TASMANIAN PHYSIOGRAPHY.

DISCUSSION ON "NOTE ON THE ISOSTATIC BACKGROUND OF TASMANIAN PHYSIOGRAPHY," by A. N. Lewis.

[Papers and Proceedings of the Royal Society of Tasmania, 1926, p. 1.] (11th April, 1927.)

Plates XV.-XX.

(Read 12th September, 1927.)

Mr. P. B. NYE, M.Sc., B.M.E., Government Geologist.

Like Mr. Lewis, I only desire to set forth my views as a contribution to Tasmanian geology. I cannot see eye to eye with Mr. Lewis in many points in his paper and particularly in regard to some of the major conclusions.

In the first place, in view of Dr. Walkom's papers on the Mesozoic fossil flora of Tasmania (Walkom, 1924-1925), I think we ought to drop the term Jurassic as applied to any rocks in Tasmania. Fossil evidence points rather to their being Triassic and this period will probably be found to include the whole sandstone series. Dolerite intrusions occurred throughout the world at this time and Du Toit puts them in the Upper Triassic. We may therefore consider with a reasonable degree of certainty that the date of the diabase intrusions in Tasmania was late Triassic and that we have no more recent sedimentary rocks except the small patches of Tertiary deposits. In regard to the classification of the sedimentary series particularly dealt with in Mr. Lewis's paper, this cannot be regarded as wholly fixed. For example, near Mt. Nicholas a coal seam occurs beneath some limestone beds. Is this a member of the Greta series or have we an upper limestone above the Greta series? I do not think that the Knocklofty Sandstones are higher than the felspathic sandstones and at present they appear to resemble the Ross series but I would like to undertake further field work before definitely arriving at the above conclusion. Also, it cannot be said that the whole sedimentary series represents a continual and unbroken era of deposition. At Glen Morey, York Plains and Jericho, a bed containing pebbles belonging to the Lower Marine series is included in the felspathic series. This indicates the occurrence of differential earth movements at some time between Permo-Carboniferous and felspathic sandstone deposition.

In regard to the diabase, the opinions of the Geological Survey in 1922 quoted by Mr. Lewis require some explanation. During the work in the Midlands undertaken when compiling my report on the Underground Water Resources of Tasmania, I came to certain conclusions. Later the Survey undertook the work referred to, *The Coal Resources of Tasmania*. At the same time as I was working in the Midlands, Mr. Keid was working on the East Coast and Mr. Reid in the South. Mr. Hills wrote up the results of our investigations and, without consulting me, included my opinion of the structure of the Midlands in a general statement intended to apply to the whole of Tasmania. Mr. Lewis is now doing a similar thing by applying his knowledge of the south-eastern districts to the whole of Tasmania. This is a procedure against which I should like to warn him.

My original statement, the one which Mr. Hills repeated, is true for the Midlands but in areas in which older rocks outcrop it is an entirely different matter. We must distinguish between these two cases which are entirely separate. When a basement of earlier Palæozoic rocks can be seen, the conditions are very different from those pertaining in the Midlands and eastern portion of the island where the series under discussion (Permo-Carboniferous to Triassic) is much thicker.

Turning to the most controversial portion of Mr. Lewis's paper, I cannot agree that the faulting occurred towards the early Pleistocene and adhere to my previously published opinion that the origin of our present physiography dates from the time of the diabase intrusions. I will deal with the data Mr. Lewis puts forward on p. 17 of his paper in support of his view.

1. As to the features of our greater valleys I merely say "which do not possession the characteristics of water "erosion."

2. I repeat my views published in Underground Water Resources Paper No. 1 and No. 2 and elaborated in the diagrams attached thereto. "All these intrusive masses have "the appearance of arising from a large underlying mass "situated at no very great depth below the surface. . . . . "These intrusions were closely connected with much-fault-"ing in the strata, and both these events are probably con-"temporaneous with the cessation of deposition of the Trias-

"Jura sediments and their elevation by earth movements. ". . . . The intrusion of the diabase was followed by a "long period of denudation which, in most portions of the "area, has continued uninterruptedly up to the present time "and has been mainly responsible for the productions of the "present topographical features."

In several places I have seen definite proof that this was so. For instance, narrow dykes with different sedimentary rocks on each side, indicating a fault through which the diabase welled up. We do see the core of diabase and the remains of the retaining wall of sedimentary rocks on the sides of the highest mountains. There certainly is no trace of an overflow as far as our present evidence extends. Many thousands of feet of covering rock have since been removed by erosion.

3. I do not think Mr. Lewis means this. If a sill occurs as he states it surely must determine the form of the mountain.

4. I agree that where there is a sill, sedimentary rocks below it cannot have been raised by that sill. Any elevation of such rock would be caused by the transgressive bodies in the vicinity and not by the overlying sill.

5. I have seen many cases of diabase cutting inclined sedimentary rocks at many different angles up to 90deg. I have also seen a tabular body cutting inclined strata at a very low angle.

6. The mountains have not been in existence since Jurassic times but have been forming since that time and have only now reached their present stage of erosion. The interval of time is not too long considering the many thousands of feet that have been removed. The present configuration of the surface depends entirely on the form assumed by the blocks hoisted up or otherwise by the diabase intrusion and subsequent history has simply been the removal of the sedimentary series from the hard igneous rocks. Cliffs and juvenile drainage quite follow from a normal development since Triassic times by water erosion.

In regard to the Tertiary peneplaination, Mr. Lewis places great reliance on the existence of pebbles of rocks foreign to the neighbourhood over much of the area under discussion. These pebbles certainly do exist but the sources are near at hand. The glacial series is full of such and they are scattered through the remaining members of the sedimentary series. With the erosion of these rocks the pebbles have accumulated on the surface.

The most convincing argument against Mr. Lewis's views occurs in the Avoca district. There you can find many faults and one with a throw of 2,000 feet but the pebble beds and overlying basalt lie uninterrupted over the top of the fault, indicating a long period of erosion between the faulting period and the deposition of the sediments. This fault is typical of many others occurring in north-eastern Tasmania. This indicates that no differential movement has taken place since the diabase intrusions. This statement is certainly true of the Midlands, East and South-East. It does not necessarily hold good in localities where the older platform has been exposed to view. I do not see any reason to change my previously published views.

Mr. W. H. CLEMES, B.A., B.Sc.

Mr. Clemes identified himself with Mr. Nye's remarks. He also felt that he could not reconcile the field data with Mr. Lewis's theory of Tertiary block faulting. In particular, the diabase we now see must have consolidated at a great depth. This was indicated by the large size of the crystals. The amount of erosion that must have occurred to expose this rock required a much greater time than Mr. Lewis had assigned to it. Mr. Clemes instanced the large size of the crystals on the summit of Mt. Olympus (4,500 feet) and the amount of erosion effected by the Ouse, as compared with the Shannon. He also instanced the beds of pebbles, as at Lindisfarne largely mingled with chips of fossil conifers which were derived from eroded Permo-Carboniferous strata. Mr. Clemes also said that he could not agree that Tasmania had remained nearly at sea level until towards the close of the Tertiary Period, as otherwise, from whence did the sediments come?

Mr. L. F. GIBLIN, D.S.O., M.C., B.A.

Mr. Giblin raised several points, amongst them being—

(1) Is there any necessity to assume a special magma? Will not general isostatic principles sufficiently explain these intrusions?

(2) Can the accordance of mountain tops be taken as evidence of a peneplain? Is this not equally evidence of a similar action all over the country by the diabase magma?

(3) Cannot some finality be reached in regard to the nomenclature of this rock? Should it be called "diabase" or "dolerite"?

In regard to the last point Mr. Lewis explained that the correct petrological classification was "dolerite," and Mr. Nye said that he absolutely agreed but that the word "diabase" had been so long used by the Geological Survey that it was inexpedient to change it.

Professor E. J. C. PITMAN, B.A., B.Sc., also made some remarks from the point of view of mathematics.

Mr. A. N. LEWIS.

Mr. Lewis Replied to the Discussion Generally, but Took Time to Reduce His Reply to Writing, and It was Read (in Title) on 12th September, 1927.

I think this question is of such fundamental importance that no efforts should be spared until it is satisfactorily solved. I greatly admire the work done by Mr. Nye for Tasmanian Geology but at the same time I suggest that the ideas advanced by myself are worthy of more consideration in the light of field evidence than has yet been afforded them.

I unhesitatingly accept Mr. Nye's view that Triassic is the correct correlation of the rocks under discussion. Hitherto the age of the dolerite has been placed as Cretaceous. I suggested that Jurassic was probably more correct than Cretaceous and, for the reasons stated by Mr. Nye, I agree that I was not quite bold enough in my correction.

I cannot agree, however, that there is any marked distinction between the occurrence of the dolerite in the areas in which the older Palæozoic rocks outcrop and those in which they do not. In my view, the localities in which the old early Palæozoic floor of the sedimentary series is exposed are the best places in which to study the occurrences of the dolerite. We must postulate the accumulation of several thousand feet of newer sedimentary rocks on the eroded surface of Cambro-Ordovician, Silurian, or Devonian rocks. This platform was gradually sinking or being pressed downwards. The sedimentation period was suddenly concluded by the inruption of the dolerite through this platform into and perhaps over the newer rocks. To-day, through the combined action of uplift and erosion, the older rocks in some places have been exposed and the way in which the dolerite affected them may be studied. In a few localities this effect may be seen and also remnants of the overlying newer sedimentary series are preserved, enabling the study of the effect of the dolerite on both. It seems

to me that what occurred there probably occurred deep below the present surface of the Midlands plain, the Central Plateau and other localities in which only the upper members of the sedimentary series are exposed. Further, that the true physiographical subdivision of Tasmania is not into areas in which older rocks occur and areas in which dolerite and Permo-Trias sediments predominate, but into elevated tracts and non-elevated tracts, irrespective of their respective constituent rocks.

I now turn to elaborate further the contentions in my paper which Mr. Nye specifically attacks.

1. This question of the ordered sequence of the erosion of our valleys by river erosion was the one which first put me on inquiry in regard to the subject matter at present in dispute. Take first the valley of the South Esk. If we presume an orderly sequence of river erosion we must start with an original plateau. Into this the original rivers must have carved valleys near its edge, presumably near the sea. These valleys would then be widened and deepened and would cut back into the plateau by headward erosion until a sharp divide was all that was left of the original surface. From this divide there would be a traceable sequence of slopes to the eroded plains below. This slope may be irregular and would probably be steep at the head of the valleys and would lessen and widen as the maturer plains were approached. But throughout, a study of the surface should reveal the process whereby the river system has progressively deepened and widened its valley. Instead of such features we see a plain over 50 miles by 30 miles in its widest parts, thoroughly mature in topography and covered with river drifts that have been accumulating through several geological periods. Bounding this plain we have the Central, the Ben Lomond, and the Eastern Tiers Plateaux. These have been barely affected by erosion of the headwaters of the South Esk. Their topography is that of the earliest undefined drainage stage of recent uplift. They meet the mature plain with an escarpment 2,000-4,000 feet high and barely seamed by the rivers flowing over it. This escarpment does not occupy, as a rule, a mile of horizontal distance. To postulate an orderly sequence of river erosion, we must presume a backward regression of almost a cliff face for any distance up to 50 miles over a broad expanse of country without marked erosion of the plateau surface above. The same arguments are applicable to the southern valleys, although here the features are more con-

fused. In the case of the Derwent we would have to imagine that, while the main river could erode a deep valley 150 miles long, its tributaries would leave almost unmarked the bastion of the Mt. Wellington plateau, 4,000 feet high, and with an almost unaffected surface, right at the spot at which the erosion of the river valley must have commenced. The Huon-D'Entrecasteaux Channel valley presents the same problem. It therefore seems to me that our major valleys and plains have not been entirely carved from an original plateau.

2. I absolutely grant and hope that I have never been taken to deny that the dolerite intrusions must have been accompanied by much faulting and that in many places the dolerite intruded upwards through such breaks. My present contention is that any direct effect of this faulting on physiography has been removed by erosion and that our present mountains and plateaux are the result of a second and very much later series of earth movements. Mr. Nye instances examples of considerable faults traversing the Midlands plain. These, I submit, are due to the earlier earth movements. They are apparent to-day as a feature of the largest segment that was not affected by the more recent movements. The fault lines to which I call attention are represented by the escarpments several thousand feet high which bound this block, and I suggest that the existence of faults traversing a block which was not uplifted by more recent movements or one which was so affected do not affect the arguments in favour of such uplift. We do not see any welling up of dolerite through the faults which I ascribe to the more recent phase, and because we see it in the case of the earlier one is not, in my opinion, an argument against the contention that some of this country, faults and all, has been bodily uplifted while other areas have not been so affected.

3. If a sill were intruded below sea level and subsequent deposition demonstrated that the area was below sea level for a long time after the intrusion and, later still, it was elevated into a mountain and the sill thereby exposed, could it be argued that the sill determined the form of the mountain? This has hardly been the case in Tasmania, but I submit that the principle is the same.

4. Mr. Nye's argument under this head is the one he has adopted before but in countless instances I have been unable to find any field evidence of these transgressive bodies. I have followed this point out in very many places

in the field, and feel that in most cases where massive beds of sedimentary rocks flank a dolerite-capped mountain, their existence at their present elevation cannot be due to dolerite below them. To give one or two instances. In the case of Mt. Wellington the sedimentary series is unbroken from just below the Organ Pipes (3,300 feet) down to the Cascades (about 400 feet), and there is no trace of transgressive igneous rock to cause the elevation. The Central Plateau and the Ben Lomond Plateau rest on a foundation of sedimentary rocks. If these rocks have been elevated by dolerite below they would form a curious sandwich with the following arrangement downwards:—Sandstones (small depths but much eroded), dolerite (2,000 feet), mudstones and limestones (1,000-2,000 feet), dolerite (unknown depths). La Pérouse forms the best illustration but I have sufficiently described it earlier.

5. In my original paper the paragraph which is the subject of these remarks is perhaps not expressed to convey my meaning quite accurately enough. It should read, rather: "When a sill can be observed intruding tilted strata "the sill very frequently conforms to the dip of the strata. "It may have been intruded thus but in many cases the field "occurrences give the impression that, subsequently to the in-"trusion, the strata, with the included sill, have both been "tilted." Of course, dyke and laccolith occurrences cut across the strata.

6. Professor Arthur Holmes, in his recently published excellent little book, *The Age of the Earth*, gives the current opinion that from 205 to 290 million years have elapsed since the end of the Triassic Period. Also, on p. 9 he shows that the average rate of erosion is 1 foot every 3,000 years in low-lying country. According to these calculations, after the elevation of our present mountain masses by the dolerite there must have been about 70,000 feet of sediments above the present surface of the mountain plateaux! The considerable elevation and the extreme erosion especially during the ice age would probably require a greater accumulation of rock mass to permit the present mountains existing until the present day. In spite of the erosion of such enormous mountains and in spite of the lapse of such a length of time, the drainage of our plateaux is now so immature that over great areas the direction of the drainage is still undetermined and features of erosion are absent!

The maximum possible covering of sedimentary rock above the present dolerite capping can be approximately

estimated. Dolerite at La Pérouse intrudes the upper levels of the felspathic sandstones; on Mt. Wellington, the lower levels of this series. Similarly on Ben Lomond, and on the Central Plateau and Mt. Field, to give only occasional examples, it occurs near the top of the Ross series. Ample evidence exists to show that the dolerite intruded just as the deposition of the felspathic series as we now see it was concluding, and that there did not exist any great depth of rock above that series. The Geological Survey in *The Coal Resources of Tasmania* give 2,350 feet as the greatest thickness of the Ross series, felspathic sandstone series, and Mt. Nicholas sandstone series together. I think a considerably greater thickness is disclosed at Catamaran. But even putting the original maximum depth of these beds at 5,000 feet it can hardly be conceivable that it would take over 200 million years to erode such soft rocks at such an elevation. The time factor is the most convincing argument I can advance for my proposition and I am content to base my opinion that our physiography was not finally initiated in Triassic times on the view that had this been so the whole country would many periods ago have been reduced to a most mature peneplain. Around Hobart many channels of streams that only flow after heavy rain have been eroded to a depth of 12 feet and more since the land was cleared—say in the last 75 years. The silt that has formed in the Tamar in the same time gives some indication of the effects of erosion. Tertiary basalt (about 30 million years old according to Professor Holmes's calculation) has been cut through to the depth of 200 feet by the Elizabeth River in the Midlands, and by the Forth, Emu, Blythe, and many other rivers of the north coast, all at elevations little above sea level. Could the very little harder dolerite be supposed to stand barely affected for over 200 million years? At the Cataract Gorge, Launceston, the South Esk flows over a bar of extremely hard and coarse dolerite on the top of which are Tertiary pebble beds. Mr. Nye thinks that it is merely superimposed on a mass of dolerite having reached it after eroding through Permo-Carboniferous mudstone, while it seems to me that it is flowing over a late Tertiary fault scarp but in either case it has cut 400 feet or more into the hardest dolerite since the deposition of the Tertiary pebbles, say, in, at very most, 30 million years (although I think 5 million would be nearer the mark). I think this is sufficient indication of the answer to the query of whether the dolerite cappings of our mountains could persist for 200 million years.

In regard to the pebble beds, I admit the force of Mr. Nye's argument on this point. The explanation of the origin of these pebbles depends on the view adopted as to the sequence of geological events, and therefore their occurrence is of little use in deciphering these events.

In regard to the Avoca fault, I have sufficiently dealt with this point earlier. Obviously, a long interval of time elapsed during which any physiographical features were removed between the occurrence of this fault and the deposition of the covering pebble beds and basalt lava flows. I do not wish in any way to dispute this, but I do say that this fact is no evidence that an existing escarpment 3,000 feet high near by was not caused at a later date.

Passing now to the comments by Mr. Clemes, I can only draw attention again to the fact that must never be lost sight of, namely, that we have in many places occurrences of dolerite which have intruded all strata from the Cambro-Ordovician to the Upper Triassic, and these occurrences show great variations in themselves but no petrographical means of distinguishing between deep-seated intrusions and those which must have occurred near the surface has yet been discovered. Size of crystals as an indication of depth of intrusion is a dangerous guide. No farther away than the Domain a gradation from a glassy, entirely microcrystalline basalt to a coarse holocrystalline dolerite with crystals up to 1cm. in length can be traced in a depth of 200 feet.

I do not suggest that all covering rock must have been eroded since the uplift. On the contrary, I think I have fully explained my opinion that in many cases the sills were exposed and denuded, perhaps as early as the Cretaceous Period. I feel that the case is not that I have not left sufficient time to elapse since the final uplift, but that other workers have not grasped the enormous length of time that has elapsed since Triassic times and the improbability of our mountains persisting since that period. Lastly, Tasmania has only had its present boundaries since Pleistocene times. Pebble beds may have been derived from east, west, or south.

As to Mr. Giblin's remarks, I feel after further thought that his first query is unanswerable, and I must modify my original paper to that extent. There is probably no reason for assuming a "special magma." Relying on Tasmanian evidence alone we could presume an increase in heat due to the effect of radio-active minerals and re-

sulting in a rise in the upper level of the zone of flowage which was relieved by the diastrophic movements of the early Devonian period, evidence of which is visible in our granite batholiths. The remainder of the Devonian and the Carboniferous periods were ages of quiescence. By the Permian the internal heat had so far increased that a more or less perfect isostatic adjustment was possible, which adjustment reached its most perfect balance during the time in which the felspathic sandstone series was deposited, that is, immediately following the dolerite intrusions.

I suggest, that the subsequent happenings were somewhat as follows: Two large adjacent segments foundered, one of these being to the east and south-east and the other to the west and south-west of the present Tasmania. In doing so, they tilted gradually outwards and the results were, firstly a fracturing of the crust over the present location of Tasmania and secondly a gentle squeezing of the subjacent fused rock material towards this fractured zone. This magma, of a basic and wholly undifferentiated facies, then occupied the fissures in the fractured zones caused by the parting of masses of the sedimentary series.

Such an explanation, if confirmed by field evidence, will account for the absence of violence in the intrusion, the presence on different sides of a dyke of rocks of different stratigraphical levels and for the general impression given by the field occurrences of total replacement of some segments by igneous rocks. I must, however, repeat my opinion that all surface features resulting from such disturbances were removed by erosion before the deposition of the Tertiary sediments.

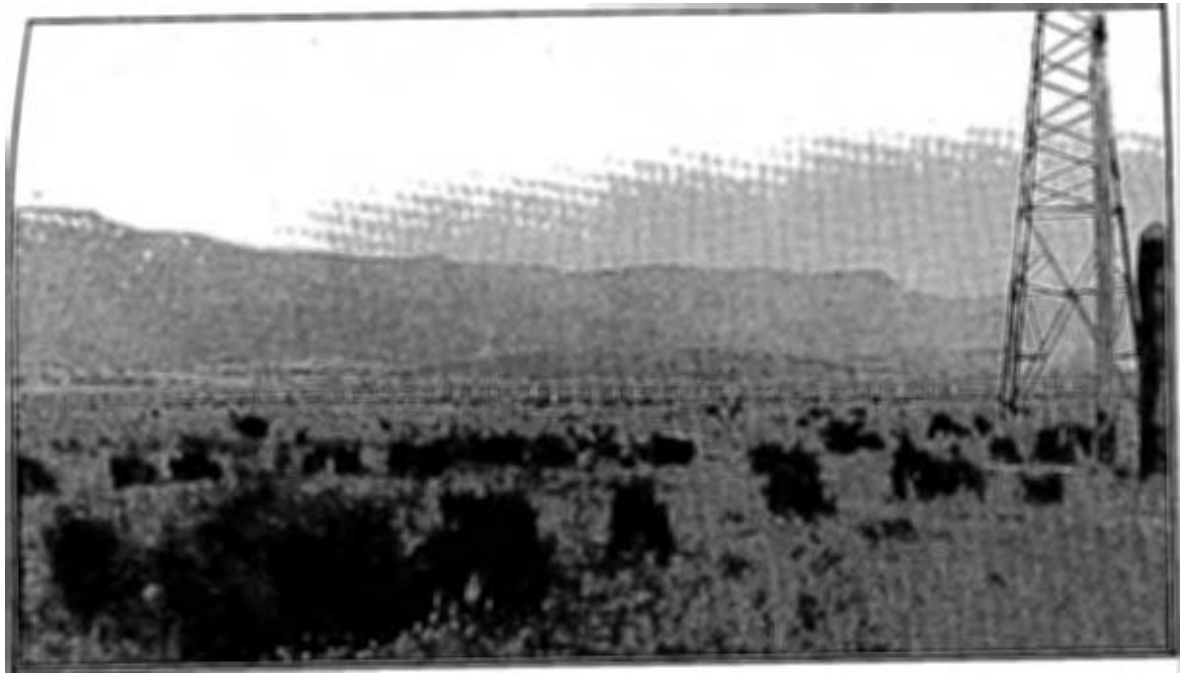
As to Mr. Giblin's second point I merely say: "Perhaps." Accordance of mountain tops is one of the commonest pieces of evidence pointing to the existence of an ancient peneplain.

In regard to the terms "dolerite" and "basalt," I merely say that because a term has been used before it was known to be inapplicable is no reason for continuing its use. Far from it being inconvenient to change the accepted nomenclature, it is most inconvenient to have to repeat to every student and visiting geologist that what you are calling diabase is not diabase and everyone, including yourself, knows that it is not diabase but dolerite but because it was once called diabase it would never do to ever commence to call it by its proper name. (For a further account of the history of the use of the terms "dolerite" and "diabase" see Am. Journal of Geology, Vol. 30, No. 3, Apl.-May, 1927).

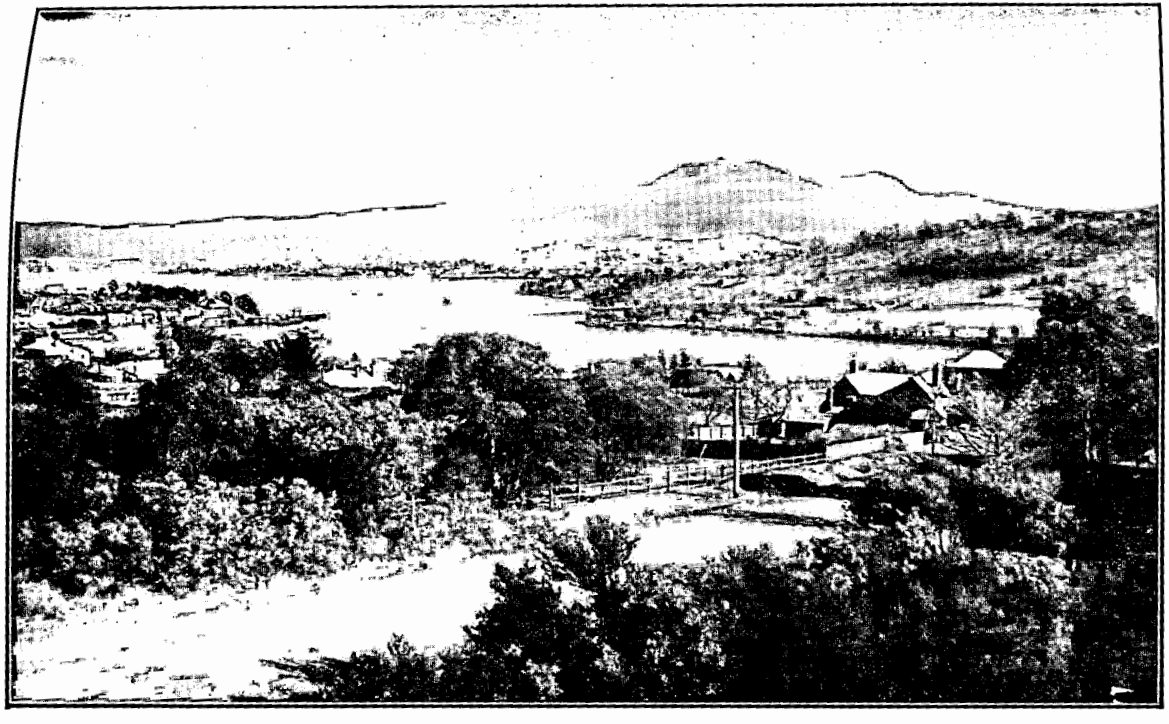
Plate XV.

P. and P. Roy. Soc. Tas., 1927.

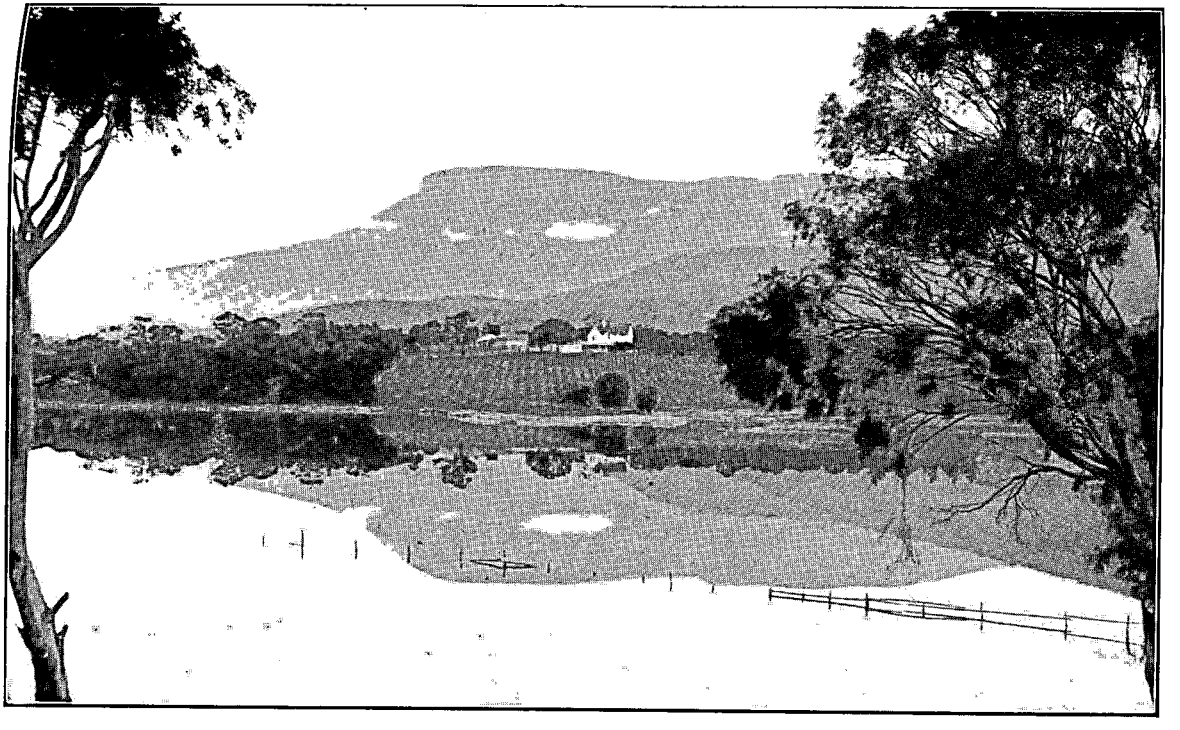
Brady's Look Out and edge of Central Plateau from North of Arthur Lakes. A. N. Lewis, photo.



Western Tiers from south of Cressy. A. N. Lewis, photo.

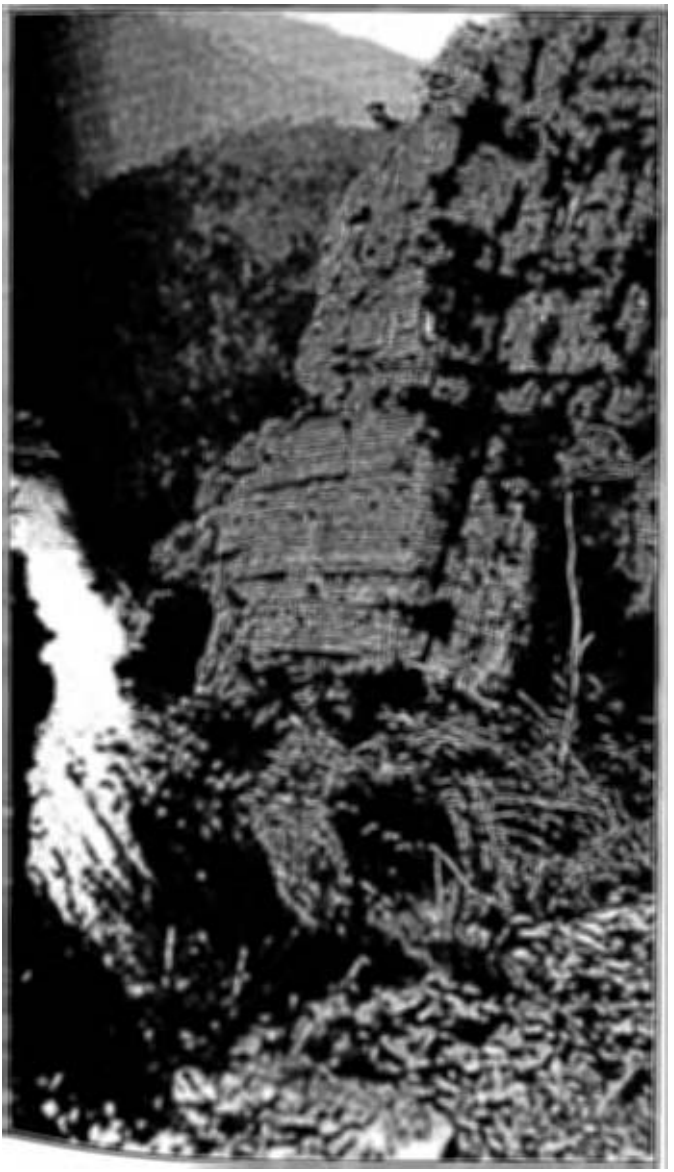


Mount Wellington and Lower Derwent Valley. J. W. Beattie, photo.



Mount Wellington from Claremont. J. W. Beattie, photo.

Central Mersey Plain. Mt. Roland (background). Mole Creek township (right centre).
J. W. Beattie, photo.



Mersey Canyon, Mole Creek.

A. N. Lewis, photo.

EXPLANATION OF PLATES.

PLATE XV.

View from north of northern Arthur Lakes overlooking edge of Central Plateau and showing Brady's Look-Out (4,400 feet) in the background. This view gives a general idea of the general level surface of the plateau broken by occasional prominences.

PLATE XVI.

This view shows the typical escarpmented wall of the Central Plateau and, in the foreground, the mature plain of the South Esk and its tributaries. Only the upper two-thirds of this escarpment are composed of dolerite and the lower one-third consists of the Upper Marine series of the Permo-Carboniferous sedimentary series. The Central portion of the upper edge of the plateau is shown, from the other side, to the right of Plate XV.

PLATES XVII. AND XVIII.

These views of Mt. Wellington illustrate the earlier remarks. Only the upper quarter of the mountain consists of dolerite, which forms a cap on the top of the sedimentary rocks below. Were the Derwent Valley to be considered as entirely waterworn, the main river would have cut a valley 150 miles long while its tributaries have had as little effect on this portion of the valley sides as is shown by these plates.

PLATE XIX.

In the Mersey Valley at Mole Creek, Mt. Roland in the background. The Mersey flows at the foot of Mt. Roland in the centre of the picture and then behind the tree-covered hill behind the village.

PLATE XX.

The canyon of the Mersey. This is taken only a mile or so lower down than the view shown in Plate V., and shows, it seems to the writer, as good an example of the effect of recent block-faulting movements as can be seen. This is, however, only typical of the gorges of many of the rivers descending from the Central Plateau.