Waters 1964 — Pleistocene legacy to the geomorphology of Dartmoor. *Dartmoor Essays.* Ed. I. Simmons (The Devonshire Assoc. Adv. Sci. Lit. & Art); p. 73—96.

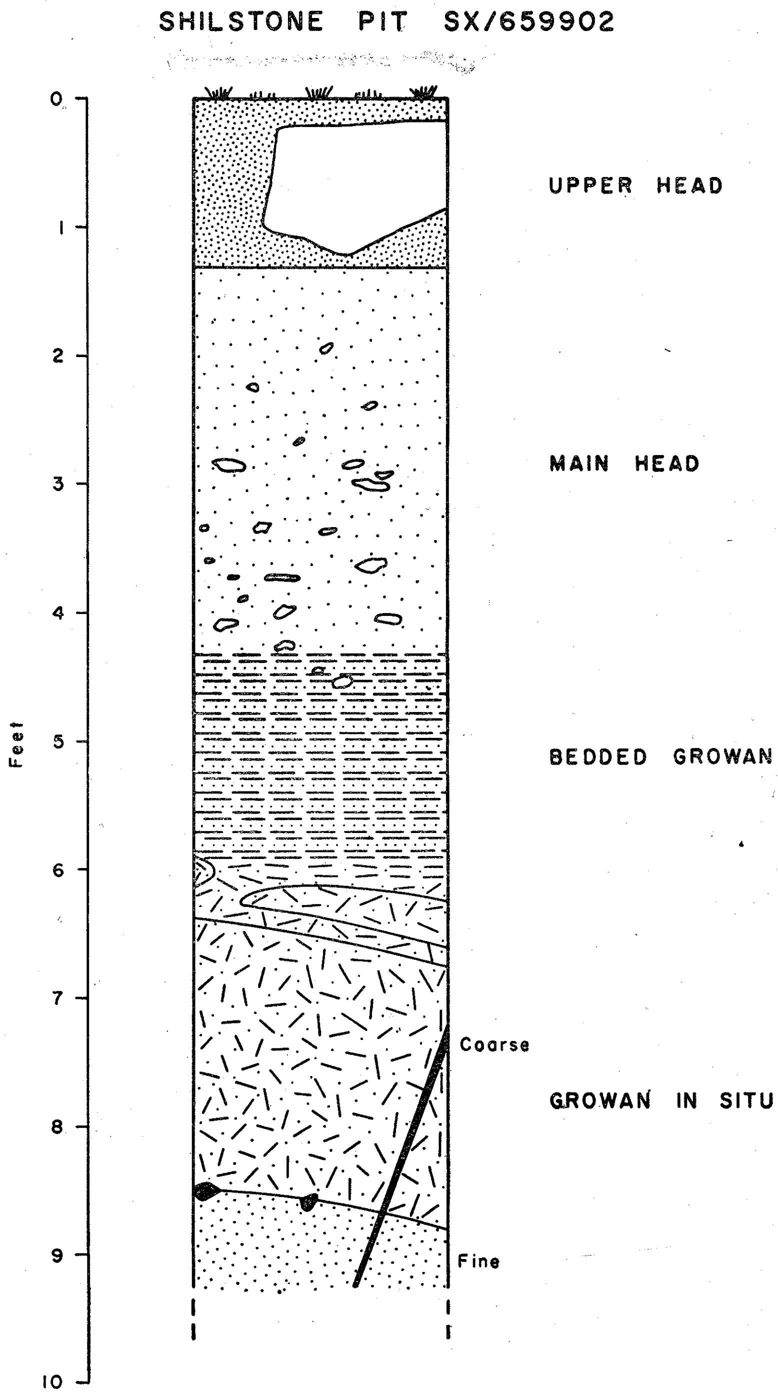


Fig. 1. Pleistocene succession on Dartmoor, as illustrated in Shilstone Pit

joints which gave rise to the tor would also give rise to large core stones in the rather more weathered areas. These would be stones of the kind that are seen surmounting many of the tors like Hound Tor but they would have been completely surrounded by fine grained products, and as these latter became mobile the stones would move downhill and a proportion of them would be broken by frost. Others however might have been embedded in sufficient fine grained material to be rafted down to low levels, and such a mass is shown in the uppermost layer in the diagram. There are in fact several sections in which this kind of succession is topped off by quite large masses of sound granite which may cover 20—30% of the surface area. Commonly they are embedded in the Holocene post-Mesolithic peat and these, one feels, represent the last phase of solifluxion.

I hope this account may serve to initiate our discussion. Possibly to some of you who are inclined to think of solifluxion as simply the downhill movements in a tundra climate of material produced by freeze—thaw processes, and therefore entirely angular and with the regular distribution of grain size from small to large that appears to be characteristic of frost shattered deposits, this may suggest a new view of the constitution of some of the head that we have seen. So with that rather lengthy exposition from your Chairman may I call now for contributions bearing upon this from the members of the Symposium?

Mr Jennings —Would the scheme of development you have outlined permit of some frost-shattering of corestones and other granite blocks as they were moved from their original locations around the residual granite masses, which now constitute the tors? The degree of angularity exhibited in the "clitters" below Hay Tor, for example, would seem to demand some modification of form from that to be expected of subsoil weathering alone.

Professor Linton — Yes, there is definite evidence of fracturing in some cases where sharp-edged plane faces occur. But many quadrangular blocks that appear "angular" at a distance have in fact well rounded edges and have been little modified since they were exhumed from zone B.

Professor Cailleux — The rounding may be a remnant of Tertiary weathering but it could also result from frost action. In Antarctica I have observed and studied this disaggregation of granite. On fresh granite, shattering occurs first and then the block becomes rounded by granular disintegration. So that this rounding can result from frost action. It is interesting to see the word *growan*. In France, we have the same word — *grouan*.

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Dr Pippan — At several places in the western crystalline Upper Austrian Basement Mountains north and south of the Danube, sections occur where grus (growan) is overlain by Upper Pliocene quartz gravels which can attain up to some metres in thickness. The grus must be earlier than the Upper Pliocene gravels at the top. It developed from the later middle Miocene to the early Pliocene under the influence of a warm, moist climate. The grus often shows an intensely red colour. In this area four generations of block formation can be found:

(1) Bare, little weathered, mostly sharp-edged blocks which are united to form a block river or sheet.

(2) More strongly weathered blocks, partly united by soil blocks which are overgrown by moss and herbs. The edges of blocks are a little blunted.

(3) Rather weathered blocks, partly packed into soil and strongly overgrown by bushes and sometimes even trees. Mostly the edges of the blocks are well-blunted or somewhat rounded.

(4) Strongly weathered blocks, thoroughly packed into soil and densely overgrown by herbs, bushes and trees. This kind of block sheet cannot be recognised as such from the surface but only by exposures. On steep slopes the edges of the blocks are well blunted or rounded, on gentle slopes less so.

The disintegration of rock into blocks does not only occur under the influence of a periglacial climate. The process is both complicated and differentiated and cannot be explained in a schematic way, merely in terms of periglacial processes. There exist several possibilities of block formation under different climatic conditions and at various altitudes. The character of the rock and its intensity of jointing are more important than a specific climate. The disintegration into blocks can proceed under warm-moist, periglacial, interglacial or recent climatic conditions; in the first case by grus formation, in the other cases by frost action. It can even be active below a soil or plant cover, by roots and seepage following the joints of the rock. The blocks can be separated by rain wash or migration of grus. Because of these highly differentiated possibilities of block formation the four block generations cannot simply be correlated with the four glacial periods.

Professor Dylik — After seeing the problems and the details of the evidence in southwest England I would like to describe what we have in the Sudetes. There is no doubt that we have traces of solifluxion and mechanical weathering of the Pleistocene cold conditions. One supposes that there has to be deep Tertiary weathering, but as yet no evidence of this has been found. The particles in the growan are mostly angular or sub-angular and observations on this seem to be most important. The question of the warm weathering is also rather doubtful. I am not going to say that in the Sudetes and also here that this chemical weathering did not occur. It is very probable that it did, but there is little direct evidence of it in the Sudetes. There are no traces of kaolinite except some infillings in little fissures. Perhaps this is not enough to afford evidence for warm weathering.

On the Dartmoor excursion we saw abundant evidence of decomposed granite, and this prompts me to pose another question. Must this decomposition be Tertiary? In the interglacials there were suitable conditions for the production of clay minerals. To answer the question we must try to find the weathering products not here but much farther away. If there was Tertiary weathering, probably there must also have been Tertiary or early Pleistocene mass movements, and if we have traces of Pleistocene processes on existing slopes the pre-existing weathered material must have been removed long ago.

Professor Linton — Professor Dylik has introduced an important point which must remain a hypothesis which is unproven until the mineralogy of the deposits has been worked out. Though I spoke in terms of weathering in later Tertiary periods there is good reason to think that weathering in the interglacials went far enough to produce results of this kind. The tors that we saw on Dartmoor are at high levels and quite possibly do represent survivals from pre-Pleistocene times; but there are tors at lower levels here and elsewhere, such as Brittany, that cannot be thought of in these terms. If the hypothesis that I have adopted applies to them, then they must be the result of later interglacial weathering and glacial conditions.

Dr Brunsden — On Dartmoor if you examine the majority of the sections you do find a zonation very similar to that described by Professor Linton. We have, I think, found evidence of chemical weathering and movement of clay minerals along the joint planes. So far, we have identified only two, kaolinite and illite, but they are there — often yellow-stained and usually a millimetre in thickness. In addition, there are indications of the inward weathering of biotite in the granite — spheroidal weathering of some kind.

When one comes to date the tors one finds some interesting relationships. All the decomposed rock occurs almost entirely within the height range of the middle Tertiary surface, i.e. between c. 1000 and 1300 feet. But late Tertiary terraces show little or no granite weathering — tens of feet on the big surface above and little or nothing on the late Tertiary terraces below. This would suggest that there was very little chemical weathering after the big Tertiary surfaces were cut. If one plots the height of occurrence of the tors, one finds almost 70% of them between the early Tertiary and late Tertiary surfaces. The only exceptions are the obvious Pleistocene tors which are like buttresses or ribs in the gorges of rivers leaving the moor. These are of quite different form — angular and shattered.

The problem of the angularity of the clitter of the boulder streams is also related to the weathering. Not only is there a downward increase in the percentage of solid rock but there is also a downward increase in angularity. Blocks coming out from the low zones are all angular.

Professor Rudberg — May I add two observations concerning features in modern periglacial environments? In Arctic Canada (Axel Heiberg Island) this type of granular and spheroidal weathering is found in basic rocks (gabbro, dolerites). Core stones occur standing on top of each other with weathering fragments all around them. The other example is from the high mountain areas in Scandinavia, high up in the blockfields and boulder fields. There, almost all the boulders are angular products of post glacial weathering. Some rounded boulders occur in between and these are erratics from distant areas. Their edges are fractured by frost weathering and some steep *roche moutonnée* forms are preserved on their tops. Snow banks below them have caused the marginal weathering. Circles and solifluxion lobes occur and result in very even surfaces when seen from a distance.

Mr Stephens — May I make one general comment concerning the diagram on the board compared with the head deposits on the coasts of southwest England? The diagram seems so simple in the small number of layers above the undisturbed growan by comparison with the number of layers of head that one finds on the coast. The point was made yesterday that there may not be as many layers as perhaps I would believe, but in places one can find multiple layers, six or seven, certainly more than the three above the bedded growan. I wonder, therefore, if the conditions that we have seen on Dartmoor and therefore the sequence of deposits, are precisely the same as those in the head deposits of the coast. I am thinking of a site at Pendower in Cornwall where a cliff some 50—60 feet high occurs with up to five separate layers. It is therefore not easy to equate what occurs on Dartmoor with what is found round the coasts.

Professor Linton — This introduces an important problem which we will discuss later. Dr Pissart like Professor Cailleux has observed that rounded forms are not always the results of chemical weathering, and I would agree. But I would also remind him that angular forms are not always the result of frost shattering. In certain places the growan has aplite dykes in it, and the growan is quite disaggregated but the aplite dykes, which have only been weathered by chemical weathering presumably, are already broken down to considerable depths into angular pebbles. This is found not only on Dartmoor but in other areas, and I have seen it to perfection in Portugal where probable frost action at sea level may be regarded as quite out of the question. Chemical weathering does act in this way along joints and produce angular fragments.

One of Professor Rudberg's observations is of quite general application. Any rock protruding above the surface of a ridge is in a dry position and tends to suffer less from the processes of weathering, whatever they are. Castle kopjes within the tropics and features in the Arctic alike will tend to survive in such positions. And once the inselbergs or tors are produced they tend to have an incredibly long life.

Dr Blake has asked about the possible consequences of exfoliation. Exfoliation is reflected in the architecture of the tors and in the forms of the hills themselves. Indeed, the relation between the tors and the hill forms and the sheet jointing was noted about fifty years ago, and commented on by R. Hansford Worth.

Professor Rhodes Fairbridge — I could not agree more about the primary geological control of the dome itself. In well-mapped areas where there is practically no superficial obscuration by vegetation you can see that the primary structure of the granite intrusion is followed by the hill and there is no question that the hill is reflecting the ultimate structure.

I would also like to make a comment on what happens to the debris in between. I think it is not merely a question of removal by solifluxion or rainwash but also by wind. The concentration of coarse material around the foot of the moors is obvious, but the vast amount of clay mineral that is produced during chemical weathering must be accounted for by streams. Yet frequently the volume does not seem adequate for the removal of this clay. Where has the clay gone? There have been during the periglacial and glacial stages tremendous amounts of aeolian removal during arid phases; many of the surfaces were unvegetated and so we must look to the loess and loess-like deposits of France and southern Europe where deposits of limon are found. Some of the lenticles within the head may also be aeolian.

Incidentally, speaking of deep weathering, I have been down gold mines in Australia where you can go down 600 feet before you reach solid granite: you could manufacture some magnificent tors out of that.

On the south coast of Australia where this type of environment has been subjected to marine erosion the weathered material has been washed away leaving magnificent bare domes marked only by some flaking. It is striking that in the 6,000 years while sea level has been approximately where it is now the amount of marine erosion of these domes in the intertidal zone has been almost zero.

Professor Linton — We will continue our discussion by taking up the second of the general questions, namely, the climatic implications of structural features in the head.

Yesterday afternoon we saw good examples of head which had been affected by frost wedging, and the frost wedges had become fossil. The frost wedges had been overlain by later material. We have also seen cases of these disturbances in the lower bedded growan deposits, cryoturbation of some form, and that has been overlain by head. Now it is clear that the conditions that lead to solifluxion are not the same climatic conditions that lead to frost wedging and possibly not the same as those that are responsible for the other cryoturbation features. There are many of us in this room who work on these deposits in this country and we would be grateful for the views of those who have studied these features in regions where they are at present active, and who could inform us for our future assistance of the kind of climatic conditions under which these several processes take place and therefore what kind of oscillations of climate and what magnitude of oscillations we have to assume when we find these things in combination.

Professor Dylik — When we speak of solifluxion or congelifluxion we think of many processes and many deposits which are not always the same. It is important to bear this in mind when we are considering their climatic implications. I prefer the term *congelifluxion*: solifluxion is found in all climates; congelifluxion is characterised by the presence of permafrost or frozen ground. The process itself and the sediments of congelifluxion are controlled chiefly by the thickness of the active layer and also by the value of the angle of slope. If we have a thin active layer we have congelifluxion in the classic form. From one phase of congeliflual activity we probably cannot expect very thick sediments. In order to have thick sediments we must have repetition of the process many times, and we have to envisage a situation in which the sediments laid down in one summer do not disappear due to rainwash. Where the angle of slope is greater and the active layer thicker, mass movements of greater amounts of material than in the former case occur. Thus congelifluxion is not always the same and probably we have a great family of processes and a great family of corresponding sediments. Where thick masses of material occur with no traces of bedding the climate was not very severe but moderately cold and rather humid; and where the bedding is very distinct this is evidence of many phases of repeated processes of the same type. On moderately steep slopes many of the congelifluxion deposits were reworked and or removed by rainwash. I think this is the origin of the rhythmically bedded sediments. Where large masses of congelifluxion and associated deposits occurred they were preserved by permafrost. They were incorporated into the permafrost and frequently deformed.

In central Poland the majority of frost-fissured polygons are found in the rhythmically bedded deposits. Frost-fissured polygons are the periglacial structures which should be considered as indicators of the most severe and dry climatic conditions. In the same sediments in south-west England and in central Poland this leading horizon is basic to the understanding of the head; it represents the phase of maximum cold and aridity. Thus the deposits below it reflect the occurrence of a waxing phase which was cold and wet and those above it are related to the waning cold phase which was characterised by climatic conditions similar to those of the waxing phase.

Professor Linton — Professor Dylik has spoken of solifluxion associated with permafrost. We would like to know whether you would make this a universal association or whether solifluxion of this kind, congelifluxion as you prefer to call it, can occur where there is no permafrost.

Professor Dylik — Congelifluxion as I understand it is associated with frozen ground, but of course solifluxion processes are not only to be found in areas of permafrost.

Professor Péwé — I would like to consider a point raised by Professor Linton. He suggests that at the end of the Tertiary and in the Pleistocene the total precipitation was the same and the land slope was the same. The only changed parameter was the temperature. This would affect the vegetation and the precipitation, some of which would then fall as snow. Thus there would be more concentrated runoff and in consequence, more material would move downslope during the Pleistocene cold phases than in the Tertiary. But these conditions would not necessarily give solifluxion. For solifluxion to occur precipitation must be released in a different form. If cold, then either seasonally frozen ground or permafrost is required, which is supposed to act as a rigid base in the Spring. Material in the active layer must be over-saturated. Where is the water coming from? Some is derived from snow, but we must not overlook the water that comes from the ground as it thaws and for this it seems that we must have silty material. I have never seen solifluxion in blocky material: nor do I believe in seasonal frost as the basement for solifluxion: it always has to be permafrost. In the dry interior of Alaska, an area of continental (sub-Arctic) climate with a mean annual air temperature of −3 °C, there is no current solifluxion, not even at 1000 feet above sea level. Active solifluxion occurs where the mean annual air temperatures are from −5° to −6 °C, and permafrost is always present.

There are more factors than temperature involved in solifluxion. Material accumulates and freezes as it is deposited. The material builds up a few inches and then it is partly frozen. As it grows it is perennially frozen. If ice wedges grow in the material as it accumulates, they extend continually upwards; but if, as we usually find, they form later, quite a rigorous climate is required. However, I do believe that if the wedge ice is to be replaced, then regionally the temperature must be raised considerably. This is because the silt-rich material is also ice rich, and a large amount of heat is necessary to thaw it. Therefore I would visualise conditions becoming quite warm with mean annual air temperatures of approximately 0 °C.

Professor Linton — Professor Péwé spoke of solifluxion as he knows it. I wonder if we can be sure that this is similar to the head of south western England. One of the most characteristic features of the head in southwest Britain is the lamination or orientation of the large fragments with their axes parallel to each other and pointing downslope. It has been spoken of as bedding, but clearly is not bedding in the ordinary sense, because it is a feature only of the larger fragments. I have used the term *quasi-bedding* for this distinctive feature of the head. It distinguishes it from such terrace features that we have in our own mountains that arise without frost. Does it characterize the deposits that you are speaking of in the Alaskan landscape?

Professor Péwé — Yes, very much so. Twenty years ago I was taking dip and strike angles of the material and then realised that it was head.

Professor Cailleux — True bedded deposits have been photographed by Malaurie at the foot of a slope on the Island of Disko (Western Greenland). I have observed active solifluxion flows on slopes near Finse in Norway, in the Alps near Chambeyron (2600 m), in the Pyrenees (2700 m), in Western Greenland and at the Bunger Oasis in Antarctica.

In all these localities the climate is relatively humid and solifluxion active. But at McMurdo, Antarctica, no solifluxion occurs because the climate is too cold and, in effect, too dry [5]. On Axel Heiberg Island, where the mean annual temperature (−15° C) and mean annual precipitation (< 150 mm) are almost identical with those at McMurdo (−17°C; < 150 mm), the solifluxion reported by Professor Rudberg [6] is made possible by the relatively high summer temperatures (+1° to +5° C as opposed to −3° C at McMurdo) and the receipt of a part of the precipitation as rain which together produce an active layer.

Mr Andrews — I would like to stress not only the temperature and moisture conditions necessary for the formation and dissipation of ice wedges but also the importance of vegetation. Though climatic change undoubtedly influences vegetational changes, the time since deglacierisation is also important in order to allow successive colonisation. With the advent of forest tundra into a former tundra area the thermal regime of the ground is drastically changed by the drifts of snow. This in itself causes a thermal amelioration. Possibly the areas of active and inactive ice wedges in Alaska coincide with vegetation belts.

Professor Péwé — Why would the forest advance? Would this not reflect climatic change? We have on the far west dormant ice wedges where the forest has not invaded. If the forest was established over active ice wedges it would have to withstand a still more rigorous climate.

Professor Linton — We may now take up the question of sea level in relation to climatic environment particularly in relation to what we saw yesterday at Croyde and at Baggy Point. There Mr Stephens showed us with the utmost clarity two raised marine platforms. One at its highest point at 45 feet above Ordnance Datum, the other at about 25 feet O.D. Both of these marine platforms have marine shingle. In both cases this marine shingle, especially in the upper case, is intermixed with head. In both cases they are overlain immediately by dune sand cemented into calcareous sandrock, but ultimately both are overlain by head. We have then evidence of conditions of high sea level and presumably the higher beach was formed before the lower, but I do not think that there is enough

[5] A. Cailleux 1962 — Études de géologie au detroit de McMurdo (Antarctique). CNFRA, No. 1, 42 pp., 28 fig., Paris.

[6] S. Rudberg 1963 — Morphological processes and slope development in Axel Heiberg Island, Northwest Territories, Canada. *Nach. Ak. Wiss. Göttingen, Math.-Phys. Kl.*, No. 14, pp. 211—228.

evidence to show that the lower cuts into the higher. We have these two benches formed in succession, both overlain by head. And if the apparent evidence that we saw at Pencil Rock of the intermingling of the beach shingle with the head is to be taken at its face value then it would appear that the sea was at this high level when cold conditions, suitable for the production of the head, obtained. We have here a paradox in terms of glacio-eustatism, the conjunction of a high sea level with a cold phase. Some of us would be grateful if Professor Rhodes Fairbridge could throw any light on this problem.

Professor Fairbridge — I think that it takes 800 million cubic metres of ice to lower sea level by one millimetre and so you could have quite a nice glaciation and hardly lower sea level at all. To have a glaciation and lower sea level by 100 metres requires continental glaciation on a vast scale covering half the North American Continent and half of the European continent as well. But it seems to me that here we can have the necessary conditions at the beginning of a glaciation. There is a definite time lag between the amelioration of climate and physical reaction to that change. We can date this by radiocarbon methods in the case of deglaciation. We cannot date the onset of a glaciation, and the timing is somewhat different bacause of the latent heat factor above all else. The amount of heat required to melt the ice is tremendous and is only applied to the upper surface, whereas in the case of cooling this is effected throughout the whole atmosphere; each individual grain of snow is affected. So that we must expect the onset of a glaciation very rapidly and the deglaciation very much more slowly.

Looking at the record that we have of the last 20,000 years we find that the air temperature curve, on the basis of the palaeobotanical evidence, is different from the eustatic curve, the measure of ice melting. During deglaciation there was a long delay between the rise in temperature and the rise of ocean level and I suspect a similar delay in englaciation, but not such a great one. So a waxing cold phase might have been contemporaneous with a high sea-level.

Mr Stephens — If, as Professor Fairbridge suggests, the very large erratics were distributed during a phase of advancing ice (that is, at the beginning of a glacial phase) then one has to account for the position of the erratics on a rock platform below the main head. This head would surely have formed as the glacial phase replaced the previous interglacial, and thus the time interval would have been very tight to get the erratics into position before the head. If the erratics were distributed at, say, the beginning of the last glacial period, then this would make the head also of last glaciation age, and because this head rests upon the raised beach then, by comparison, the Fremington boulder clay would also have to be of last glaciation age because it too rests upon the same beach gravels. But the Fremington till is seen in places to be weathered to a depth of from 3 to 8 feet, and its exact equivalent in the Isle of Man, in Ireland and Cheshire shows weathering to a total depth of 14 feet. The depth of weathering in tills north of the southern Ireland end moraine, which is regarded as the limit of last glaciation ice by Irish workers, seldom exceeds 1 foot.

Thus we are faced with an acute problem over the age of the erratics and the age of the heads. The evidence in southern Ireland argues for at least some of the heads being of Saale age, and for the Fremington till being equivalent to the Eastern General (Irish Sea) till which is regarded as Saale age in Ireland. I should like to hear people's views on the amounts of weathering in tills, as I find it difficult to attribute a till, which shows up to 4 metres of weathering, to the last glaciation. May I add that I have yet to see it proved in the British Isles that till of the last glaciation is deeply weathered anywhere.

Professor Péwé — In some areas of Wisconsin or Würm moraines 12 to 15 feet of weathering (decalcification) can be found; but the climate may have been warmer.

Dr Brown — Weathering zones on mid-Western tills are substantially thicker than they are on those in this country of comparable age. In Mr Stephens' case I would be much more worried about the discrepancy between a weathering of 4 metres on a till in Ireland and 1 metre on a till Devon; and yet the implication is that these belong to one and the same period. This is a discrepancy that needs to be explained.

Mr Stephens — Yesterday you saw only 1 metre of weathering, but over a period of years at Fremington 1 metre becomes 2 or even 3 metres of weathering as the position of the face of the section is changed.

Professor Linton — We cannot allow the symposium to open up a completely different field. We should have to establish different criteria of weathering, and I am not sure how far Mr Stephens is referring to decalcification or to other mineralogical changes and we could easily have a fruitless discussion by not talking about the same thing.

With your permission I am going to thank all the speakers who have contributed to the discussion and to ask them to let Dr Gregory or Professor Waters have a fuller note of their contributions while the proceedings of this symposium are still fresh in their minds.