Papers and Proceedings

of the

ROYAL SOCIETY

OF TASMANIA

FOR THE YEAR

1911

TASMANIA :

Printed at "The Mercury" Office, Macquarie Street, Hobart.

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Royal Society of Tasmania

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*Fellows who have contributed Papers read before the Society.
†Life Members.
(The addresses of Members residing in Hobart are omitted.)

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*McAulay, Professsor Alexander, M.A.
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Macfarlane, Hon. James.
Macgowan, E. T., M.B., B.S.
*Macleod, P. J., B.A.

Nicholas, George C., *Ouse.*
*Noetling, Fritz, M.A., Ph.D.
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Nicholls, H. Minchin.

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Pratt, A. W. Courtney.
Purdy, J. S., M.D., D.P.H.

*Ritz, H. B., M.A.
Roberts, E. J., M.B.
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Robinson, J. Moore.
*Rodway, Leonard.

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Wolfhagen, Waldemar.

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Note—Fellows are requested to notify any errors in their names, titles, or addresses. List closed December 1st, 1911.

Royal Society of Tasmania.

ABSTRACT OF PROCEEDINGS.

APRIL 10th, 1911.

The first monthly general meeting of the Society was held at the Museum on Monday evening, April 10th, 1911. Mr. R. M. Johnston occupied the chair.

Mr. L. Rodway read a paper on "The Hymenogastraceæ of Tasmania." It was stated in Cooke's work on Australian fungi that Tasmania was poor in fungi of this kind, but it was really the richest country in the world in them. He had personally discovered 40 or 50 new species, besides others which were common to other countries as well. A description was then given of the characters of this family of fungi, and the various species which it contained.

Dr. Fritz Noetling read a paper on "Percussion Marks on Siliceous Rocks," describing the methods by which stone implements were detached from the parent boulders of which they originally formed part, and pointing out how, from the character of the markings upon them, they must be due to human agencies.

The Chairman said that Dr. Noetling had done masterly work in regard to our native flints, but he did not think that they should be prepared to accept all the markings that had been described as due to human agency. While supporting Dr. Noetling in the main, he was not prepared to follow him to the full conclusions he had arrived at.

Dr. Noetling explained that the appearances he had described in stone implements could be produced in any homogeneous substances by a smart blow.

Mr. R. Hall read a paper in regard to the feather tracts of Sphenura, a bird found only in South-West and South-East Australia. Two species were found in each place, and the position of the birds scientifically was somewhat uncertain. It was hoped that a study of the feather tracts would enable their relationships to be better understood.

Amongst the specimens exhibited was a fine piece of agate from Broadmarsh, and a small fish, which is occasionally found round the Hobart wharves. Locally it is known as the "blue-eye." It has been identified as Trachynops teniata, hitherto regarded as a N.S.W. species.

MAY 8th, 1911.

The general monthly meeting of the Society was held at the Museum on Monday evening, May 8th, 1911, Hon. G. H. Butler, a vice-president, in the chair.

Mr. R. Hall read a letter from Professor Orme Masson, of the Melbourne University, president of the Australasian Association for the Advancement of Science, requesting him to take steps to form a Tasmanian sub-committee to assist in the collection of funds for the Antarctic expedition to be undertaken by Dr. Douglas Mawson.

Mr. T. Stephens moved that a committee be appointed for the purpose named.

Professor Flynn seconded the motion, which was carried without discussion.

The appointment of the committee was allowed to stand over until next meeting, the secretary stating that he would be glad to receive subscriptions in the meantime.

Mr. J. A. Johnson moved that there be added to rule 42 a sub-section E—Psychology and Education. He did not think it necessary to weary the Fellows present by explaining that psychology and education, either separately or together, formed a science coming within the objects of the Royal Society. In his opinion, the greatest revolution which had taken place in the last 20 years in any department of scientific work was that which had taken place in regard to education. It was as great as that which took place in regard to physical science, when we learnt that this world of ours was not the centre of the solar system. In arguing on behalf of psychology he wished his hearers to understand that he was not arguing on behalf of metaphysics, but for the study of the simpler facts of mental phenomena on which all were agreed, and facts bearing on the work of education. That, he proposed, should be the work of the new sub-section. He knew several engaged in the work of education who would be glad to take part in the work of the society if subjects such as these were included.

Mr. Herman Ritz supported the motion. He considered it too late in the day now to deny that the training of the human mind was scientific work, and for that reason it came well within the scope of the society.

Mr. T. Stephens said that he had the fullest sympathy with the objects of the mover, but he thought they should consider whether such subjects as education and psychology came within the original scope of the Royal Society. The original founders of the society contemplated as its scope physical science and natural philosophy, and it was many years before any attempt was made to introduce innovations. As a member of the profession of education for over half a century, he agreed with everything that Mr. Johnson had said, but he considered that education and psychology were big enough subjects to have a society of their own. There was a good deal to be said in their favour, however, if the majority of Fellows desired them to be introduced as the subjects for a new section of the Royal Society. At the same time, he thought they should be regular. No individual member had the right to propose such a motion as this; it must come from the Council.

The Chairman: It has been before the Council, and the Council brought it up to-night.

Mr. T. Stephens said he would move as an amendment that the proposed new section be given a trial for the current year. If it proved beneficial to the society, it would then be easy to move for its continuance.

Mr. A. O. Green thought that some better reason should be found than the one that these subjects were outside the scope of the society. If they were, they should be included within it as soon as possible.

Mr. L. E. Piesse said that the nresent constitution of the society was dated 1907, and in it the objects of the society were defined as the study of science in all its various branches. It could not be denied that education was a science nowadays. He would sunnort the proposal, as he thought the influence of the society would be very much increased by the inclusion ot these subjects, and that it would benefit by an increase of members.

The Chairman said that he thought that any good attempt to broaden the work of the society was one that they should support. They must broaden their work, and take in all sciences, so increasing the interest of the people in their doings, or else they would go into extinction altogether. He could not see any object in limiting the new sub-section to the current year, as the Fellows could always stop it if they wished to.

Mr. J. A. Johnson: If we cannot keep the section going it will die a natural death, and drop out.

Mr. T. A. Stephens: Then I will not move any amendment, but will support the motion as it stands.

The motion was carried.

The reading of Dr. Fritz Noetling's paper on "The Manufacture of the Tero-Watta" was postponed until a future meeting.

MAY 22nd, 1911.

A special meeting of the Royal Society was held at the Museum on Monday evening, May 22, 1911, for the purpose of considering the new rules drawn up by the committee appointed for the purpose at the last annual general meeting. Hon. Dr. Butler occupied the chair.

The rules were taken seriatim.

On rule 4, "The society shall consist of ordinary members, honorary members, and corresponding members."

Mr. T. Stephens said that it was proposed to do away with the title of "associate," and he had not heard any good reason advanced in the committee for doing so. Associates had only been admitted for three years, and there had only been one during that time, but it did not follow that there would not be many more in the future. He would object to the omission of the word "associate."

Mr. E. L. Piesse said that it was proposed to reduce the annual subscription to one guinea. The subscription now was £1 10s., and associates were admitted at one guinea. If the subscription was reduced to one guinea there would be no need to have associates.

Mr. T. Stephens said that it was also proposed to do away with the title "Fellow," and substitute "member." He was quite in accord with that, as there was no qualification for Fellows, but he did not think it could be done without the repeal or amendment of the Royal Society Act.

The consideration of the clause was postponed until the amount of the annual subscription was decided.

Rule 8, fixing the annual subscription at one guinea, was agreed to without discussion.

On rule 22, that the Council should consist of nine ordinary members, to be elected at the annual general meeting,

Mr. T. Stephens said that there was considerable difference of opinion in the committee in regard to this rule. He did not care whether the Council consisted of 12 members, as at present, or nine, as proposed, but he objected to the whole Council having to be elected at each annual meeting, as a number of men might be elected who knew nothing about the business of the society.

Dr. Crouch: What was this committee?

The Chairman: It was a committee appointed by the Fellows at the last annual meeting for the purpose of revising the rules.

The provision that the Council should consist of nine members was agreed to.

On the proposal that the Council should be elected annually, the meeting divided, with the following result:—Ayes, 15; Noes, 10.

The Chairman stated that several proxies had been received.

Mr. L. Rodway raised the question as to the legality of the proxies, as he did not think they had been laid before the Council as the rules required.

The Chairman said that two of the proxies had been irregularly made out, and he did not think that the others could be received, as they had not been considered by the Council.

After some discussion, it was decided that the proxies which had been handed in prior to the meeting should be received, as there had not been a meeting of the Council between the calling of the meeting and that evening.

When the proxies were added to the other votes, the rule was declared agreed to by a majority of one.

On rule 50, which proposed to do away with voting by proxies at any meeting of the society,

Mr. Rodway moved as an amendment that country members or others who were unable to attend should be allowed to vote by post. If only those who attended the meetings were allowed to vote it would tend to destroy the national character of the society.

After discussion, the proposed amendment was agreed to.

Mr. T. Stephens moved as a further amendment that Fellows resident not less than 10 miles from Hobart should be allowed to vote by proxy at special general meetings.

The amendment was negatived, and the rule as amended agreed to.

Postponed rule 4 was agreed to.

It was decided that the new rules should come into force on January 1, 1912, and that in the meantime they should be referred back to the drafting committee for arrangement in suitable sequence.

The meeting then terminated.

JUNE 12th, 1911.

The general monthly meeting of the Society was held at the Museum on Monday evening, June 12th, 1911.

His Excellency Sir Harry Barron, K.C.M.G., occupied the chair.

Messrs. G. V. Brooks, R. S. Dickenson, M.A., E. I. Gower, B.A., Robert Hall, C.M.Z.S., E. H. D. Keene, M.A., R. O. Miller, B. Sc., were elected Fellows of the Society.

The formation of a Biology Section was announced, which would hold its first meeting, to appoint office-bearers, during the month.

Mr. R. Hall announced that, in connection with the Mawson Antarctic Expedition, a committee of twelve had been formed to represent the Royal Society, the Australasian Science Association, and the Field Naturalists' Club. Four members to represent the citizens would be added later.

Mr. T. Stephens, M.A., exhibited a specimen of Alyxia buxifolia, sometimes called native sandalwood. Mr. A. M. Lea, F.E.S., showed a case of beetles belonging to the genus Bactocera.

Mr. J. W. Beattie read a paper on "Early History of the Islands of Bass Strait." The paper was illustrated by lantern slides prepared from photographs made on a recent tour with His Excellency the Governor, and Ministers.

The paper dealt generally with the physiography of the groups of islands, as well as with the early settlers and voyagers.

His Excellency, at the conclusion of the lecture, said that the half-castes should never have been put down in the islands, like a flock of sheep, without being taught how to make a living, or being given some object in life. There were about 250 of them, and they were a respectable, quiet people, and he believed they would be willing to work, if they could only be shown what to work for. They had now a school, with a schoolmaster and his wife, who had taken up the work in a strong missionary spirit, determined to help them and show them what they had to live for. He hoped to see a great change in the half-castes before long. Flinders Island was now being taken up, and there was a prospect of the islands going ahead. During his visit to the Straits he landed at 13 or 14 islands, and he was perfectly certain that no one who had not visited them could realise the conditions under which the people lived, and the simplicity of their surroundings. Flinders Island had a population of 300, and there was not a doctor, a nurse, or a chemist's shop upon it.

Mr. Rodway's paper on "A New Lichen," and Dr. Noetling's paper on "The Tero-Watta," were taken as read.

JULY 10th, 1911.

The general monthly meeting of the Society was held at the Museum on Monday evening, July 10, 1911.

Hon. G. H. Butler, a vice-president, occupied the chair.

Mr. E. L. Piesse moved, "That the Attorney-General be asked to introduce a bill to incorporate the society, and confer upon it powers as to holding property, litigation, and to make and alter rules." He explained that the present Act was an old one, and difficult to understand. There were two small pieces of land in which the society was interested, and there might be others. He had prepared a draft bill containing four operative clauses.

Mr. A. O. Green seconded the motion.

Dr. Noetling moved, and Mr. Ritz seconded, an amendment, that the original Act of 1853 be retained. This was negatived, and Mr. Piesse's motion carried.

The following papers were read:—

(1.) "Notes on Treubia Insignis," by L. Rodway. The author reported that it had rot been found hitherto in Australia. Its habitat is upon the slopes of Mount Wellington.

(2.) "Notes on the Hunting Sticks, Spears, and Baskets of the Tasmanian Aboriginals," by Fritz Noetling, M.A., Ph.D. The author exhibited specimens, and dwelt on the throwing sticks of the aboriginals, which, he said, were not curved like the boomerang. The sticks were chiefly used in hunting expeditions. Their spears were of great length, and it was extraordinary that accounts stated that the natives could throw these spears as much as 60 yards. The speaker exhibited two baskets, made of some plant fibres by aboriginals, and mentioned the interesting fact that they were identical with the rude sedge baskets made by the lake dwellers of Europe in the middle glacial period.

Dr. Clarke mentioned that he once knew an old man who was wounded by one of those spears. He was 102 years of age when he (Dr. Clarke) knew him, and he stated that in 1827 he was out in the Tower Hill district with a survey party, when a party of blackfellows sneaked up, and one of them flung a spear at him, which wounded him in the fleshy part of the arm. The old man died at the age of 102 years.

Messrs. A. J. Taylor and L Rodway took part in the discussion.

The reading of a paper by Mr. H. Stuart Dove "On the Connection of Swifts with the Weather" was postponed, and the meeting terminated.

JULY 31, 1911.

A special general meeting of the society was held at the Museum at 8 p.m.

His Excellency Sir Harry Barron, K.C.M.G., presided.

The meeting was convened with the purpose of hearing a lecture by Comte de Fleurieu, a member of the French Geographical Society, on the early geography of Tasmania.

The visiting lecturer said that in the days of the old French navigators, D'Entrecasteaux, Baudin, and Peron, his great uncle, Chevalier de Fleurieu, had been high in the Marine Department of France, and his name had been given to several of the discoveries made in Australian waters. When he (the lecturer) first came out to Australia he was disappointed to find that none of the names so given had been retained.

Fleurieu Bay was now called Oyster Bay, Fleurieu Island was Cape Barren Island, and when he went to Port Cygnet to see the Fleurieu River of the old charts he found that it was called the Agnes Rivulet. He then gave a description of the expeditions sent out by France, and of the work done by Flinders up to the time of his arrival at the Isle of France, where he was detained a prisoner for six years. He defended the French from the charges which had been made against them of having copied Flinders's charts, and pointed out that the French charts, in some cases, dealt with portions of the coast, especially on the north and west of Australia, that Flinders had never visited. In regard to nomenclature, he pointed out the necessity of adhering, as far as possible, to the original names, especially those that bore a historic signification. A number of the early charts of Tasmania were then shown by means of an optical lantern. Commencing with the charts of Tasman, the lecturer explained them, and, following on, dealt with those of Marion, Furneaux, D'Entrecasteaux, Hayes, and Flinders. He also compared the French charts with those of Flinders, showing the differences that existed between them. In concluding, the lecturer spoke of the necessity of preserving the names given by the early explorers. If there was a section of the Royal Society devoted to history and geography, he hoped they would take the matter up. If they did so, he believed that the French Government would be glad to send them copies of maps and documents dealing with the matter. If they put back in Tasmanian nomenclature certain names given by the early French and English explorers, they would be adding to the ties which drew the French towards Tasmania, and foster the feelings of sympathy that there were between them.

His Excellency said that the lecture was hardly one that opened up discussion. He was in sympathy with the Comte de Fleurieu, however, in his suggestion that the names given by the grand old explorers should not be allowed to die out, and he thought that they also sympathised with him upon his own historical name having been omitted from our maps. If it was possible to restore the old names, he hoped it would be done. He asked that a vote of thanks to the lecturer might be carried by acclamation, which was done.

The proceedings then terminated.

SEPTEMBER 11, 1911.

The general monthly meeting of the society was held at the Museum in the evening, at 8 o'clock.

Hon. G. H. Butler, a vice-president, occupied the chair.

The Secretary to the Council announced the receipt of eleven handsome volumes detailing the work of the Harriman Alaskan expedition.

The Chairman explained that two offers had been made to the society for a portfolio of drawings in their possession. The meeting decided it should be sold, and fixed the price at £100, the proceeds of sale to be invested as the Council may deem fit, and the interest used as the Council may decide.

The following paper was read:—

"The Connection of Swifts with Weather," by Stuart Dove, F.Z.S. The writer detailed a number of observations which tended to show that the swifts appeared immediately before atmospheric disturbances. Ants in the winged state formed a large part of the food of the swift in this country, and the writer had noticed that the winged ants issued from their nests more particularly during the damp and close weather which precedes weather changes. It had occurred to him that the frequent appearance of the swifts shortly before or after atmospheric disturbances might be due to their winged food occurring more plentifully at those times.

Mr. T. Stephens exhibited a piece of bone which had been found by his son when engaged in laying out the route of the Stanley-Balfour railway near Circular Head. The bone had been found in the same district as the large bones of the extinct animal which had been discovered in Mowbray Swamp. He had sent the bone, which was very hard and polished, to Dr. Hall, of the biological department of the Melbourne University, but the species of animal to which it belonged could not be determined.

Mr. Stephens further drew attention to the discovery on the Australian Alps of a grass, Poa saxicola, which had previously only been found on Mount Wellington. The discovery was reported in the proceedings of the Linnean Society of New South Wales.

Mr. L. Rodway said that he had been over nearly all the mountains in Tasmania, but he had only found Poa saxicola in one small area on Mount Wellington. The discovery was, therefore, a very interesting one, provided that the grass had been correctly identified.

Professor Flynn exhibited dissections of the reproductive organs of certain marsupials, and gave a short address on the manner in which the embryos were borne.

A fish's egg was also exhibited which Professor Flynn identified as that of Callorynchus antarcticus, sometimes called the elephant fish.

Mr. Robert Hall exhibited a number of swallows and swifts to illustrate Mr. Dove's paper. Mr. Hall also exhibited a fish (Optonurus denticulatus, Rich.) closely resembling the "whiptail," which had been dredged in 800 fathoms of water off the coast of New South Wales. It was now safe to record this deep water species as new to the list of the Tasmanian fauna.

OCTOBER 9th, 1911.

The general monthly meeting of the society was held at the Museum on Monday evening, October 9th, 1911.

Dr. Fritz Noetling occupied the chair.

Messrs. G. H. Gibson, M.B., C.M., T. Dunbabin, M.A., R. B. Montgomery, A. R. Tucker, and J. Moore Robinson were elected Fellows.

The Chairman informed the meeting that Bishop Montgomery had presented this portfolio to the Society, and with that knowledge it would be unwise to dispose of it. This was approved by the Fellows.

The following papers were read:—

"Further Notes on the Habits of the Tasmanian Aborigines," by Fritz Noetling, M.A., Ph.D.

"Notes on the Septum of Trichosurus canina," by Prof. T. T. Flynn, B.Sc.

Mr. L. Rodway exhibited a specimen of a fungus new to Tasmania. It was known as Geoglossum hirsutum, and though it had been described from Australia, it had never previously been found in Tasmania. He also showed a specimen of a plant named Thismia rodwayi, which he had first found in 1890 on Mount Wellington. It lived in the ground, running along in the humus, and subsisted on the decaying vegetable matter in the soil. There were nine members of the genus, but they were all inhabitants of the tropics. This plant had never been found in Australia or Northern Tasmania, and it was very curious that a plant which belonged to an essentially tropical genus should be found in Southern Tasmania, and nowhere else in Australia. He expected, however, that careful research would show that the plant existed in Australia. It belongs to the family Burmanniaceæ, which adjoins the Orchideæ.

Mr. G. Brettingham-Moore exhibited a hollowed stone, which he thought showed traces of human handiwork. He had found it on Maria Island.

Dr. Noetling said that he thought the stone showed traces of human workmanship, and he did not think it could have been due to Europeans. He could not, however, suggest any use, from what was known of the natives, to which it could have been put.

Mr. L. Rodway said that he did not know of any edible seeds which the natives were likely to use for food, and he did not know of any article of their food which would be likely to be pounded in the stone, unless it was fern roots.

Professor Flynn considered that the stone was purely a natural phenomenon, and was not due to human agency.

Dr. Noetling exhibited a number of stones which showed traces of glacial action. It had been found in South Australia that there were indications of a glacial period at an age when life in any form had not yet appeared on the globe. The boulder clays in which these stones were found showed signs of having undergone great changes due to lateral pressure. Many stones or pebbles were embedded in glacial ice, and when that ice travelled over hard surfaces these pebbles became marked with striæ, which were unmistakable. There was no other agency known which could produce marks of this nature. The theory had been put forward that the poles were continually altering their positions, and that at one time the South Pole was in the neighbourhood of where Sydney now stood. The fact that traces of a very ancient ice-age could be found in Australia was not known when the theory was formed, but the present discovery was a very curious confirmation of it. Whether the theory was true or not, geologists had greatly altered their views of late as to climatic conditions in ancient times. The traces of the ancient ice-age in South Australia extended from South Adelaide to Hergott Springs, and had a thickness of 1,500 feet.

Dr. Noetling also exhibited a very curious specimen, in which the cast of a fossil brachiopod shell had been replaced by gypsum.

NOVEMBER 14th

The ordinary monthly meeting of the Royal Society of Tasmania was held at the Museum last night. The chair was occupied by the President (His Excellency Sir Harry Barron).

Messrs. W. T. McCoy, B.A., and A. C. Stephens, B.A., were elected Fellows of the Society.

Application was made by the Field Naturalists' Club for the use of a room at the Museum. Several members were not in favour of rescinding the rule relating to the letting of rooms, which, it was said, would be necessary before the permission could be granted. Some discussion took place on this question. Ultimately it was postponed, on the ground that no proper notice had been given of an intention to discuss the matter.

Dr. Fritz Noetling read a paper on "Gigantic Marsupials in Tasmania." He contended that until quite recently Tasmania was connected with the mainland, and that there was a relation between fossil marsupials found in Tasmania and those lately unearthed in Queensland. Existing species of mollusca specimens, obtainable at the present time, bearing relation to the gigantic marsupial which is now extinct, showed that the latter were creatures of recent ages.

Mr. Thomas Stephens agreed generally with Dr. Noetling's views; his theory was ingenious, and probably correct. He hoped that the construction of the Stanley-Balfour railway, which was now going on, would, through the agency of the deep cuttings to be made, give a lot of information concerning the geology of the North-West Coast.

Professor T. T. Flynn was to lecture on the "Anatomy of Petaurus sciureus." He had been able to secure an animal from Mrs. Roberts, of Beaumaris, and made an examination. The result was purely of a scientific nature, and as it referred only to the anatomy of the animal, would not be very interesting.

A paper entitled "Notes on Duterrau's Reconciliation Picture" was read by Dr. Noetling, who said that nobody knew what has become of the picture. As it was a reconciliation between the blacks and whites, it was an extremely interesting one, and of some scientific value.

I am indebted to Mrs. Lorenzo Lodge, who was on very intimate terms with the Duterreaus, for the following information:—"Benjamin Duterreau was descended from a French family, who had taken refuge in England on account of religious trouble. He was born in London in 1767, where his art education was acquired. He learned the art of steel engraving, and practised it as a business there. Attracted by accounts of the Swan River colony (W.A.), he left London with the intention of settling in the colony, and arrived there in 1832. At that time glowing accounts of Van Diemen's Land were in circulation, and hearing these, Mr. Duterreau changed his purpose of settling at Swan River, and came on to Hobart Town. accompanied by his daughter and sister-in-law. They resided in the old white house at the corner of Campbell and Patrick streets, and there Mr. Duterreau practised portrait painting principally, and had a well-known reputation in Hobart Town as a portrait painter. Colonel Arthur took a great deal of interest in Mr. Duterreau. Miss Duterreau became governess to the family, and, no doubt, he induced Mr. D. to undertake the portrayal of the aboriginals, and encouraged and aided him in the work. He frequently visited the studio during the progress of the work. As the aboriginals were brought in by Robinson—camping in the yard of his house at the corner of Elizabeth and Warwick streets—he used to supply Duterreau with subjects, bringing them down to him personally to the Campbell-street house, and the results of those visits are to be seen in the numerous paintings, copperplate engravings and plaster casts now in the Museum here. A few years before his death he removed to the stone house in Bathurst street, at present occupied by Mr. Lucas, next King's Hall, and he died there in 1851, at the age of 84 years. His daughter married Mr. Bogle, of the firm of Kerr and Bogle, merchants, of Hobart, and ultimately returned and settled in England. Most of Duterreau's best work was sent, by request of Mrs. Bogle, to England after her father's death."

Mr. J. W. Beattie said that a Mrs. Lodge informed him that she recollected such a picture. There were two, a small one and a very large one. In reply to a question, she informed him that the large one was given to the Government, and was now probably in the "vaults of the Legislative Council." Upon inquiry, he ascertained that the small one was framed by Hood's, of Hobart, for Mr. J. Walker, and sold by that gentleman in 1841 for £50. The large one, painted by Bock, was framed by the same firm in 1843, and was said to be also in the Legislative Council vaults.

Specimens of various descriptions were exhibited and explained by members.

His Excellency referred to the lecture to be given by Dr Mawson at the Town-hall, and asked members present to attend. It would deal with Antarctica and the expedition.

1.—NOTES ON THE MARKS OF PERCUSSION ON SILICEOUS ROCKS.

Pl. I. and II.

By Fritz Noetling, M.A., Ph.D., Etc.

(Read April 10, 1911.)

1. INTRODUCTION.

When a solid body suffers a blow, it is obvious that that portion of the energy of the blow that is not converted into heat, must give rise to vibrations radiating from the point of impact in all directions. It is further apparent that these vibrations may not only result in detaching a flake, but if the energy was large enough, there may be a surplus, resulting in vibrations which must give rise to accessory marks, not only on the parent block, but also on the detached flake. On the other hand, the energy may not be sufficient to achieve these results, yet it must leave some traces behind at that point of the surface where the blow struck, that is to say, the point of impact. We will thus have a wide range, beginning with the inefficient blow, that is to say, a blow which was not sufficient to detach a flake, and ending with a blow of such energy not only to detach a flake, but to give rise to numerous accessory marks of percussion. Between these two extremes there must be various stages, according to the force of energy applied and the result achieved.

It goes without further saying that the permanent effects of a blow must largely depend on the tenacity of the substance. The more ductile it is, the larger will be the energy required to detach a flake, and the more of it will be spent in the production of useless vibrations. The more brittle it is, the less energy will be required, and the larger is the amount of energy available for the production of accessory effects. The resistance to the transmission of oscillations created by the blow is another factor which is of great importance. The whole problem is, in fact, a physical one which ought in the first instance be treated on a mathematical basis, but I am afraid that it would be of little use to the Archæologist if dealt with in an abstract sense. From his point of view it is better to study the effects, the cause being known.

It is certain that these effects must be a function of the composition of the matter, supposing the energy being the same. The effects of a blow striking with the energy of 100 foot pounds, must be quite different if the substance be lead or antimony, a hornblende rock, jadite, nephrite, or flint. It would be outside the scope of this paper to investigate into the effects of blows on Hornblende rocks or Nephrite and other allied substances largely used in the manufacture of stone implements. I propose to deal here only with the effects of blows on that substance of which most of the stone implements are produced, viz., siliceous rocks.

In its purest form the siliceous minerals are represented by crystallised quartz, having 46.67 per cent. of silicon. Through the admixture of other substances, a large variety of minerals are produced, but in all of them the percentage of silicon is considerably smaller than in crystallised quartz. The mineral most commonly used in the manufacture of stone implements is flint in Europe, the various siliceous rocks resulting from the metamorphism of permian rocks, which are called chert or hornstone and porcellanite in Tasmania. (1)

However different in composition these minerals and rocks may be, they have one feature in common, viz., an exceedingly fine conchoidal fracture.

But even the casual observer cannot fail to notice that the nature of the fracture greatly varies in the different kinds of siliceous rocks. I have no data for determining what causes produce the most suitable fracture for the manufacture of implements. Pure rock-crystal and its nearest relation, chalcedony, have not, particularly not the latter, the same fine fracture, as, for instance, the impurer flint, and it seems certain that the quality of fracture does not depend on the pureness of the silica. In fact, the instance here quoted proves that the less purer mineral has a better fracture than the purer one. Natural and artificial glasses have an exceedingly good fracture, and it almost seems as if the quality of fracture were dependent on the presence of iron. I advance this theory with all reserve, as long and tedious chemical and physical examinations would be necessary to establish it.

In manuals and text-books of Mineralogy the fracture of siliceous rocks is described as "conchoidal," the surface produced by fracture having elevations and depressions in form like one-half of a bivalve shell.

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1) Noetling, Preliminary Note on the Rocks used in the manufacture of the Tronatta.—Pap. and Proceed. Roy. Soc., Tas., 1909, page 85.

2. HISTORICAL SUMMARY.

As far as my knowledge goes, the first who studied the mechanics of the fracture of flint was a Frenchman, M. Jules Thore, who published in 1878 a paper in the Bull. de la Soc. de Bord., Dax, on this subject. Unfortunately this journal is not available in Hobart, and it may be that much of what I am saying here has already been expounded in this paper.

Many authors, particularly Mortillet (1), Skertchly (2), and Sir John Evans (3), have given essays on the manufacture of gun flints, and the connection between this craft and the manufacture of stone implements. However valuable these observations may be, there is a great difference between the equipment of a modern gun flint manufacturer, and that of neolithic or palæolithic man, and it is more than probable that implements of the gun flint maker considerably differed from that of a Tasmanian, for instance. With regard to the latter, we know that his only implement for detaching a flake from a parent block was another stone, and even the more delicate work of marginal chipping was done by means of a stone, as the invention of pressing off small flakes by means of a piece of bone or stag's horn had not been made by the Tasmanians. The most important fact we can glean from all these accounts is the statement (4) "that success depends a great deal upon the condition of the flint as regards the moisture which it contains, those which have been too long exposed upon the surface becoming intractable, and there being also a difficulty in working those that are too moist." I can fully confirm this from my own observations and experiments. A pebble of chert taken from the gravel deposits, which still retains its moisture, flakes much better than the same kind of rock when found on the surface of an old camp. The next important observation is the statement that the surface must be struck at an angle of about 45deg., and that a spherically-ended hammer was used. All these accounts deal, however, more with the art of manufacture than with the mechanical effects of percussion, and even Sir John Evans has devoted only a few lines to this important subject. (5)

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(1) Musée Préhistorique, pl. II.

(2) Memoir, Geological Survey of England, 1879.

(3) Ancient Stone Implements of Great Britain.

(4) Ancient Stone Implements of Great Britain, page 18.

(5) Ancient Stone Implements of Great Britain, pages 273-274.

Apart from a diagram on the back of the cover of Prof. Schweinfurth's publication (1) I know only of one publication that more explicitly deals with this matter. Prof. M. Verworn, in his most interesting and important memoir on the archæolithic implements of the Cantalian (2) gives a complete description of the various marks produced by percussion. Previously G. and A. de Mortillet had drawn attention to certain features observable on flint flakes, which he took to be the most characteristic signs of an artificial nature of such flake. Prof. Verworn adds a considerable number of other marks which had hitherto been entirely overlooked, in particular he draws attention to the Strahlen spruenge (star-cracks).

Though very exhaustive, I do not think Prof. Verworn's study is quite complete, and my studies on the Tasmanian tero-watta (3) have afforded important additions on this subject. Though a good deal of what I have to say here will not be new to the student in Europe, it will be of the greatest interest to the Australian student. In particular I shall be able to prove that the "thumb mark" of the amateur collector is not an intentional, but quite an accidental feature.

3. THE MARKS OF PERCUSSION.

We will begin our studies ab ove; that is to say, we will assume that an Aborigine found a boulder or pebble of such kind of rock as was suitable for the manufacture of a tero-watta. As I pointed out the nature of these rocks in a previous paper (4) I need not go here into further details. We will further assume that he obtained the boulder from a gravel deposit, and took it to his camping ground in order to break it at his leisure.

In most cases, particularly when the boulder was obtained from the diluvial gravel beds, its surface was covered with a crust due to weathering. This crust must

(1) Deutsch-Französisches Wörterverzeichniss der die Steinzeit betreffenden Literatur, 1906.

(2) Die archæolithische Cultur in den Hipparionschichten von Aurillac (Cantal) Abhand. d. K. G. d. Wiss. Goettingen. Math. Nat. Classe, N.F., Vol. IV., No. 4, 1905, page 21.

(3 According to W. Schmidt (Zeitsch. f. Ethnologie, 1910, Heft. VI., page 915) either tero-na or tero-watta (tero-na-watta?) is the correct designation of the Tasmanian archæolithic implement.

(4) See this journal, 1909, page 85.

be distinguished from the patina, which formed later on the surfaces produced by flaking, though the original crust and the later patina are in fact only different stages of one and the same process, viz., chemical decomposition of the original matrix.

This boulder could be broken in two ways, either it was dashed against a hard object, or it was struck with another stone. It is obvious that the first method was very uncertain, and though it may have occasionally been resorted to (1), it is pretty certain that usually the boulder was broken by means of another stone, which served as a hammer, and which frequently must have been wielded with great force. Such hammer stones are by no means uncommon, particularly in quarries and it is a remarkable fact that, with very few exceptions, chiefly Diabas pebbles were used as hammer stones.

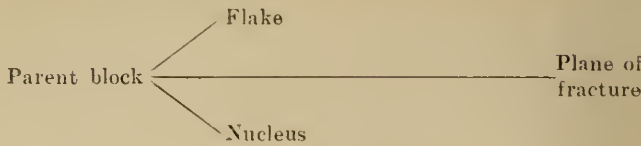
Whether other stones were used as anvils is not quite certain. A priori it is very probable that such stones were used, because it is easier to break a stone resting on a hard than on a soft, non-resisting surface. The nucleus from Kempton does not indicate that it rested on some hard material when it was broken, and so far I have not found any stones which I could definitely identify as anvil stones. It is, however, pretty certain that during the finishing process the tero-watta was held in the hand, and did not rest on an anvil. This is, however, a different question altogether, though it is of some importance because the marks of a blow on the stone held by the hand are probably quite different from those on a stone resting on a hard support.

We will now assume that the Aborigine, having provided himself with a hammer stone, struck the boulder with a strong, smart blow. It will be useful to explain a few terms before proceeding further. We may call the original boulder the "parent block," and every fragment that was struck off, however big or small it may have been, a "flake." What remained of the parent block after one or more flakes had been struck off is called "nucleus" or "core."

The parent block is, therefore, divided by a blow into flake and nucleus, the flake being the desired object, the nucleus the useless residue. It need hardly to be mentioned that more than one flake can be, and has been,

(1) Ling Roth, Aborigines of Tasmania, 2nd ed., page 151.

struck off a parent block before the desired one was obtained. We have therefore



We will now study the effects of this blow on the parent block; there are only two alternatives: either a flake was detached, or it was not. If the blow was effective, a flake of smaller or larger size was detached; but if it was not effective, the result must be a shattering of tne surface into countless splinters at that point where the hammer struck the parent block, viz., the "point of impact." It is further obvious that in order to detach a flake, the hammer must not penetrate into the matrix of the parent block. If it does, a good deal of the energy will be spent in shattering and pulverising the matrix, and the remainder of the force is probably not sufficient to detach a suitable flake. The hammer must also strike the surface in one point only, and for this reason a spherical hammer or a pebble is the most suitable implement. A flat or pointed hammer will either shatter the surface or penetrate into the matrix.

If the blow did not detach a flake, that is to say, if it was ineffective, such result may have been due to insufficient energy, or the penetration of the hammer into the matrix, or both causes. The result will, however, always be the same, viz., a shattering of the surface, and its intensity is determined by the energy of the blow and the resistance of the parent block.

A.—MARKS OF INEFFECTIVE BLOWS.

Traces of ineffective blows are frequently observed; they are particularly common on rejects in the quarries, and Plate I. gives a very good idea of the effects of an ineffective blow.

The principal result of an inffective blow is the production of a fairly deep impression or indentation whose surface is broken by numerous fine fissures running more or less parallel; the fine lamellæ of rock thus produced are intensively splintered by cross fissures, thus producing

REJECT FROM NICHOLS'S QUARRY WEST, MELTON MOWBRAY, SHOWING THE TRACES OF TWO INEFFECTIVE BLOWS.

a zone of intense destruction amounting almost to pulverisation of the matrix. It is impossible to mistake the marks of an ineffective blow; they are too characteristic.

Professor Verworn was the first who drew attention to this feature, which he calls "Splitterbrueche" (splinter-fractures), and he is of the opinion that they were the result of several blows administered to one and the same spot if the first blow was not sufficient to detach a flake.

This may be so with regard to the flint implements of Europe, but it certainly does not apply to the tero-watta of Tasmania. So far I have not found a single specimen which would corroborate Prof. Verworn's view. If the blow did not detach a flake, but produced splinter fractures only, the second blow was never administered to the same spot, but directed a little away from it. This may have had the desired effect or not, and another specimen from the same locality proves that at least three ineffective blows were placed side by side without detaching a flake.

B.—MARKS OF EFFECTIVE BLOWS.

(a) The Production of Flakes.

We will now examine the results of an effective blow, viz., one that detached a flake from the parent block. It is obvious that in order to be effective the blow must be administered with sufficient energy to overcome the resistance of the parent block, and the hammer must not penetrate into the matrix, and it must strike its surface at one point only.

It is further obvious that when a flake was detached from the parent block, that point of the surface which was struck by the hammer was on top, or nearest to the hand holding the hammer stone. The plane of fracture along which the flake was detached from the parent block must be nearer to its centre than its surface. The position of the parent block with reference to the workman, and the position of the flake with reference to the parent block enables us thus to distinguish five sides which must occur in every flake, viz.,

1. External face.
2. Internal face.
3. Proximal end or edge.
4. Distal end or edge.
5. Lateral edges

} as shown in Plate II. (1)

(1) It must be understood that Fig. 1 to Fig. 9, Plate II., are diagrams only.

1. EXTERNAL FACE (Indical Face). (E.F.)

It is obvious that the original crust or surface of the parent block must represent the external face of the first flake that was struck off. This flake may remain as it is, and it may be taken in use just as it fell off, or else it may be considerably altered by chipping, to such an extent that sometimes hardly any trace of the original surface is left.

The external face, or, as I prefer to call it the Indical face, is always more or less convex, only in rare instances it is flat. (See later, Internal Flakes). The term "external flake" may appropriately be used for all flakes, whose indical or external face is formed by the original surface of the parent block.

2. INTERNAL FACE (Pollical Face). (I.F.)

It is obvious that the internal face, or, as I prefer to call it, Pollical face, is opposite the external one, and must represent the plane of fracture along which the flake was detached from the parent block. It is, therefore, unquestionable that if there are any marks of any kind on it, the negatives of such marks must appear on that part of the nucleus where the flake became detached.

The internal or Pollical face is usually flat, sometimes slightly convex towards the proximal end, but it never attains the convexity of the external face. The accessory marks of percussion must always appear on the internal face (1).

3. THE PROXIMAL END OR EDGE (p.e.)

That portion of the flake which was struck by the hammer stone may be called the proximal end or edge. It is obvious that the proximal end must bear the strongest effects of percussion, having sustained the first impact.

4. THE DISTAL END OR EDGE (d.e.)

That portion of the flake opposite to the point of impact or proximal edge may be called distal end or edge.

(1) It is hardly necessary for me to explain that it is the case of an external flake just as it fell off, and not of one whose external or indical face was subsequently wrought.

Being furthest away from the point of impact, the marks of percussion must decrease in intensity from the proximal towards the distal edge.

5. THE LATERAL EDGES.

Strictly speaking we should also distinguish between the lateral edges but it is clear that the terms "right" and "left" will be misleading, because the right edge of the Indical face is the left of the Pollical face, and vice versa, with regard to its left edge.

In my descriptions, and unless space or other reasons do not permit it in the illustrations, I always place a flake in such a way that its proximal end represents the top, the Pollical face being looked upon. In this position I apply, for want of any better ones, two nautical terms. I call the left side or edge, port side or port edge, and the right side, starboard side or edge (1). Thus, if we speak of the port side or edge of the Indical face, we know it is exactly opposite of the port side or edge of the Pollical face, while if we were to speak of the left edge of the Indical face, we were always obliged to add "which represents the right edge of the Pollical face."

The two faces are of necessity always well defined, but as the intersection of the plane of fracture with the surface of the parent block, the line of fracture must form a closed curve, the lateral edges are frequently not so distinctly set off against the distal edge. This is particularly shown in the semi-crescent flakes whose distal and lateral edges merge into one semi-lunar curve.

The above features are characteristic of the external flake, but they must be somewhat modified with regard to the external Indical face, should there subsequently more flakes be struck off the same parent block.

The Tasmanian Aborigines had two ways of further treatment of the parent block after the first external flake had been struck off. We will assume that Plate II., Fig. 2 be the first external flake that was struck off. As already stated, the negatives of all marks on its Pollical (internal) face must appear on the nucleus.

We will now assume that the next flake (Plate II., Fig. 4) was detached from the parent block by a plane of fracture that was approximately parallel to that which separated the

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(1) If anybody can suggest better terms than these two which avoid the misleading words "left" and "right," I am only too pleased. For the present I cannot find anything better.

first one. In other words, that the parent block was turned and the hammer struck again the surface. The flake thus detached cannot strictly be called an external flake, though some portion of the original crust is still present in it. We notice, however, that it is in a different position; instead of being present on the external face, it is now on the proximal end of the flake. The external (Indical) face of this flake is really the counterpart (negative) of the first flake that was detached from the same block.

Flakes of this type may be called internal flakes, and the nucleus of Kempton, with its 43 flakes, affords an excellent illustration of this type. The external (Indical) face of the last flake that was struck off is formed by the negatives of the internal faces of the two previous flakes, their planes of fracture intersecting at an angle, and thus producing a ridge extending more or less medially from the proximal to the distal end. (Fig. 4a.)

All flakes having one or more ridges extending from the proximal to the distal edge, which are usually called "knives," are internal flakes, because it is indubitable that long and flat planes cannot be produced by subsequent trimming or marginal chipping, but they must represent the planes of fracture of previous flakes; in other words, the negatives of the Pollical (internal) face of such flakes (1).

To the European mind the above seems to be the most sensible method of striking off flakes. The mind of archæolithic man, including the Aborigines, hit, however, on still another one.

Though I have not found, so far, a core and flakes illustrating this other method, the proofs are ample enough in the shape of implements. Theoretically the external flake should have sharp edges all round, because the plane of fracture intersects the surface of the parent block in a line. Specimens of this type are not very common, most of them I found at Devonport. If, however, a larger number of tero-watta is examined it will be noticed that though they are unquestionably external flakes, the proximal end, instead of forming an edge, is truncated by a plane, which I call "Percussion Face, P.F.," for reasons explained further on, which always forms an obtuse angle with the Pollical (internal) face. If the implement is, as is usually done,

(1) The famous Aurignac knives appear to me to be internal flakes, and their peculiar form is, in my opinion, not the result of a deliberate intention, but more probably due to the manner by which the flakes were detached from the parent block. (See also the figure in Sir John Evans' book illustrating the manufacture of gun flints.)

placed in such a way that the pollical (internal) face forms a right angle with the horizontal, this plane is always inclined towards the Pollical face.

I have made a few measurements to determine the angle formed by these two planes, and find the following angles:—

Tero-watta from	Pontville (Shene)	123deg.
,,	Rose Dale	124deg.
,,	Merton Vale	127deg.
,,	Winton	129deg.
,,	Maryvale	130deg.
,,	Hutton-park	133deg.
,,	Old Beach	135deg.

The size of this angle is significant, and its importance will be explained later on.

The Percussion face exhibits a very characteristic feature; its "radial" diameter, that is to say, the distance from the external to the internal edge is always smaller than the distance from side to side, the "peripheral" diameter. As both edges are convex, the internal one usually less than the external, the outline of the plane of percussion is that of a spherical bi-angle, the two points being at the port and starboard side respectively.

Of course, this lenticular shape is not always well preserved, and more often than not, only traces remain, particularly when there is marginal chipping along its external edge.

There cannot be the slightest doubt that the Percussion face is the remainder of an old plane of fracture which was formed when a former flake, whose internal (Pollical) face now forms an angle of about 135deg., with the internal (Pollical) face of the second flake was struck off the parent block.

In order to understand this fully we must revert to the first external flake, and Plate II., Fig. 2a., will further illustrate this. Let us assume that the first external flake (No. 1) was struck off the parent block, the remaining nucleus then exhibited on one side a more or less level or flat plane representing the plane of fracture. This plane must of necessity be the negative of the external (Pollical) face of the external flake (No. 1) struck off, and if we were to proceed according to the first method, it would form the external (Indical) face of the next flake, i.e., the internal flake, 1st order (No. 2.) The Tasmanian Aboriginal, as well as archæolithic man in Europe, treated such a nucleus differently: he turned the parent block round till the plane of fracture was fairly level, and then struck it with a smart, sharp blow at an angle of about 45deg. If the blow was effective, the flake became detached, and it is easy to see that if the blow fell under an angle of 45deg., the new plane of fracture, that is to say, the Internal or Pollical face of the new flake must form an angle of 135deg. with the old one. (See Fig. 2a.) Hence the significance of the angle formed by the Percussion face and the Internal (Pollical) face, because we can gauge from it the angle under which the hammer stone struck the parent block. In the specimens mentioned above it would be.

Tero-watta from	Pontville (Shene)	57deg.
,,	Rosedale...	56deg.
,,	Merton Vale	53deg.
,,	Winton	51deg.
,,	Maryvale...	50deg.
,,	Hutton-park	47deg.
,,	Old Beach	45deg.

These figures prove conclusively the statement which, if I am not mistaken, was first promulgated by Sir John Evans, that in order to be effective the critical angle under which the hammer must strike the parent block is approximately 45deg.

It is now also clear why this plane of fracture is called the Percussion face.

We might call flakes thus detached external flakes of the second order, and such a flake would exhibit three faces, viz.: —

(2)
Percussion Face.

Internal (Pollical) Face. (3)

External (1) (Indicial) Face.

The External face being the oldest, the Percussion face the next, and the Internal face the youngest in order of succession. The last two representing planes of fracture, the first the original surface of the parent block.

It must, however, be understood that these features are generally not as simple as here described; frequently the external (Indical) face is considerably changed by striking off small flakes in order to reduce the thickness of the implement. Equally often the Percussion face has entirely disappeared, or is greatly reduced in size by marginal chipping along its external (Indical) edge. This is particularly the case in tero-watta that are carefully worked all round by marginal chipping. It is, however, always possible to locate from the marks of percussion exhibited on the internal (Pollical) face the position of the Percussion face, and it is very seldom, even in the most highly finished tero-watta, that not a trace of the Percussion face can be discovered.

It is, of course, quite feasible to strike off several external flakes of the second order from one and the same parent block, after a good working plane of percussion had been produced by the detachment of the external flake of the first order. No doubt this has been frequently done, but it is also probable that internal flakes were struck off. Such internal flakes should show a portion of the original crust at the distal end (unless it was removed by subsequent chipping), besides a percussion plane, which may, however, also have been removed by marginal chipping. The fine knife-like tero-watta figured in my paper, "Notes on the Tasmanian Amorpholithes" (Fig 23, 23a, 23b), most probably represents a flake of this type.

It will be easily seen how these flakes, which we may call Internal flakes of the second order (Fig. 5a), differ from those of the first order. In the latter there is no real Percussion face, the plane of percussion being formed by the original surface of the parent block. Unless removed by chipping the internal flake of the first order should have a fragment of the original crust adhering at the proximal end, and there may also be some of it at the distal end. The last flake struck off the nucleus from Kempton is a typical example of an internal flake of the second order.

The above characters, distinguishing the different kind of flakes, are summarised in the following table:—

External (Indical) Face formed by the original crust of the parent block: I. EXTERNAL FLAKES.	1. External and Internal Face only, original crust forms plane of Percussion.	External Flakes of 1st order, Fig. 2.
	2. External, Internal, and Percussion Face, plane of Percussion represents a former plane of fracture.	External Flakes of 2nd order, Fig. 2a.

External (Indical) Face formed by the plane of fracture of former flakes: II. INTERNAL FLAKES. (Usually distinguished by one or two longitudinal ridges on the Indical face.)	3. External and Internal Face only, original crust forms plane of Percussion and is if preserved at proximal end.	Internal Flakes, 1st order, Fig. 4 and 4a.
	4. External, Internal, and Percussion Face, original crust is if preserved at distal end.	Internal Flakes, 2nd order, Fig 5. and 5a.

External flakes of the first order are not very common; of this class are the most primitive types of human implements produced from the fracture of siliceous rocks. External flakes of the second order form the great majority of the tero-watta, and they frequently show a most elaborate finish of the Indical (external) face.

Internal flakes are apparently rarer than the former; the last flake struck off the Kempton nucleus forms an exceedingly good type of the first order; the second order is apparently more common than the first, and all the tero-watta, generally called knives belong to this group.

C.—ACCESSORY MARKS OF PERCUSSION ON THE INTERNAL (POLLICAL) FACE.

The accessory marks of percussion are strictly limited to the internal (Pollical) face, where they extend from the internal edge of the Percussion face all over the surface up to the distal edge. These are:—

1. The process of percussion.
2. The cone or bulb of percussion.
3. The concentric wrinkles of percussion.
4. The scar of percussion.
5. The radiating fissures of percussion.

1. THE PROCESS OF PERCUSSION (P.P.)

Pl. II., Fig. 3.

The process of percussion does not often occur; if it does, it invariably forms a kind of projection of the pollical edge of the percussion face, as will be seen from Plate II., Fig. 3.

I cannot find any reference to this peculiar effect of percussion, but it is unquestionable that it represents nothing but the top of an abortive cone of percussion.

Professor Verworn mentions certain features which he calls "conical fissures" occurring on the percussion face. These conical fissures turn their convexity towards the indical face, while the convexity of the process of percussion is directed towards the pollical face.

There is no doubt that both features are closely connected, probably representing a more or less imperfect cone of percussion.

I never observed conical fissures in the Tasmanian tero-watta, while it seems that the process of percussion has not been observed in European archæolithes. This may probably be due to the difference in the nature of hornstone and flint, though it requires further obesrvations before this view can be considered as certain.

2. THE CONE OR BULB OF PERCUSSION (C.P.)

Pl. II., Fig. 6.

Perhaps the most characterisitc feature is the cone, or, as it is frequently called, the bulb of percussion (1). It is always situated at the proximal end of the Pollical face, and its point merges into the Percussion face. (Plate II., Fig. 6)

The occurrence of this cone in the Tasmanian tero-watta is rather peculiar. In the first instance, it represents always a truncated cone, the point being cut off by the Percussion face; secondly, it is always composite, being composed of several cones showing different angles of sides, the top portion showing invariably a more acute angle than lower portion. Generally two cones, separated by a sharp edge, are formed. The top or proximal cone showing an angle of about 30deg., the lower, or distal cone, having an angle of about 60deg. In rarer instances the lower portion is composed of two cones, whose angles, however, differ very little. So far I never observed that the top cone was divided into two portions.

Sir John Evans has given a very ingenious explanation of the origin of the cone of percussion. The only question we might ask is, how is it, that if this purely mechanical explanation be correct, that the cone of percussion is only produced in siliceous rocks and minerals?

(1) The term bulb of percussion was according to Sir John Evans first used by the late Dr. Hugh Falconer.

3. CONCENTRIC WRINKLES OF PERCUSSION (W.P.)

Pl. II., Fig. 9.

The vibrations of the molecules which gave rise to the cone of percussion at the point of impact must necessarily decrease in strength with the distance from this point. Instead of a cone, curious concentric wrinkles are produced, exactly like those caused by a stone thrown into water. Plate II., Fig. 9, illustrates this feature:—

Professor Verworn has already observed that these wrinkles form an invaluable assistance in determining the oroximal end of an archæolithic implement, their concavity being invariably turned towards the point of impact. This is a matter of course, because the point of impact forms the centre from which the vibrations radiate, and the wrinkles produced on the Pollical face must naturally represent concentric circles.

It sometimes happens that one of these wrinkles coincides with the line of fracture. In that case the edge is not sharp, but rounded off, and, therefore, useless for cutting puropses. It requires sharpening by marginal chipping (retcuches). I have several fine specimens in my collection, showing the partly sharpened edge, while another portion still preserves its original rounded-off shape. Considering that the curvature of the wrinkles is turned towards the Indical or External face, it is rather difficult to understand why the marginal sharpening was produced by blows directed from the Pollical towards the Indical face, and not vice versa, which seems so much easier and more effective. This is again one of those problems which puzzle the modern mind, and which I have frequently met with in the course of my researches. The only explanation I can offer is, that the archæolithic Tasmanian could not possibly think of any other way of sharpening the edges than by blows directed from the Pollical face towards the Indical face; it was apparently impossible for him to conceive any other method, and if ever he happened to make a mistake, he promptly corrected it by reverting to the time-honoured method.

4. THE SCAR OF PERCUSSION (S.P.)

Pl. II., Fig. 7.

Frequently there appears on the Pollical face, instead of either cone or wrinkles of percussion, an ellipsoidal mark.

separated by a sharp edge from the remainder of the surface; it usually represents the highest part of the Pollical face, and inside the edge it is slightly concave.

The longitudinal axis generally runs in the direction of the blow, i.e., from the proximal to the distal end, and the top coincides with the Pollical edge of the plane of percussion.

Inside the sharp edge there are sometimes faint concentric wrinkles, but they never extend beyond the edge.

This is the "thumb mark" of the amateur collector, and though there is no doubt that the thumb rested on the flat Pollical or Internal face, the scar of percussion is not an intentional, but purely accidental feature.

A combination of cone and scar of percussion is often observed; in that case the scar commences some distance below the point of the cone, and the concentric wrinkles run diagonally.

5. RADIATING FRACTURES OF PERCUSSION (R.P.)

Pl. II, Fig. 8.

On either side of the marks above described there appear frequently, though not always, short, closely set, splintery fractures, radiating from the point of impact. Sometimes they may also appear on the top cone, but they are generally limited to both sides. It often happens that these radiating fractures are the only signs of percussion on the Pollical face, and then they are just as valuable in determining the point of impact, and therefore the proximal end, as any of the other marks. Professor Verworn was the first who noticed these "Strahlen-spruenge," as he calls them, but a comparison of his figure with a Tasmanian tero-watta seems to indicate that though due to the same cause, the "ray fissures" are not quite identical with the "radiating fractures." Verworn's "ray fissures" are true fissures radiating from the point of impact apparently all over the Pollical face; the "radiating fractures" of the tero-watta are certainly not fissures; on either side of the cone, scar or wrinkles of percussion, close to the point of impact, the surface does not flake smoothly, but the force apparently produces a number of thin lamellæ, which, by breaking off, produce this peculiar feature.

It is more than probable that the different chemical composition of flint and hornstone produces these somewhat different features, of what must be considered one and the same effect of percussion.

CONCLUSION.

The accessory marks above described represent the five principal mechanical effects of percussion, but it must not be supposed that they always occur together in one and the same specimen. In fact, so far, not a single specimen has come under my notice which exhibits all of them simultaneously. The production of accessory marks of percussion is unqestionably influenced to some degree by the mineralogical nature of the rock. The cone of percussion is more frequent in the porphyritic breccia and porcellanite than in chert or hornstone rock; while, on the other hand, the concentric wrinkles of percussion are always well defined in chert or hornstone, while hardly noticeable in porcellanite or porphyritic breccia.

The shattering of the surface is also always much better shown in chert or hornstone than in any of the other rocks.

It is therefore certain that the nature of the rock influences the character of the marks of percussion. The different composition must produce a different resistance to the transmission of vibrations, and as a result we may anticipate the production of certain marks in preference to others in certain rocks.

But though this may be so, it is impossible to say at this stage, what amount of energy was required to produce a certain effect. So far, all we know is that a flake, however large its size, was detached by a simple blow only from the parent rock. It is further very probable that failure was not chiefly due to insufficient energy, but probably more to the hammer stone not striking the surface at the critical angle of about 45deg. The intense shattering of the surface which denotes the ineffective blow proves conclusively that the blow was administered with great force, yet no flake was detached. Insufficient energy can, therefore, not have been the sole reason of failure; it might be argued that the ineffective blow was administered by inexperienced hands; for instance, of children. This may be so or not, it only proves that these hands had not learnt to direct the blow at such an angle towards the surface that the energy was utilised in detaching a flake, and not shattering the surface. The same may also frequently have happened to older and more experienced hands.

Though we can, therefore, to some extent account for the marks of the ineffective blow, it is impossible to say what caused the accessory marks of the effective blow. If we take it that the sole object of the effective blow was the detaching of a flake, any marks accidentally produced during this process must represent wasted energy. In other words, if a certain amount of energy had not been wasted in the production of these marks a much smaller force would have been sufficient to detach a flake. The distinctiveness of the accessory marks of percussion may, in some way, be a measure of the quantity of misspent energy, but this does not explain why either cone, scar, wrinkles, or a combination of these three prominent marks were produced. It is, perhaps, probable that the angle under which the hammer struck the parent block has something to do with the production of these marks. It is certain that the best effect was produced when the hammer struck at an angle of 45deg.; if the hammer struck the surface at an angle of 90deg., the result was most probably intensive shattering, but no detachment of a flake. It is, therefore, very probable that the accidental marks of percussion are a function of the angle under which the hammer struck the surface of the parent block. In all probability they are the results of blows that struck the surface at an angle of more than 45deg. and less than 90deg. This view is greatly supported by the evidence of the tero-watta above mentioned; in the tero-watta from Old Beach, which was detached by a blow that struck the percussion face under an angle of 45deg., there are hardly any accessory percussion marks, while all the others show them to a great extent. It is obvious that the smaller the angle was under which the hammer struck, the less was the effect, and it is more than doubtful that a blow directed at an angle of less than 30deg. will have any other effect except just grazing the surface.

Sir John Evans's observation further seems to confirm this view. He says (1):—"If a blow from a spherical-ended hammer be delivered at right angles on a large flat surface of flint," the result will be the cone of percussion. If this view be correct, the cone of percussion would represent one extreme, the neatly detached flake without any accidental marks, the other extreme of the line extending from 45deg. to 90deg., and all other marks would be produced by blows striking the surface between these two extremes.

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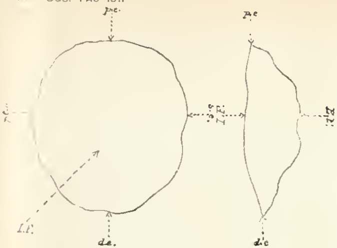
(1) l.c., page 273.

I am, unfortunately, not in the position to verify this theory by experiments, which can only be carried out in a laboratory well equipped for such purposes. It would, however, be of the greatest interest if such experiments were made, if for no other purpose than to prove or disprove the view that such marks can be produced by other than human agency.

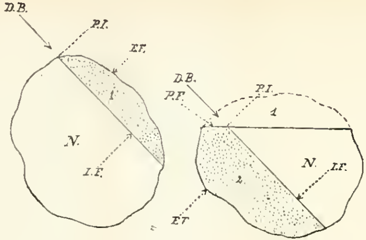
During the early part in the controversy that was going on about the origin of the Eolithes, or, as I prefer to call them, Archæolithes, it has been frequently held that natural agencies could produce such marks of percussion as here described, and even Prof. Verworn assumes that natural processes could produce them. If a siliceous rock falls from a great height on a hard surface, it is very probably broken if the energy developed be sufficient. If the pebbles moved by the energy of a torrent strike against each other, flakes may probably become detached; even if the force of the surf hurls the pebbles of the shingle against hard objects it is possible that they may be broken, but will all this result in the marks of percussion here described? I certainly doubt it; never have I noticed among the shingle broken pebbles showing marks of percussion, nor did I notice them anywhere else.

I maintain that any of the marks of percussion here described, including those of the ineffective blows, cannot be produced accidentally by natural agencies, but only by the agency of a hammer held by a human hand intentionally striking a stone. And, furthermore, in order to produce them it must be a spherically-ended hammer, that is to say, a pebble, which hits the surface in one point only. Even if this view were considered to go too far, it is absolutely certain that all specimens showing a Percussion face, and on whose Pollical face the accessory marks of percussion appear, must be produced by human agency, because it is impossible to assume that a boulder was first divided by any kind of natural agency and afterwards a similar agency acted on the plane of fracture detaching thereby a flake.

My studies have led me to believe that, next to the Percussion face, the five accessory marks of percussion are the surest signs of human agency. Retouches or rough marginal chipping may be produced by natural agencies, tending to press or break off small splinters, but the marks here described can only be produced by a hammer striking one point of the surface, and not penetrating into the matrix.



Figs. 1 and 1a.



Figs. 2 and 2a.

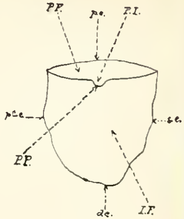
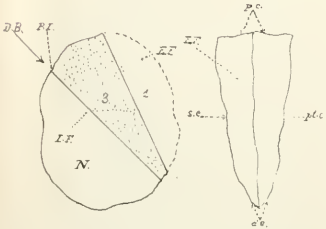
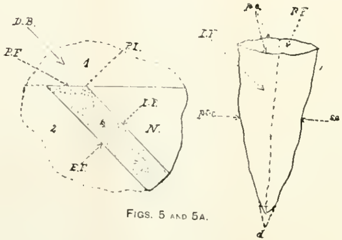


Fig. 3.



Figs. 4 and 4a.



Figs. 5 and 5a.

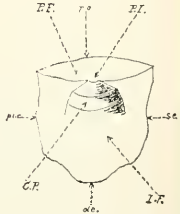


Fig. 6.

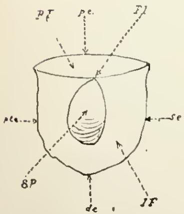


Fig. 7.

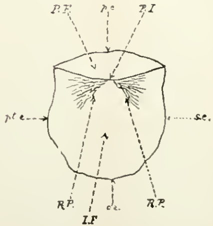


Fig. 8.

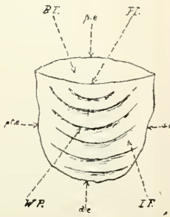


Fig 9

2.—THE HYMENOGASTRACEÆ OF TASMANIA.

Pl. III.

By Leonard Rodway, Government Botanist.

(Read April 10, 1911.)

The researches of systematic botanists in Australia have been chiefly directed to elucidate the members of the more conspicuous phyla. Inquiry into the Flowering plants, Gymnosperms, Pterydophyta, Bryophyta, and the larger marine Algæ has steadily progressed, though many forms belonging to most of these groups yet remain to be discovered and described. But when we come to the important groups of the freshwater Algæ and the Fungi we find information still in a very backward condition. There have been few workers in these groups and of these very few who have really specialised them. Most of the work has been done by students of the higher plants, who could not resist the temptation of collecting peculiar fungi they met with and sending them to Europe, where from time to time they have been recorded. The only works available to Australian students where a general review of the fungi has been attempted have been Hooker's "Flora Tasmaniæ" and Cooke's "Australian Fungi." Besides these, McAlpine has published a classified list of Australian Fungi, and in the Royal Society's proceedings for 1897 appears a classified list of Tasmanian Fungi by myself. The freshwater Algæ of Australia have not yet had the advantage of even a classified list.

The publication of Cooke's "Australian Fungi" is really the first and only general account of the group, and stands as a base from which we could make further advance. There was no pretence that this book included even the majority of Australian species. It was published as a compilation of species known to date, with a full recognition of its incompleteness as a Handbook of Australian Fungi. Numbers of new species have been published since its appearance, and everyone who has made a study of this interesting group is well aware that the number of species yet to be described will probably run into thousands.

Cooke points out in his introduction to the Handbook that Australia is peculiarly rich in the Sub-class Gastromycetes. He says, after quoting figures:—"From this we conclude that Gastromycetes are unusually strong in Australia, certainly including some interesting genera not hitherto discovered elsewhere, but weak in subterranean species."

Discoveries since the publication of Cooke's work still bear out the general statement, but quite upset his conclusion that there is a paucity of underground Gastromycetes; so far from this being the case, Tasmania at least is so rich in these forms that if no more species are in future added from the mainland, it would still place the underground species for the Australian region as very high. Leaving the partially submerged groups, as Scleroderma and Secotium, on one side, and referring to what is generally known as underground forms, which is the sense intended by Cooke, we have in Tasmania twenty-one species, of which nineteen belong to the family of Hymenogastraceæ. This is in a described fungus-flora of under seven hundred species. In England, at the time of the publication of Massee's "Fungus Flora" (1892), there were 4,895 species, and the Hymenogastraceæ contained only twenty-three species.

Judging from these figures, we may conclude that in Tasmania at least, however backward may be the knowledge of other groups, we have described nearly, if not all, of our members of the Hymenogaster family; were it otherwise, we must possess a most astonishing number. The object of the present paper is to bring together our knowledge of this interesting family, information that is not at present at the service of local students. At the end of the paper a record will be included of the genus Secotium, because otherwise some of that group might be easily taken to be Hymenogasters, also because it is directly continuous with it.

For the information of those not acquainted with the systematic position of the family, some general statements may be permitted. There are many classes of fungi, but of these two stand out from the rest by containing all the species that attain a conspicuous size. These two classes are the Ascomycetes and the Basidiomycetes. In the first class the spores are borne in closed sacks or asci; in the second, the spores are borne upon basidia. A basidium is an enlarged cell upon which four, rarely fewer, or more, spicules are formed, upon the apex of each of which a spore is developed. The Ascomycetes contain the little elf-cups so common everywhere; Cyttaria found on our Beech, Morels, the white mould of Roses, some underground species, and other forms which need not be considered further. In the Basidiomycetes the basidia are nearly always very numerous and closely packed upon the surface of gills, tubes, spines, or other apparatus for enconomically enlarging the surface, and, therefore, the spore output, but yet in some genera the surface is plain. We are familiar with gill-bearing forms in such Agarics as Mushroom and most Toadstools, with spinous forms in the Urchin, tube-bearing forms in Punk. In all these the layer of basidia forms a superficial membrane, and on account of this they are grouped together into a sub-class named Hymenomycetes.

But there is a large group of Basidiomycetes in which the basidia are not formed upon an exposed surface, but line convoluted tubes or spaces within the substance of the fungus, and the spores can only escape after maturity by the rupture or rotting of the outer case. This sub-class from the spore development taking place in a body that is enclosed within a coat of barren tissue is called Gastromycetes. We are all familiar with such forms in the Puffballs.

There is much variety amongst members of the Gastromycetes, and consequently they are divided into many families and genera. Most of the forms are superficial at maturity, and a common habit with these and some of the underground genera is for the spore-bearing portion to become dry and dusty at maturity, as in Puffball.

The family which is the subject of this paper consists of irregularly spherical, underground fungi, whose basidia line irregular chambers or convoluted tubes. The substance does not break down at maturity, and no provision is made for the exit of the spores. Dispersal takes place by rotting or more often subsequently to being eaten by small marsupials. Four parts of a tuber will be named. The outer barren coat is the peridium; the spore-bearing substance is the gleba; and there may be a sterile base; also, when the fungus is ripe, the barren part of the gleba between the spore spaces is the trama. The size of these fungi ranges from one to three centimetres diameter. The measurement of the spores is given in micromillimetres. A micromillimetre is $\frac{1}{1000}$ of a millimetre, or, roughly, $\frac{1}{25000}$ of an inch. Students may note that we have two underground tubers belonging to the Ascomycetes that may at first be mistaken for Hymenogasters. They are

Genabea tasmanica. Mass. et Rod. and Stephensia varia, Rod. The very evident spore production within asci will at once indicate where they belong. It will also be evident that classification is very artificial; this in our present knowledge of fungi is unavoidable.

The Hymenogastraceæ, therefore, are subspherical bodies, underground, or accidentally superficial at maturity, whose spores are produced on basidia which line the surface of irregular spaces in the substance of the gleba. The barren tissue of the gleba does not liquify or become in any way broken down at maturity. No special orifice is formed for the escape of spores, nor does the peridium burst at maturity, but the fungus depends for the dispersal of the contained spores upon rotting, or, more commonly, upon consumption by animals.

We have six genera of the family, and the following key will assist in their recognition:—

Spores spherical, nodulose or echinulate.
  Peridium well developed. 1. Hydnangium.
  Peridium, thin or none. 2. Gymnomyces.

Spores oblong, or if nearly globose, they are smooth.

Sterile base, none. Spores oblong, smooth.
  Gleba gelatinous. 3. Hysterangium.
  Gleba normal. 4. Rhizopogon.

Sterile base present. Spores seldom smooth. 5. Hymenogaster.

1. HYDNANGIUM, Wallr.

Peridium fleshy, sometimes thin, and membranous continuous with the trama. Gleba fleshy, crowded with irregular or tortuous spaces. Spores spherical, rough or echinulate, brown or pale. Sterile base well developed to quite obsolete.

Forms with a sterile base are sometimes placed in a separate genus, Octaviana, but this character is not always constant in individuals of the same species.

HYDNANGIUM TASMANICUM Kalchbr.,

Subglobose, pale, 1-2 c.m. diameter. Peridium thick, continuous with the relatively thick trama. Spaces large, 1-3 m.m., irregular, dark brown, giving a marbled appearance in section. Spores brown, covered with very coarse nodules, 13 micron. Sterile base absent.

HYDNANGIUM AUSTRALIENSE, B. et Br.

Subglobose, pale, 1-2 c.m. diameter. Peridium thin. Gleba pale, and exuding white fluid on section at least till old, becoming red-brown. Spaces numerous, small, tortuous, the trama thin. Sterile base sometimes slight in other specimens piercing the tuber to the apex. Spores pale yellow, coarsely warted, 10-12 micron.

HYDNANGIUM CARNEUM, Wallr.

An irregular tuber, 2-3 c.m. diameter, pale pink. Peridium very thin and delicate. Gleba friable, pink, hymenial spaces very crowded, rather large contorted, trama very thin. Sterile base present. Spores white, finely echinulate, 13-18 micron.

HYDNANGIUM ARCHERI, Berk.

"Obovate, small with a large sterile base, without febrils; gleba compact; spores globose, echinulate. 21-22 micron. diameter" (Cooke.) I have not met with this species.

2. GYMNOMYCES, Mass. et Rod.

Peridium none or rudimentary. Gleba fleshy; hymenial spaces numerous, not much contorted, trama thin. Sterile base absent, except in a few isolated tubers. Spores hyaline, globose, rough, or echinulate.

GYMNOMYCES PALLIDUS, Mass. et Rod.

("Kew Bulletin," June, 1898.)

Irregularly spherical, 2-3 c.m. diameter, nearly white, very fragile, with no apparent peridium. Gleba very pale; hymenial spaces about 1 m.m. diameter. Spores minutely warted, 9-10 micron.

In one specimen only amongst a considerable number was any sign of a sterile base found, and then it took the form of a slender process emerging from a depression.

GYMNOMYCES SEMINUDUS, Mass. et Rod.

("Kew Bulletin," June, 1898.)

Very similar in form, size, and colour to the last, but of firmer consistency. Peridium thin, delicate, and silky. Gleba not very fragile, spaces small and irregular. Spores echinulate, 11-12 micron.

3. HYSTERANGIUM, Vitt.

Peridium distinct, and not continuous with the trama. Gleba gelatinous, developing contorted hymenial cavities. Spores smooth, elliptical. Sterile base seldom present.

HYSTERANGIUM FUSISPORUM, Mass. et Rod.

("Kew Bulletin," June, 1898.)

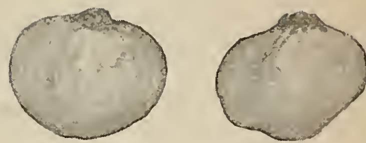
Subglobose, irregular, pale straw coloured, 1.5-2 c.m. Peridium very thin, membranous. A small sterile base is sometimes present. Gleba rather dense, pale, densely packed when mature with minute convoluted spaces. Spores smooth, broadly fusiform, with narrow acute ends, hyaline, 20-22 x 8 micron.

HYSTERANGIUM MEMBRANACEUM, Vitt.

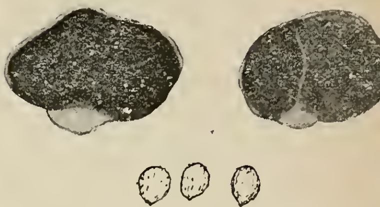
Irregularly spherical, white but readily marking with indigo if touched when young, very delicate consistency when fresh. Peridium thin, dry, white almost floccose. Gleba white when young, marking with indigo where cut, becoming pale brown when old. Spaces very small and numerous, convoluted. Sterile base present, and extending below into a root-like process. Spores elliptic, smooth, sometimes rather pointed at one end, 12 x 5 micron.

FIGS. 1 AND 2.

FIGS. 5 AND 5A.



FIGS. 6, 6A. AND 7.

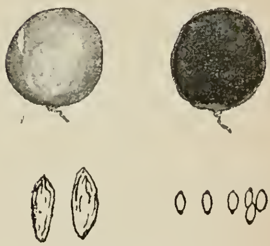


7-8 x 6 *u*

FIGS. 1A., 2A. AND 4.

7 x 4 *u*

FIGS. 9, 9A. AND 9B.



11-13 x 5-6 *u*

FIGS. 8, 8A. AND 8B.

12 x 18 *u*

FIG. 9C.

FIG. 1 - 4—SECOTIUM GUNNII.

FIG. 8 - 8B.—HYSTERANGIUM VISCIDUM

FIG. 5 - 7—HYMENOGASTER VIOLACEUS.

FIG. 9 - 9C—HYSTERANGIUM AFFINE.

HYSTERANGIUM NEGLECTUM, Mass. et Rod.

("Kew Bulletin," Sept. 1899.)

Irregular, 2-4 c.m. Peridium thick, smooth, fleshy sooty-brown. Gleba dense, dark rich brown, the spaces numerous, tortuous not as minute as in most species. Spores oblong, obtuse, smooth or obscurely rugulose, pale brown, 12-15 x 8 micron.

HYSTERANGIUM AFFINE, Mass. et Rod.

("Kew Bulletin," June, 1898.)

More globose than most species, with the longer diameter erect and a root-like process from below, from under one to nearly two centimetres diameter. Peridium rather thick, fleshy pale brown. Gleba dense, somewhat greenish, spaces minute, tortuous, with generally much bluish gelatinous trama intervening. Sterile base very small. Spores pale, bluish green, oblong, not very obtuse, 11-13 x 5-6 micron.

VAR. IRREGULARE, Mass.—Less regular in form; peridium thinner; gleba brownish; spores very obtuse, 10 x 4 micron.

VAR. TENUISPORA, Rod.—Differs from the type in the thinner peridium, gleba darker, nearly black, and in the more slender spores, 12-14 x 2.5-3 micron.

HYSTERANGIUM CLATHROIDES, Vitt.

Very irregular in shape, about 2 c.m. diamter. Peridium thin, floccose continuous, with surrounding mycelium. Gleba grey-hyaline, soft, almost waxy. Spaces not very tortuous nor crowded, narrow, pale brown. Spores oblong, pointed at both ends, smooth, pale brown, 10 x 5 micron.

HYSTERANGIUM VISCIDUM, Mass. et Rod.

("Kew Bulletin.")

An irregular tuber, 3 x 1.5 c.m. Chocolate brown, with a viscid surface. Peridium gelatinous, rather thin. Gleba pale but dotted with the minute hymenial spaces, which are brown from the contained spores. Spores oblong, obtuse, papillate, yellowish brown, 14-15 x 10 micron.

4. RHIZOPOGON, Tul.

Peridium thick or thin, continuous with strands of mycelium which partially envelope the surface. Gleba dense, hymenial cavities very numerous, small, and contorted. Trama very thin subgelatinous. Spores oblong, smooth. Sterile base not developed. The genus is only separated from Hysterangium by the mycelial strands arising from the surface.

RHIZOPOGON RUFESCENS, Tul.

Irregularly globose, 2-4 c.m. diameter. Surface pale at first, then pinkish brown. Peridium very thin, continuous with the trama and the surrounding mycelium, not generally apparent at maturity. Gleba white, sometimes cinerous, or brown when beyond maturity. Spores oblong, obtuse, smooth, 11 x 5 micron.

A common European species, found up to the present in Tasmania only beneath Austrian Pines. Probably introduced with that plant, and symbiotically associated with it.

5. HYMENOGASTER, Tul.

Peridium fleshy, generally thin. Gleba fleshy, the hymenial cavities small and irregular, trama thin, formed of elongated cells, not floccose nor gelatinous. Spores generally elliptic or fusiform, rarely nearly globose, generally rough, papillate, or sulcate, rarely smooth. Sterile base present, sometimes piercing the greater part of the gleba.

Like most genera of this family, not marked by any positive character. Distinguished by the consistence of the trama, sterile base, and elongated rough spores, with which are associated forms that appear to have a closer affinity here than elsewhere.

HYMENOGASTER ALBELLUS, Mass. et Rod.
("Kew Bulletin," June, 1898.)

Irregularly globose, pale, 2-3 c.m. diameter. Peridium thin, cellular, distinct. Gleba pale brown, firm, the spaces not minute. Sterile base reduced to a flat cushion. Spores elliptic, obtuse, or with one or both ends narrow, yellowish brown, minutely warted, 16-17 x 8-9 micron.

HYMENOGASTER NANUS, Mass. et Rod.

("Kew Bulletin," June, 1898.)

Irregularly globose, 1-3 c.m. Sooty brown, with a mucilaginous surface. Peridium rather thick, fleshy, the outer portion gelatinous, separable from the gleba. Gleba firm, light brown, spaces rather large. Sterile base well developed. Spores elliptic, subobtuse, brown, warted, 14-15 x 8 micron.

HYMENOGASTER RODWAYI, Mass.

("Kew Bulletin," June, 1898.)

Irregularly globose generally, 2-3 c.m. diameter, pale. Peridium very distinct, fleshy. Gleba dark brown, spaces small and irregular, showing an indistinct tendency to radiate from the sterile base towards the periphery. Sterile base usually conspicuous and giving off branching veins penetrating the gleba. Spores elliptic or lemon-shaped apiculate at one end, longitudinally ribbed, ribs simple, or forked and anastomosing, strong, converging at the ends, 20 x 10-12 micron.

HYMENOGASTER ALBIDUS, Mass. et Rod

("Kew Bulletin," Sept. 1901.)

Irregularly globose, dirty white, 1-2 c.m. Peridium very thin floccose, continuous with the surrounding mycelium. Sterile base rudimentary or absent. Gleba pinkish white, turning light brown when dry, spaces tortuous larger than in H. Rodwayi. Spores elliptic, pointed at one end, the other very obtuse; longitudinally ribbed or coarsely rugose, pale brown, 21-28 x 14-18 micron.

HYMENOGASTER VIOLACEUS, Mass. et Rod.

(Kew Bulletin," June, 1898.)

Subglobose, violet and viscid, 2-3 c.m. Peridium thin but distinct. Gleba rather firm, brown. Spaces numerous, tortuous, about 1 m.m. diameter. Sterile base obsolete to very distinct in some specimens, a sterile vein running right through the gleba to the apex. Spores globoso-elliptic, brown, minutely warted, 7 x 9 micron.

HYMENOGASTER LEVISPORUS, Mass. et Rod.

("Kew Bulletin.")

Irregularly globose, white, 2-3 c.m. Peridium very thin, subflocose, continuous with the surrounding mycelium. Gleba rather dense, pale brown, spaces small. Spores pale brown, spherical, or nearly so, smooth, 10-11 micron.

5. SECOTIUM.

In Cooke's "Handbook of Australian Fungi" Secotium is placed in Lycoperdaceæ, a family characterised by apical dehiscence and a disintegrated gleba at maturity. In Secotium the trama is persistent, arranged in crumpled plates, radiating from the columella and dehiscence occurs by a basal cleft round the stem. A typical Secotium has a well-developed stem, which pierces the gleba to the apex, and there expands, and is continuous with the peridium. In the lower part the surface of the peridium assumes the character of an arachnoid volva covering the groove. In some Tasmanian forms the stem is almost reduced to the sterile base of Hymenogaster, and then the fungus is seldom exposed above ground, except by accident. S. Gunnii often has the appearance of a deformed Agaric, and the natural position of the genus appears to be intermediate between the Gastromycetes and the Agaric family of Hymenomycetes.

From Tasmania four species have been described.

SECOTIUM ERYTHROCEPHALUM, Tul.

"Gregarious, rather long stemmed; stem erect, smooth, naked, white, narrowly fistulose; peridium innate, simple, even, smooth, carmine-red; cells unequal, large, septa thin, distinct, destitute of flocci, basidia arising from the walls bearing 2-4 spores; spores elliptic, even, brown on long sterigmata, 10-11 x 5 micron."—Cooke's "Handbook." I have seen no specimen.

SECOTIUM GUNNII, Berk.

Irregularly globose, pale, smooth, 2-4 c.m. diameter; stem solid, short, thick, continuous with the peridium above. Gleba pale brown, the tramal plates thin, radiating, and very distorted. Basidia clavate, tetrasporous. Spores brown, smooth, elliptic apiculate at one end, 7 x 4 micron.

SECOTIUM RODWAYI, Mass.

("Kew Bulletin," Sept., 1901.)

Subglobose, fleshy, cream-coloured, smooth or tomentose deeply excavated below. Stem very short, usually piercing the gleba to the apex, where it is continuous with the thin peridium, in some specimens much reduced above. Gleba rather dense, pale cream-coloured. Cystidia and Basidia pyriform about the same size. Spores globose, colourless, verruculose, 7-8 micron.

SECOTIUM SESSILE, Mass. et Rod.

("Kew Bulletin.")

Subglobose, 2-3 c.m. diameter, pure white, and delicate. Peridium very thin. Stem short, usually vanishing in the gleba. Cystidia fusiform, much exceeding the basidia. Spores globose, colourless, minutely verruculose, 7-8 micron.

NOTE ON TRACHINOPS TAENIATUS.

Robert Hall.

(Read April 10, 1911.)

This perciform fish apparently has not been recorded from Tasmania, our specialist, Mr. R. M. Johnston, being unfamiliar with it.

Hitherto only two species have been described, one from New South Wales, the other from Victoria.

The Tasmanian form appears to agree with *T. taeniatus* of New South Wales.

The geographical distribution should be an interesting one, judging from the fact that this conspicuous little form is not contained in the collections of the Australian Museum. Evidently it is local and uncommon. Although Boulenger speaks of the four British Museum specimens of *T. taeniatus* as having been found in New South Wales and Australia, and of their reference to *T. caudimaculatus* as being found on the Victorian coast. Their localities are more likely to agree with McCoy's Port Jackson for *T. taeniatus*, and Port Phillip for *T. caudimaculatus*. MacLeay speaks only of *T. taeniatus* and gives no exact habitat. No mention is made of Tasmania. The specimen referred to in this note I found in August, 1909, among the piles in the old pier adjacent to Argyle-street, Hobart.

A shoal was observed by Mr. A. Kirk opposite the abattoirs, up the Derwent, during the spring of last year. I know of no other records, and it would be interesting to learn if it is found in the Kent and Flinders Groups. This would indicate a broken or continuous distribution, and a part of the fauna of the Bassian coast. If it were found on King Island and not on the Kent or Flinders Groups it would appear as more properly belonging to the Eyrean coast. This will depend on the specimens yet to be collected. The Derwent fish has its middle caudal fin much produced, the canines conspicuous, and has the light longitudinal line along the base of the dorsal fin. Although neither of the mainland species are here for reference, these characters are inclined to weave them. It appears to be sluggish in habit judging partly from the fact that it remained among the mussels of the pile until the pile was hauled up clear of the water.

McCoy, in his "Prodromus of the Zoology of Victoria," figures very faithfully this fish, and I am indebted to Mr. H. M. Nicholls for drawing my attention to it.

4. THE FEATHER-TRACTS OF SPHENURA BROADBENTI: McCOY.

Pl. IV.

By Robert Hall, C.M.Z.S.

(Read April 10, 1911.)

The genus Sphenura is represented only in South-East Australia and in South-West Australia, and in each area by two species. It is a disappearing genus.

The species occupy a similar type of country, and point to an old and closer connection between the two faunas: a land bridge suitably wooded.

At the present time the genus is placed in the sylviidæ with a heterogeneous collection of genera. To compare the pterylosis of these genera would probably throw further light upon their relationship.

The specimens* under review represent four phases:

(A) Approximately four days old.

(B) Seven days later, with the eyes open.

(C) One of two nearly ready to leave the nest.

(D) An adult male for comparison of markings.

Phase A (fig. 1) may be considered as absolutely naked until the third day. If there are any neossoptiles of more than one kind they are vestigial, being represented by rictal bristles, and those probably closely related to filoplumes.

In the earliest stage, as well as in the later ones, the only representation is that of prepennæ on the defined tracts, the quills, and the bristle type about the mouth. Preplumulæ are absent.

*Locality, Otway Forest, Victoria, Oct.-Nov., 1910, by the favour of Mr. Geo. Graham.

There should be a moult in spec. A to provide the single rhachis prepennæ of spec. B: not considering the after shaft, which is present. A section of a follicle of spec. A would probably show this down-like feather making way for the succeeding prepennæ. This view is favoured by the presence of odd specimens of fig. 1 among the more fully developed prepennæ of stages B and C.

Looking at the youngest phase (figs. 1-2), those generally known as downs are absent. The feathers still in the follicles of the tracts of figs. 1-2 are down-like, and with the brown pigment of the true feather. This leads one to believe the first generation has been suppressed. They occupy the same areas that the feathers of fig. 3 will do. These latter (second stage) compare with those of the newly-hatched megapodius†.

Downs are said to be absent in Atrichornis, a second remnant genus also represented in the south-west and south-east of the continent by a single species in each area.

The colour of stage A is uniform bluish grey, excepting the lower mandible, the proximal edge of the upper mandible, the inside of the mouth and the tongue, which are strong lemon yellow.

Figs. 1 and 2 show the feather tracts of the dorsal and ventral surfaces.

Pteryla ventralis. It forms a single tract that is not divisible into the usual short outer and long inner branches. It forms a band of great breadth extending from the shoulder a considerable distance backward, and then narrowing down, but not to meet, thus leaving a distinct and broad apt mesogastrei.

Pt. Spinalis.—A. Rami, all arising from a common base.

B. Two types, one without rami, from a common base, the other having a central rhachis.

C. Two types: one as in A, the other showing a well-developed ramus, with a long rhachis.

D. Contour feathers, as in the latter of C, being much longer. There are few hooklets when compared with the normal quantity in Cracticus and Collyriocincla.

† W. P. Pycraft (Willey's Zool Results, New Britain, etc., Plate XLIX., fig. 6, 1910), kindly lent by Prof. T. T. Flynn.

The tract is oval, tapering off in the lumber region, and becoming attenuated, though not junctioning with the oil gland, and remaining free from other pterylæ.

Pt. Capitis.—Beginning at the anterior nares, it is forked along the culmen, and passes over the frontal parietal and occipital region, bulging downwards, immediately over the apertures of the ears, and later joining the pteryla colli dorsalis, ear-coverts breaking from their follicles.

Pt. Colli Dorsalis. This tract blends with the pt. spinalis and pt. capitis.

Pt. Caudalis.—Ten rectrices in B and C, with the ten coverts well developed, two-thirds the length of the former, while in A there distinctly appears to be twelve rectrices, with ten upper coverts just showing their follicles. Uropygium not tufted.

Pt. Femoralis.—Made up of two sets, a strong outer series and a short feeble inner series at the anterior end. This inner series does not yet show in specimen A (fig. 1). The posterior portion is abnormally long, indicating the habitat as wet and scrubby. This tract is free from any other.

Pt. Cruralis.—The tract does not encompass the leg. In B the whole length of the inner surface is bare, broken by an oblique line of a single row of well-developed yet unburst follicles. There are a few additional feathers in specimen C, the upper part of the tibial area of the leg being bare.

Pt. Humeralis.—Well developed.

Pt. Alaris.—Metacarpo-digitalis 10, subequal to A, B, C, D, the 9th and 10th being a little shorter; veins not emarginated as in the contours, the 11th remex wanting.

Cubitals 10, 1-8 being subequal, the 9th being much smaller, the 10th still much reduced, and scarcely distinguishable from its covert. There is an 11th quill that does not indicate whether it is a cubital or a covert, either in position or its form.

While there is no sign of primary coverts in the left wing of specimen A, there is a sign of them in the right wing, being sufficiently distinct to be counted.

The wing is eutaxic.

Tectrices.—In spec. A the t. majores of the cubitals are 10, and well indicated, the t. mediæ being 6, and well marked, a lens being needed to see the t. minores, t. marginales being not yet visible.

The coverts of the under surface of the wing do not yet show any sign of appearing. The dorsal major coverts of the primaries are not yet visible.

There is no appearance of pt. spuria. In spec. B the veins of pt. spuria are breaking from their sheaths, though not so advanced as the coverts, the major coverts of the secondaries being more advanced than those of the primaries. Only the innermost of the t. mediæ is exposing its vein.

The pt. marginales are just showing. The four rows of tectrices upon the under surface are well developed, but just breaking from their sheaths.

The carpal covert is equal to the t. minores of the cubitals, being much further ahead of the primary medium coverts in their development.

In pt. spuria five strong feathers take part in the formation.

Parapteron is naked in spec A and B, in C there are two rows, each four feathers.

Fig. 3 gives a lateral view of phase B. The veins breaking from the follicles indicate the pale rufous and grey colours that are in C and D. The plumage of C is already rufous-tinted, as in the adult D, being brighter upon the ear coverts than upon the head. The under-surface is devoid of the lunations of the adult, and the throat has a pale cream-coloured band across it, extending outwards to the region below the eye. This prominent mark does not appear on the adult D, its mark of the past.

At this stage the tail coverts are well developed, and more than half the length of the rectrices. In colour they are almost as intense as in the adult. The broad outer

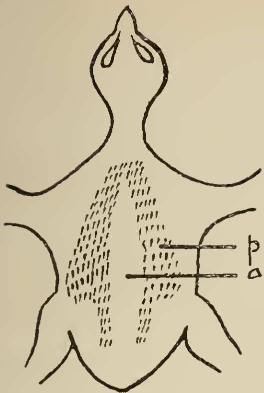


Fig. 1. Spec. A. Vent.

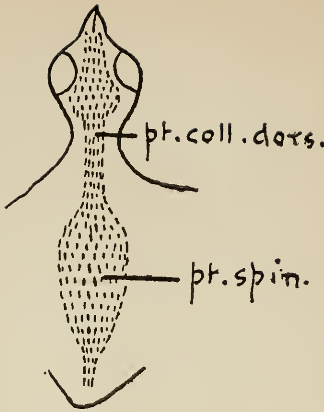


Fig. 5. Spec. B. Dors.

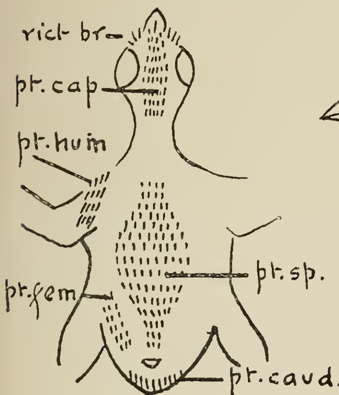


Fig. 2. Spec A: Dors.

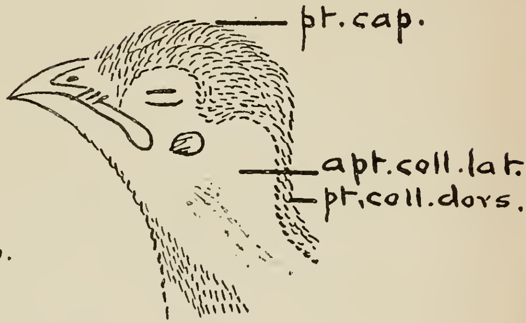


Fig 4: Spec. B: pt. cap etc.

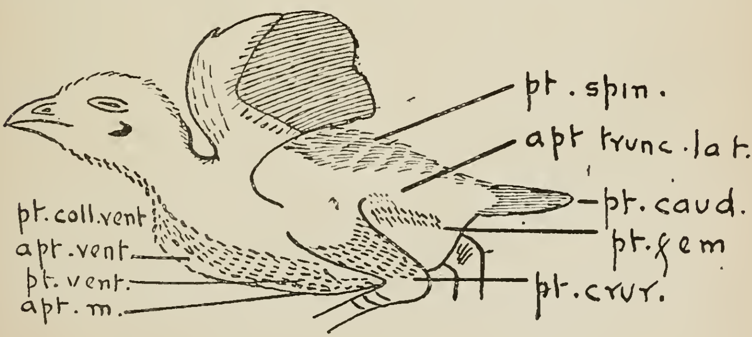


Fig. 3. Side view of spec. B.

margins of the wing-quills and the tail are not so rufous as in the adult. The contour feathers have the same loose appearance as in the adult stage, the hooklets being reduced, and only in part junctioning. On the left side only two rictal bristles are developed, the usual three being upon the other.

The cause of the distinctive markings of the under surface in the adult D is due to the terminal portion of each of the barbs of the feathers being deep brown, and the subterminal portion white in crescentic and imbricate position.

The same locality feathers on C are uniform grey, with the tips of each barb showing a dark hue, but not sufficiently strong to get any other effect than grey upon the breast and chest.

5. THE MANUFACTURE OF THE TERO-WATTA.

By Fritz Noetling, M.A., Ph.D.

Pl. V., VI., VII., VIII.

(Read June 12, 1911.)

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1. GENERAL AND HISTORICAL REMARKS.

Recent investigations have proved that the aborigines obtained the siliceous rocks used in the manufacture of their stone implements from two sources, viz.: —

1. From certain localities where such kinds of rocks occur in situ (1).

2. From the gravel deposits of pleistocene and modern age, in the shape of waterworn boulders.

The former localities have very aptly been termed "native quarries," but it appears that, though these quarries were extensively worked, the material obtained from this source was not of the same importance as that obtained from the gravel beds. Among the specimens collected at Melton Mowbray only 6.1 per cent. could with certainty be identified with the rock occurring in Johnston's quarry, and about 8 per cent. were manufactured from rock found in Nichols's quarry, west. The total of tero-watta made from locally occurring rock, therefore, does not exceed 14 or 15 per cent. Among the Mona Vale specimens 11.3 per cent., and among those from Mount Morriston-Trefusis 7 per cent. were made from rock occurring at Hutchison's quarry. I have been very careful in identifying the nature of the rock, yet there may be mistakes, but on the whole I consider these figures rather above than below the mark.

We, therefore, see that at the outside 15 per cent. of the tero-watta were manufactured from rock obtained in

(1) Noetling — The native quarry on Coal Hill, near Melton Mowbray. Tasman. Naturalist, vol. 1., No 2 Sept., 1907.

Noetling. The native quarry of Syndal, near Ross. Paper and Proceed. Roy. Soc., Tas., 1908.

quarries, while 85 per cent. were manufactured from rocks otherwise obtained. It is very difficult to account for this peculiarity. Johnston's and Nichols's quarries are very conveniently situated near the camping ground of Melton Mowbray, and the same applies to Hutchison's quarry with regard to the camping ground, Mona Vale. Therefore, it cannot be distance that prevented a more extensive use of the quarry rock. We also know that the quarries were intensively worked, as hundreds of thousands of broken fragments conclusively prove. As all the fragments now found at the quarries must be considered as unsuitable rejects, we must assume that the rock obtained in situ was not very suitable for the manufacture of stone implements, otherwise there would not have been such an enormous amount of refuse. To me it seems that the rock obtained from gravel beds possessed certain qualities which the rock obtained from quarries lacked to a great extent. As the most essential quality is a good, smooth fracture, it appears probable that the same kind of rock when obtained from gravel beds had a better fracture than if obtained in situ from a quarry.

The second source from which suitable rocks were obtained are the numerous gravel beds either in the modern rivers, or of earlier geological age. The examination of thousands of tero-watta has conclusively proved that by far the greater majority represent flakes struck off from water-worn pebbles. Such pebbles have been found in all stages of operation. We find pebbles from which one or perhaps two flakes were struck off, tentatively, as it would seem, while dozens or more flakes were struck off from others. For instance, not less than 13 flakes were struck off a portion of a pebble now weighing 14¼ ounces, found near Rokeby, and more than 11 from the Kempton nucleus. My investigations have conclusively proved that the aborigines preferred the rock obtained in the shape of a water-worn pebble to that found in quarries, even if such quarries were situated close to a camping ground. As above stated, I believe that the reason for this preference was the better, cleaner fracture of the water-worn pebbles. We might expect that a piece of rock which has been subjected to the process of being rolled and worn by a torrential current must be of good quality to withstand all this wearing down process. It would appear probable that such a pebble yielded better flakes than a piece of rock picked up in a quarry, whose strength had not been previously submitted to severe tests.

However that may be, the main fact, viz., that the aborigines chiefly obtained the material for the manufacture of the tero-watta in the shape of water-worn boulders and pebbles from the gravel beds of the island remains undisputable. It would be of the greatest interest to know whether archæolithic man of Europe preferred in a similar way flint pebbles found in gravel deposits, to flint obtained directly from the chalk. This question is, however, somewhat complicated, considering the nature of the flint nodules, and I refrain from expressing an opinion.

Now, how were the tero-watta manufactured? The answer to this question is more difficult than it appears, and we will first see whether the historical accounts help to solve the problem. I can only find two references bearing on this question. Scott (1), to whom we are indebted for a great number of important observations, states that he watched an aborigine for over an hour "chipping one flint with another, so as to give them the peculiar cutting sharp edges."

A further observation is contained in Walker's (2) account of the quarry at Plenty. One of the early colonists by the name of Rayner met between 1813 and 1818 a "mob" of aborigines who were busily engaged breaking stones at Walker's quarry. "They were breaking the stones into fragments either by dashing them on the rock or by striking them with other stones, and picking up the sharp-edged ones for use." One old fellow he describes as dashing his stone upon another one on the ground, and leaping up and spreading his legs out at the same time, to avoid as much as possible being struck by the splinters.

This is all I could find concerning the manufacture of the tero-watta, and little enough it is. That a tero-watta was wrought by striking the raw material with another stone is a priori very probable, and the only point of interest in Scott's statement is the length of time. For an hour or so, Scott says, the aborigine was striking the flint, and we may presume, one and the same specimen. Rayner's statement, interesting as it is, does not contain much information either, larger pieces can probably be reduced in size by dashing them against a rock, and if convenient spalls came off they were picked up with the view of shaping them afterwards. The breakage of larger blocks, by dashing them against a hard surface is, therefore, not an

(1) Monthly Notes of Pap. and Proceed. Roy. Soc., Tas., July, 1873, page 24.

(2) Ling Roth, aborigines of Tasmania, 2nd edition, page 149.

essential feature in the manufacture of tero-watta, but merely a preliminary one, to obtain suitable pieces.

We have, therefore, no other means of finding out how the tero-watta were manufactured than the study of the traces the process of manufacture left behind. These are numerous enough, but it required a large number of tero-watta to collect sufficient evidence, and to sort it. From the account of an actual eye-witness (1), we know that two stones were required for the manufacture of a tero-watta, viz.:

1. A piece of (siliceous) rock which was to be turned into an implement.

2. Another stone to strike the former with.

In other words, a hammer-stone and an object-stone. The hammer-stone was actively employed, that is to say, it was used to deliver the blows; the object-stone was passively employed, that is to say, it was subjected to the blows delivered with the hammer-stone.

The object-stone may be of two kinds; it was either a natural pebble, or boulder of siliceous rock, which we may term the parent block, or it represented a flake struck off the parent block. Primarily we may take it that the object-stone was represented by a natural block or boulder, and the effect of a well-directed blow was to divide the parent block into flake and nucleus. (2).

All this appears to be very plain and simple, yet if we come to examine a larger number of tero-watta we at once observe specimens, which are difficult to classify. Are they hammer-stones, or do they represent nuclei? Are they to be considered as unfinished rejects, or as nuclei? It is obvious that it makes a great difference whether I consider a specimen as an actively used hammer-stone or as a passively used nucleus, and yet in many instances it is almost impossible to say which is which. Furthermore, if we consider that it is often enough impossible to discern a true hammer-stone from a sacred stone, or the latter from an anvil-stone, the great difficulties are obvious.

I will here attempt to solve these problems by studying the evidence handed over to us on the actively and passively used objects, that is to say, hammer-stone and object-stone.

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(1) Scott l.c.

(2) See also: The effects of percussion on siliceous rocks

2. EVIDENCE OF THE HAMMER-STONES.

It seems easy enough to discern a hammer-stone. Rutot has so well described the marks produced by blows that it seems almost ridiculous to be in doubt whether a stone is a hammer-stone or not. Yet, if we collect a large number of specimens considered to be hammer-stones, we perceive at once that the matter is by no means so easy. The definition of the hammer-stone requires that it should be actively used, but we find specimens which show, from the position of the marks of blows, that they could not possibly have been used actively, but that they were subjected to blows, in other words, used passively, and that they, therefore, cannot represent hammer-stones. Marks of blows alone do not characterise a stone as a hammer-stone, a fact that has been conclusively proved by the study of a large number of specimens.

A stone showing marks of blows may be—

(1) A true hammer-stone.

(2) A tested pseudo-nucleus.

(3) A sacred stone.

(4) An anvil-stone.

The great difference between these four groups is obvious, yet it is not always possible to say to which group a certain specimen belongs, so imperceptibly are they merging into each other. It may, perhaps, be possible to discern in future between the marks of active and passive blows, that is, to know whether a specimen showing marks of blows was actively used as a hammer stone, or passively subjected to blows as an object stone, but for the present there is no criterion to discern these marks.

There are, however, other features which will assist us to discern true hammer-stones. It is almost pretty certain that in order to break a larger boulder of siliceous rock, no other than diabase pebbles were employed. This seems a priori very probable. Diabase is a tough rock, chert, hornstone, or the other siliceous rocks used in the manufacture of tero-watta are brittle, and break easily. If, therefore, a siliceous rock were used as hammer, in order to break another siliceous rock, it might happen that the hammer, but not the object-stone broke.

It is, therefore, more than probable that all these stones of that kind from which the tero-watta were manufactured, viz., chert, hornstone, porcellanite, breccia, showing marks of blows, cannot be considered as hammer-stones, but must be considered as tested and rejected parent blocks (pseudo-nuclei).

This limits our field of research to some extent, as we have to consider the diabase boulders or pebbles only. Now, among this class there are a certain number which form a most conspicuous group. These are generally very regular, oval, flat pebbles, showing in the centre of either both or one face only a rough indentation or mark. The edge shows either marks of blows all round, or else at the two poles only, or at the two poles and in the middle of the two longitudinal sides. Frequently the formerly rounded edge is flattened by grinding. These stones have been considered as typical hammer-stones, a view with which I cannot agree. It would lead too far to discuss here my reasons, and I must refer the reader to a preliminary paper on this subject. (1).

It is certainly very remarkable that only a few specimens of this type have been found which are not made of diabase, but of a very hard splintery quartzite. It is further noteworthy that not one of these stones has been found in a quarry, while ordinary hammer-stones are very common. Now, if these stones were hammer-stones, why were they not used in the quarries where they were certainly urgently required? Why are they only found on camping grounds?

If we exclude this group, there remains only a small group of stones which must be considered as hammer-stones. Yet even among these there are a number, particularly when found on camping grounds, which appear very doubtful as to their true character. They may be hammer-stones, yet there is a probability that they either represent unfinished sacred stones or a special group of the latter. A further discussion of this question must form the subject of another paper.

Here I will deal only with those specimens of which I am certain that they were used as hammer-stones. These are the diabase pebbles found among the rejects in the native quarries (2). There cannot be the slightest doubt that

(1) Noetling.—Some implements of the Tasmanian aborigines, the magic stones, Tasman. Naturalist, vol. I., No. 3, December, 1907, page 1.

(2) See also J. B. Walker, the Tasmanian Aborigines, Hobart, 1900 page 8.

these diabase boulders were used as hammer-stones. In the first instance they are very battered, and almost every one is in a fragmentary, broken condition. The presence of such diabase boulders among thousands of broken fragments of hornstone is the surest sign that they were carried to their present resting place by human agency. Their battered condition proves that they were used for some heavy work, and the only conclusion we can draw from their nature is that they were used as hammer-stones. I weighed 17 specimens of these hammer-stones, which I collected at Nichols's quarry, the weights ranging from 5¼ ounces to 1lb. 5oz. As all the specimens lost considerably during use, their original weight must have been higher, but it is rather difficult to say anything about the loss. Only six out of the 17 exceeded one pound in weight, but as seven more weigh from 12 to 15 ounces, it is pretty safe to say that in their original state these stones weighed from 1lb. to 2lb.

Now, if we examine these hammer-stones we find that they all show a more or less spherical or globular shape. Not in a single instance have I found one of the flattened, oval type, showing rough indentations in the centre of either or one side only. We might well ask why is it that if this last-named group of stones were hammer-stones, they were used at the camping grounds only, and not at the quarries? The hammer-stones had to be brought to the quarry, and the evidence of the specimens proves that they were globular diabase boulders, probably water-worn pebbles. Now, if it was found necessary to provide the so called hammer-stones with a mark for the insertion of the thumb and another finger, why were the unquestionable hammer-stones of the quarries never provided with these marks?

The evidence of those specimens whose use as hammer-stones is beyond doubt, goes to prove the following facts:—

1. Diabase pebbles only, and no other kind of rock, were used as hammer-stones.

2. It appears that the essential feature of such a pebble to serve as hammer-stones was its spherical or globular form. (1). Compressed or flattened pebbles were apparently never used as hammer-stones.

3. The great majority of the hammer-stones weighed from 1lb. to 2lb., though, of course, there may be heavier

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(1) See also antea, page 43.

and lighter ones, but boulders of that weight were apparently the most serviceable.

4. These boulders were used witnout any previous treatment; in fact, they may be considered as true "eolithes."

5. The compressed diabase pebble of oval shape, showing various marks of blows along the edge, and a central rough indentation on either one or two sides, cannot be considered as hammer-stones, whatever else their use or meaning may have been.

3. WERE ANVIL-STONES USED IN THE MANUFACTURE OF THE TERO-WATTA?

Dr. Rutot, in his important paper, "Un Grave Probleme" (1) thinks that he can distinguish anvil-stones among the collection of specimens I sent him, but I am afraid that, as far as the specimen so designated is concerned, I cannot agree with him. I have not found a single flake which I could declare as an anvil-stone, and it will, therefore, be useful to discuss the question whether anvil-stones were ever used at some length.

The accounts of eye-witnesses are silent on this point. Scott does not state that the "flint" which was chipped with another rested on another stone, viz., an anvil. In fact, his statement almost seems to imply that the flint which was chipped, was held by one hand, while the other wielded the hammer. We are, therefore, obliged to study the tero-watta in order to ascertain whether they bear traces of having rested on an anvil-stone or not. It is pretty certain that if a piece of hornstone rests on a hard support, while it is hammered at, those portions of its surface that have been in contact with the hard support, must become somewhat dulled. Now, as we know that the tero-watta were wrought by blows that were directed from the Pollical face towards the Indical face, a flake must have rested on its Indical face while the process of trimming it was performed, if an anvil-stone was used. The traces of having rested on a hard support should, therefore, be found on the Indical face, but the result of such an examination is absolutely negative. Among the thousands of specimens I examined, there is not one whose Indical face shows marks of having rested on a hard support. All edges are exceedingly sharp,

(1) Bull. Soc. Belge de Geol, Palæont et Hydr, vol. XXI., 1907.

in fact, it is difficult to imagine how some of the specimens could exhibit and preserve such a fine Indical face, unless the flake was held in the free hand, while the other wielded the hammer.

The evidence of the tero-watta themselves, therefore, goes to negative the assumption that an anvil-stone was used when they were made.

As far as the evidence of the Kempten nucleus and its spalls goes, it seems to indicate that it did not rest on another stone or hard support while it was broken, but was probably mostly imbedded in the soft sand of the camping place. The Kempton nucleus does not support the theory of the use of anvil-stones, and the arguments in favour of its use at all are not very strong. It would be ludicrous to assume that the Kempton boulder was broken at some other place affording a hard natural surface as support, and that afterwards the core and all the flakes, even the smallest, were brought to the camping ground simply to be left there. If anything appears to be certain it is that the Kempton boulder was broken at the place where its fragments were subsequently found, but there is no proof that it rested on a hard support.

Now, if any supports whatever were used—and if we admit the præmisse we must assume that they were habitually used—where are they? If they existed they must be recognisable, because if a hard boulder is broken on a hard surface, the effect of the blows which broke it must also leave some marks on the support when the boulder rebounded under the effect of the heavy blows.

I have passed the whole inventory list of the specimens found on the camping grounds and elsewhere, and the only objects that could possibly come in consideration are those I have described as "magic stones." The flatness of these pebbles would render them very suitable as a support. The queer central indentations could be considered as the result of the rebounding of the block to be broken (1) and the peripheral hammering would result from the hammer-stone striking or touching the anvil-stone.

This theory would in some way explain the great variety of these remarkable stones, and also why they are

(1) When the stone was turned over the indentation on the opposite side would be produced.

never found in quarries, where the natural surface afforded a good hard support. Yet there are such a number of very weighty arguments against this view that I am not inclined to accept it, unless convincing evidence is forthcoming. In the first instance, it seems to me, that if a hard boulder is broken on another, the marks which the former left on the latter ought to be spread all over the surface, and not to be concentrated in a central space of a few millimetres in diameter (1). Further, if the peripheral marks are those of the hammer, how is it that they so frequently occur only on four opposite points, and why are they, particularly those on the longitudinal side, frequently flattened, just as if the edge had been ground? It is true that specimens occur whose edge is hammered, or even flattened all round, but often enough these specimens are without central marks. Another important point is the comparative smallness of these boulders. I cannot well imagine how a boulder of the size of the Kempton one rested on one of these small, flat pebbles while it was broken. Further, why should these anvil-stones so frequently be polished, even actually ground, like the specimen from the Old Beach? Is it probable to assume that the aborigines bestowed more labour on their anvil-stones than on the implements themselves, which were in the last instance the desired object of all the hard labour applied? All these are such weighty arguments against the theory of the indented pebbles to be taken as anvil-stones that I do not feel inclined to accept it. Yet, if anvil-stones were used at all, there are no other objects known but those stones that could have served for such a purpose.

However that may be, if anvil-stones were used at all, they were not represented by flakes of hornstone split off from a parent block, as Dr. Rutot assumes. In Europe such flat pieces or slabs of flint may have served as anvil-stones, but not in Tasmania. We have here no similar pieces of hornstone, and the anvil-stone such as mentioned by Dr. Rutot would first have to be manufactured. For the present there is little or no evidence to show that anvil-stones were used in the manufacture of the tero-watta. The only evidence, viz., that of the implements themselves, goes to prove the contrary, and I, personally, feel inclined to discredit the alleged use of anvil-stones altogether.

(1) There are no doubt some specimens which show the marks of blows all over the surface, but I cannot understand how the central indentation could originate while the surrounding surface remained perfectly smooth.

4. THE EVIDENCE OF THE NUCLEI OR CORES.

What constitutes a nucleus or core? The answer seems simple enough: any piece of rock that remains after one or more flakes were struck off represents a nucleus or core. The study of the tero-watta has, however, shown that it is not always easy to distinguish between a nucleus and an unfinished reject, that is to say, a flake that was struck off a parent block, but was not finished. Further, other specimens have been found which conclusively prove that though one or even more flakes were struck off, they cannot strictly be considered as cores. These specimens were apparently only tested as to the suitability of the rock. At Droughty Point I found a splendid specimen of this type, and a large number of these remarkable specimens were found at Devonport, but the most interesting of all came from Shene.

There is no sharp, well-defined limit between nucleus, pseudo nucleus, and unfinished reject. They pass so imperceptibly into each other that it is often absolutely impossible to decide which type a certain specimen represents. On the other hand, if a large number is collected, there will always be a few specimens which leave no doubt as to their nature.

I will, therefore, deal with the evidence of such specimens only which leave no doubt as to their character, taking the nuclei or cores first.

(A) NUCLEI.

Though a number of specimens have come under my notice which must unquestionably be considered as nuclei, none is so convincing and absolutely certain as the Kempton nucleus (1).

I found this specimen on the eastern slope of a hill north of Kempton known as the Sisters, and I first discovered, what we may now term the core, representing, apparently, about half of a large water-worn pebble. I also found 41 flakes which could all be fitted to the core, and the most interesting of all was the last flake that was struck off the core, of which I had previously made a cast. The

(1) Notes on a chipped boulder found near Kempton. Pap. and Proceed. Roy. Soc., Tas., 1908.

core weighs 5lb. 10oz., the total of the 41 flakes is 2lb. 15oz.; core and flakes weigh, therefore, 8lb 9oz. in the aggregate, but as the top portion is still missing, the weight of the original boulder was probably not less than 10lb.

The spalls that were struck off this boulder exceed more than 41, and vary considerably in size and weight. We can distinguish external and internal flakes, and the last one that was struck off a typical internal flake of the 1st order weighs almost 4 ounces. All further work was stopped after this flake had been struck off, and we must, therefore, consider it as the desired object. This view is further borne out by the fact that many of the flakes previously struck off seem by the sharpness of their edges eminently suitable as implements, yet they were disregarded.

Unless we believe the very improbable theory that an aborigine amused himself by striking off about half a hundred of spalls from a parent block with no object at all, we must take it that the object of all this hard work was the production of a flake of either certain weight or shape, or of both. So far no evidence has been found that the shape of a flake was material, and we must, therefore, assume that it was desired to produce a tero-watta of a certain weight, and as weight is dependent on the size, we might also say of a certain size.

It may seem somewhat rash to generalise from one specimen only, but the Kempton nucleus seems to prove that whenever a pebble of suitable rock was broken, it was with the view of obtaining a flake of a desired weight (and size). All others were disregarded, no matter how suitable they may appear to us. This view is borne out by the evidence of the quarries. I have repeatedly pointed out that it appears unintelligible that such a number of apparently eminently suitable flakes were rejected, while others that seem to us much less suitable were used. There is only one explanation for this fact, viz., that the primary object was to obtain a flake of a certain weight (or size). Sometimes a larger, sometimes a smaller, flake may have been wanted, but, however suitable the other flakes that fell off may have been, they were disregarded.

(B) THE TESTED REJECTS (pseudo-nuclei).

As stated above, there is another group of pebbles and boulders which has been subjected to a certain amount of hammering, which cannot be considered as hammer-stones, or as nuclei, strictly speaking.

Mostly one, sometimes two, and in very rare cases more flakes were struck off such a pebble, but the remainder was left intact. It is certainly by no means accidental that, with very few exceptions, all these pebbles consist of a more or less saccharine quartzite of varying colour. Fine specimens of this type were found at Devonport, the Arthur River, at Shene, and Droughty Point.

These specimens always show at the point where the flake was struck off a peculiar percussion mark. This is usually a small semi-circular indentation of about 5 mm. in diameter, which deeply penetrates into the matrix of the pebble. Within the area of percussion the matrix is so intensely pulverised that the surface assumes a whitish colour. Almost in all cases a flake of greater or smaller size became detached, though in one specimen from Devonport the result of the blow was a deep roundish hole only. As already stated, there are usually one or two, but very seldom more than two, of these percussion marks.

Now, the question would arise, are these pebbles to be considered as hammer-stones or not; in other words, were they actively used or passively subjected to blows? I do not think that they can have served as hammer-stones. The evidence of the true hammer-stones shows that they were used till they broke into fragments. Now, if these stones were used as hammers, why was there only one, perhaps two, points used, while the remainder of the edge remained perfectly intact? To me it seems extremely improbable that one or, perhaps, two, blows were executed with such a stone, which was afterwards thrown away, though it was perfectly intact along the greater portion of its edge. Further, I cannot believe that the deep percussion mark, showing an intensely pulverised surface, is the result of an active blow. Such a mark can only be produced if a pebble is passively subjected to a blow, and I, therefore come to the conclusion that it is impossible to suppose that pebbles of this type served as hammer-stones.

Neither do I think that these pebbles can be considered as nuclei s.s. If they were such, why should only one or two flakes have been struck off, if the rock was suitable for the manufacture of implements? I rather think that they must be considered as material that was tested as to its suitability, and on being found unsuitable, were rejected.

When material was required boulders from the gravel beds were collected, and it is more than probable that among a number of them there were a certain number, which, though seemingly hard and suitable, were really unsuitable. These boulders were tested by striking off one or two flakes, and if they were found lacking that essential quality for the production of a tero-watta, viz., a good conchoidal fracture, they were rejected. It is by no means surprising that almost all of these rejects are pebbles of saccharine quartzite, which does not fracture like the homogeneous hornstone.

The astonishing part, however, is that the aborigines ever did collect such quartzite pebbles. One ought to assume that long experience taught them to distinguish a quartzite boulder from a hornstone boulder. But what is more, one detached flake ought to have been sufficient to prove the suitability of the material or not. Yet frequently two, three, or as in the instance of the Shene pebble, some six tests were made before it was finally rejected. This is again one of those psychological problems that we so frequently meet in our studies of the civilisation of the Tasmanian aborigines.

A modern mind would soon learn to distinguish quartzite from other pebbles suitable for the manufacture of a tero-watta. But even if in special cases somewhat doubtful, a single test would be sufficient to prove whether the material is suitable or not.

Having proved that these specimens must be considered as tested rejects, we will now examine the percussion marks somewhat more closely, because none of the nuclei of finished tero-watta presents similar marks, except in cases of an ineffective blow. Even in that case there is a slight difference between the marks of an ineffective blow produced on hornstone and those of the effective blows on the pseudo-nuclei.

Exactly the same percussion mark can be produced if a well-tempered nail is placed on the surface of a quartzite pebble and a sharp blow is administered on its head. Of course it is absurd to assume that the aborigines used a nail or other sharply-pointed iron chisel to split the pebbles, but it may be probable that they placed the sharp point of a piece of rock on the pebble, and administered a sharp blow to this chisel.

However tempting it may be to assume, that the aborigines had learnt to split pebbles by means of a kind of chisel, I do not think that such a theory is in harmony with all the other facts we know as to their state of civilisation. I rather feel inclined to think that these peculiar deeply penetrating marks of percussion showing an intensive pulverising of the matrix are in some way connected with the physical constitution of the rock. With all reserve I may advance the view that the homogeneous hornstone is less elastic than the saccharine quartzite, and that while the former readily fractured when subjected to a blow coming under the effective angle, the latter resisted more strongly to the fracturing energy, and this resistance resulted in a deeper penetration of hammer into the matrix than would have taken place had the rock readily yielded to fracture.

(C) THE UNFINISHED REJECTS.

The evidence deduced from these specimens will come under the following heading, as it is essentially the Indical face that shows marks of being wrought.

5. EVIDENCE OF THE INDICAL FACE.

If we examine a large number of tero-watta we always find a number of specimens whose Indical face is more elaborately worked than that of others. We also perceive that these specimens are distinguished by a smooth, level Pollical face. So far I have not found a single specimen which has an elaborately wrought Indical, and a rough, uneven Pollical face. We may find specimens having a nice smooth Pollical face, whose Indical face shows hardly an traces of being trimmed, but we will never find a rough Pollical face combined with an elaborately chipped Indical face.

This fact proves conclusively that the production of a good, smooth, level, Pollical face was an essential feature in the manufacture of a tero-watta. Only such flakes that possessed this quality were further wrought, should they otherwise be considered as suitable.

It is obvious that the trimming of the Indical face was only necessary when the flake showed considerable thickness, and was, therefore, unhandy. In most cases the external flakes will have been submitted to this process, while the internal flakes, which were mostly of smaller thickness, did not require further reduction.

The trimming of the Indical face was invariably carried out in such a way that the blows were directed from the Pollical towards the Indical face, but never in the reverse way. This is another essential feature in the manufacture of the tero-watta, and R. M. Johnston (1) was the first who drew attention to this fact. There is no doubt that a good deal of unnecessary controversy in discussing the nature of the European archæolithes would have been avoided had Johnston's observation not been entirely overlooked. The fact he established as far back as 1888 had to be rediscovered, so to say, by Verworn (2) in 1908.

When the Indical face was trimmed it apparently happened not unfrequently that the blows did not have the desired effect. If it became impossible to reduce the thickness, the flake was rejected, no matter how much work had already been spent on it. One of the finest instances of this type that has come to my notice is the magnificent specimen found at Mona Vale. Its large thickness, 78 mm., and its weight of 3½lb., make it a most unwieldly tool, and it would require a giant's hand to grip and handle it (3).

Now, I observed that every time, when a tero-watta showed great thickness, the sides of the Indical face formed an angle of 80deg. to 90deg. with the Pollical face, while in those whose Indical face was well wrought the sides formed an angle of 45deg. to 60deg. with the Pollical face. This observation further confirms the view expounded in a previ ous paper that the effective angle under which the blow must strike the rock must be about 45deg. If it was impossible to direct the blows at this angle, it was also impossible to detach further flakes, thus reducing the thickness of the tero-watta, and the specimen was rejected as useless. Generally speaking, these unused rejects can be recognised by a saw-like edge, showing no marks of use.

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(1) Geology of Tasmania, 334.

(2) Ein objectives Kriterium fuer die Beurtellung der Manufactnatur geschlagener Feuersteine, Zeitsch, f. Ethnol, Heft. 4, 1908, pags 548 (page 555).

(3) The weight of this specimen appears more striking still if we bear in mind that 74.6 per cent. of tero-watta weigh under 8 ounces, while only 1.3 per cent. weigh more than 3lb.

6. EVIDENCE OF THE MARGINAL CHIPPING.

In a previous paper I pointed out that the origin of sharpening the edges of a flake was probably due to the peculiarity of homogeneous siliceous rocks, to produce sometimes a rounded instead of a sharp, cutting edge when the flake was struck off the parent block (1).

Now, though it is pretty certain that the flakes were struck off from the parent block by means of a spherical or globular hammer-stone, sometimes of considerable weight, it is very difficult to assume that the delicate and regular marginal trimming was done with such an implement. When I find a flake of 70 mm. in length, having a thickness of 2.3 mm. only, whose edge is most carefully and delicately worked by chipping off small regular flakes, I wonder whether this work can be done by means of a clumsy, globular stone?

If it was done in this way, the Tasmanian aborigines must have been exceedingly dexterous in wielding the hammer-stones, because the marginal flakes have often been struck off in such a regular manner that it required the greatest accuracy to direct the blow. To a modern mind it seems almost incredible that such regular delicate work could be done by means of a rough, clumsy hammer; yet, as we will presently see, it was done in such a way. We know that in the higher palæolithic stages the finer trimming of the implements was done by means of a special instrument, made of bone, by which thin flakes were pressed off. As the use of bone for implements was unknown to the Tasmanians, it is highly improbable that they applied such an instrument for the finer trimming of the tero-watta (2). We may, therefore, dismiss this theory at once.

Another theory, which is strongly supported by Dr. Rutot, assumes that the marginal chipping of the European archæolithes was done by means of a sharp-edged hammer, which he calls "tranchet" or "retouchoir." This may have

(1) This feature is, I may say, not limited to the Tasmanian hornstones, etc., but seems to be common to all homogeneous siliceous rocks having a conchoidal fracture. Among the specimens from Chelles which Dr. Rutot kindly sent me, I found a flint flake whose edge was rounded off exactly in the same way as exhibited by some tero-watta.

(2) I may add that if the aborigines had used such an instrument, it would not have escaped such an acute observer as the late Mr. Scott, and we certainly would have found pieces of bone indicating that they were used for such a purpose.

been so or not; I am not in a position to decide one way or other. Probably Dr. Rutot is quite right, but, unfortunately, we cannot say with certainty whether such an implement was used by the Tasmanians or not.

We have it from an eye-witness that they were "chipping one flint with another." We know that in certain instances the "flint" used as a hammer was a spherical diabase pebble, but, unfortunately, we do not know whether Scott's "flint" which was used as a hammer was such a diabase pebble, or whether it can be interpreted as a tero-watta made of hornstone, serving as a "retouchoir."

As already stated, it seems very improbable—at least to the modern mind—that a clumsy diabase pebble was used for the delicate marginal chipping and a priori it would seem more probable that another implement which could be handled with greater accuracy than a pebble was used. We will now investigate whether there is evidence to show that this was the case.

There is a certain group of tero-watta which are distinguished by a curious jagged saw-like edge. As the implement known as "saw" was unknown to the aborigines, though they unquestionably executed sawing movements when cutting a stick or a spear, we may dismiss the view that these tero-watta represent saws. What is more, they do not show any traces of use, the "teeth" of the edge being quite sharp and pointed. A closer examination proves that the blows which detached the flakes between the teeth were not placed quite close to each other, but at certain intervals. This view is fully borne out by a specimen from Brighton, which distinctly shows the traces of three blows placed in the way here described. Now, it is unquestionable that a number of blows, which are not close to each other, can be executed by means of a spherical hammer, as I have convinced myself by experiment. If, then, a second series of blows is directed against the same edge, by which the jagged points are removed—and it will be noticed that again these blows are not placed close to each other—the edge became perfectly sharp, and the flakes appear to be struck off with that regularity which appears so astonishing to us.

We see, therefore, that it is not necessary to use a sharp-edged hammer for marginal trimming, and that this can be done equally well by means of a spherical hammer in the way here described. The specimens showing a saw-like edge have, therefore, to be considered as unfinished rejects, and I feel obliged to withdraw the view first promulgated by me in a previous paper that sharply-edged stones were used for marginal trimming (1).

It is greatly to be regretted that Scott never inquired into the nature of the "flint" used as a hammer; if he had all the above speculations would not have been necessary.

7. EVIDENCE OF WEIGHT AND SIZE.

I weighed and measured 75 tero-watta which I selected at random from a large collection. All specimens were perfect, but it is more than probable that some of the largest specimens, patricularly the Mona Vale specimen, represent unfinished rejects, which should not properly be included among the implements actually used. I further took great care that none but tero-watta that had actually been used were examined. I admit that 75 specimens is a small number only, but I do not think that much would have been gained by weighing and measuring a larger number.

A.—WEIGHT.

The heaviest specimen weighed 3lb. 8oz., but this must in all probability be considered as an unfinished reject. The lightest specimen weighed not more than 96 grains, yet it showed distinct marginal chipping. The results are summarised in the following table:—

2 ounces and under: 20 specimens, equal to 26.6 per cent.

2 ounces to 4 ounces: 24 specimens, equal to 32 per cent.

4 ounces to 8 ounces: 12 specimens, equal to 16 per cent.

8 ounces to 1lb.: 8 specimens, equal to 10.6 per cent.

1lb. to 2lb.: 7 specimens, equal to 9.3 per cent

2lb. to 3lb.: 3 specimens, equal to 4.0 per cent.

More than 3lb.: 1 specimen, equal to 1.3 per cent.

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(1) Stud. ueb d. Technik der tasm. Tronatta Arch. f. Anthropol. N.F., Vol. VIII. Heft 3, page 204.

This table presents some striking features; 56 tero-watta (74.6 per cent.) weigh under eight ounces; only 19 (25.2 per cent.) are above that weight, and even in that small number there are included specimens which, strictly speaking, should not have been mentioned. However that may be, these figures prove conclusively that the tero-watta was an implement of light weight, and as such it was not particularly suitable for any heavy work. This view is still more emphasised if we consider that 44 specimens (58.6 per cent.), that is to say, considerably over one-half, weigh under 4 ounces.

The above figures make it appear that the largest number, viz., 24, equal to 32 per cent., weigh from 2 to 4 ounces, the lighter, but particularly the heavier weights, declining rapidly in number. Now, if we assume that the most suitable weight was from 2 to 8 ounces, we have:—

(a) 2 ounces and under: 20 specimens, equal to 26.6 per cent.

(b) 2 ounces to 8 ounces: 36 specimens, equal to 48.0 per cent.

(c) More than 8 ounces: 19 specimens, equal to 25.2 per cent.

The proportion of these three classes is rather remarkable, as we have:—

a: b: c equal to 1: 2: 1.

And I do not think that it is purely accidental. As I stated above, I selected the specimens at random, and if we find the examination of 75 specimens proves that out of 4 tero-watta 2 weigh between 2 and 8 ounces, while one is above and one below that weight, we must conclude that this really represents the true proportion.

B.—SIZE.

The largest specimen I found measures 206 mm. in length, while the smallest measures not more than 24 mm. Specimens measuring over 100 mm. (4-inch) represent only 30.6 per cent., while 69.4 per cent. remain under that size. Only 6 specimens that are under 100 mm. in length weigh more than 4 ounces, but none of them weigh more than 7 ounces. We have, therefore:—

Length more than 100 mm.: 22 specimens, equal to 30.6 per cent., weighing all more than 4 ounces.

Length less than 100 mm.: 53 specimens, equal to 69.4 per cent., almost all weighing under 4 ounces.

We can, therefore, say, with a great amount of accuracy, that in round figures half of all the tero-watta weighed from 2 to 8 ounces, and, with very few exceptions, remained under 100 mm. (4-inch) in length. One-quarter weighed more than 8 ounces and exceeded 100 mm. in length, while those that weighed less than 2 ounces never exceeded 75 mm. in length.

The above figures have conclusively demonstrated that the average tero-watta is a light implement of small size. Of course, there are exceptions, but they are few, and do not materially alter this view. The inference we can, therefore, draw is that the tero-watta was not an implement meant for heavy work. It was fit for light work only, and its size confirms, therefore, the view that it was used for chiefly in the manufacture of the wooden spears and throwing-sticks. A few other light manipulations, such as cutting the hair, the production of ornamental scars, scraping the red ochre, could be performed with it, and occasionally it was used as a knife to cut up animals. Heavier work, for instance, the splitting of fern trees, the cutting of notches into the bark of trees to be ascended, was probably done with columnar pieces of diabase, though it is probable that the heavier tero-watta may have also come in use for this kind of work.

Another very probable inference is that the hand which wielded the tero-watta was small, and that, therefore, the body to which this hand belonged was not of gigantic proportions.

8. SUMMARY AND CONCLUSIONS.

The above observations and facts can be summarised as follows:—

1. The raw material required for the manufacture of tero-watta was for the greater part obtained in the shape of water-worn pebbles from the gravel beds, for the smaller part from so-called quarries.

2. The raw material used in the manufacture of tero-

watta consisted exclusively of siliceous rocks of homogeneous nature possessing a good conchoidal fracture.

3. The parent block was broken by means of a hammer-stone; there is no definite evidence to show that the parent block rested on a hard support (anvil-stone) while being broken, but it is practically certain that the flakes were held in the hand when being trimmed.

4. The hammer-stones consisted chiefly of spherical or globular diabase boulders or pebbles, weighing from 1lb. to 2lb. in the average, though lighter, as well as heavier, ones, may have been occasionally used. The view that sharp-edged hammers of hornstone were employed, though not impossible, is not supported by actual evidence.

5. If anvil-stones were used—a theory which is more than doubtful—it is not probable that flakes of the same material from which the tero-watta were manufactured were employed. The only objects that could have served as anvil-stones are some of the indented stones described as "magic-stones," but the arguments against this view are so weighty that stronger evidence would be required before it could be accepted. In fact, all the evidence rather goes to disprove the use of anvil-stones than to prove it.

6. The blow of the hammer divides the parent block into nucleus (core) and flake (flakes, spalls).

7. Besides the true nuclei, i.e., pieces of stone which were left behind after the flake (flakes) had been detached, there are pseudo-nuclei, that is to say, boulders which were merely tested as to their quality, and rejected as unsuitable.

8. The marks of percussion on the pseudo-nucleus—a rather deep hole and intense shattering of the matrix—make it appear as if a sharply-pointed hammer had been used. This is not very likely, and the peculiarity of the marks is in all probability due to the physical constitution of the rock.

9. The flakes can be divided into external and internal flakes, and each group is again divided into two subgroups.

10. In the external flakes the original crust of the parent block (or part thereof) forms the Indical-face.

11. In the internal flakes one or more previous planes of fracture form the Indical face.

12. External and internal flakes of the first order have no special percussion face; the latter is formed by the original surface of the parent block.

13. External and internal flakes of the second order have a special percussion face (a former plane of fracture) which, though sometimes greatly reduced by marginal trimming, forms an angle of about 135deg. with the Pollical face.

14. The production of a flat, smooth Pollical face was the essential feature in striking off a flake from the parent block. This condition could only be fulfilled if the hammer struck the parent block at an angle of about 45deg.

15. The future shape of the tero-watta was primarily determined by the shape of the orginal flake.

16. A flake detached from a parent block may have been used without further trimming or not. If it was the Indical face only was worked, but never the Pollical face. (N.B.—There are certain exceptions of this rule, mostly in such instances when in the case of an internal flake there was little difference between Indical and Pollical face.)

17. Invariably the trimming of the Indical face or the edges was done by blows from the Pollical towards the Indical face, and never vice versa. (N.B.—There are certain exceptions, but they do not materially affect this rule.)

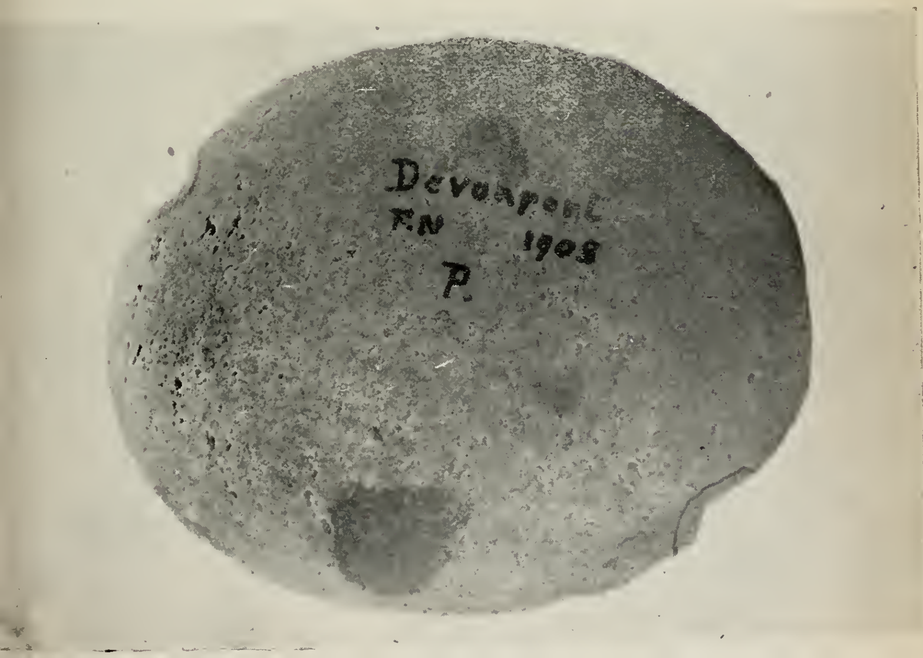
18. The trimming of the Indical face or the edges was in all probability done by means of a spherical hammer. In marginal trimming the blows were not set close, but at regular intervals, the saw-like edge thus resulting was subsequently straightened by striking of the "teeth."

19. In round figures 75 per cent. of the finished tero-watta weighed under 8 ounces, while only 25 per cent. weighed more than 8 ounces. The largest number, 32 per cent., weighed between 2 and 4 ounces, while only 14 per cent., a good number of which are perhaps unfinished rejects, weighed more than 1lb.

NUCLEUS.

TESTED REJECT.

TESTED REJECT.



HAMMER STONE (?)

20. There is reason to believe that there was a desire to produce a flake of a certain weight and size, irrespective of shape, when a parent block was broken, no matter how suitable the other flakes that fell off may have been, but the Indical face, particularly of external flakes, may have been subsequently trimmed. The inability to trim the Indical face probably accounts for the large number of unfinished rejects.

21. The accidental marks of percussion resulting when the flake was struck off the parent block appear on the Pollical face only, and their negatives can be seen on the core. (Any marks of percussion appearing on the Indical face are either the negatives of an earlier flake, or due to subsequent trimming.) These marks are: cone of percussion, scar of percussion, radiating fissure of percusssion, concentric wrinkles of percussion. The three first appear at the proximal end, while the last may spread over the whole surface. The process of percussion appears at the edge between Percussion and Pollical face, and marks the point where the blow fell.

22. If a wrinkle of percussion coincides with the edge of a flake, the edge is rounded instead of sharp, and this gave probably rise to marginal sharpening by striking off small flakes along the edge.

23. Though the essential character of the tero-watta is its unsymmetry in two directions, there is good reason to believe that certain specimens show an intentional outline, produced by marginal trimming.

24. There is no evidence to show that the tero-watta were manufactured in advance of their use; in all probability they were only manufactured when required, and immediately discarded afterwards.

25. It appears that sometimes attempts were made to rechip a previously discarded tero-watta, but there is no evidence to show that these attempts were completed.

26. The tero-watta was a universal instrument, adapted for all purposes alike, but never used as a weapon.

27. The reasons given under 24 and 26 explain the enormous frequency of the tero-watta.

6. NOTES ON TREUBIA INSIGNIS, GOEBEL.

By L. Rodway, Government Botanist.

(Read July 10, 1911.)

This hepatic was discovered by Goebel in Java, and described by him as recently as 1891. It was subsequently recorded from Tahiti, Samoa, and New Zealand, and now I have gathered it in dense woods on the southern slopes of Mt. Wellington, near the end of Strickland Avenue, and also near Forked Creek. Specimens have been forwarded to Stephani, who confirms the identification.

The plant is bright green when fresh, grows flat on the ground, and bears few lateral branches; it is about one centimetre diameter, and about five centimetres long, but it has been recorded from Java of a length of sixteen centimetres. The stem is broad and flat, and bordered on each side by oblong, leafy expansions which, at least in the anterior portions, are arranged in a succubous manner, that is, the anterior margin of each leaf is depressed and overlapped by the posterior margin of the one in front of it. On the dorsal surfaces there are two rows of suberect, transverse, green bracts, one near the anterior margin of each leaf. These bracts in the Tasmanian form are subquadrate, and about 2 mm. long; in the Javan specimens they are shorter. In the species Hepaticarum, Stephani refers the New Zealand form to a distinct species, T. bracteata, principally on account of the bracts being subquadrate, longer than broad, and appressed. On the under surface the plant bears a quantity of thick pellucid mucilage that affords protection to the growing apex, provides moisture in dry periods, and assists in anchoring the plant. This mucilage is secreted by glandular tissue formed on the lower portion of the anterior margins of the leaves. Treubia is generally classed with Aneura, Metzgeria, and Symphyogyna, and like them it has a complete absence of perianth; the work of that organ is undertaken by the enlarged fleshy calyptra. The archegonia are formed under the bracts. After fertilisation the calyptra enlarges enormously, becomes clavate, erect, and about one centimetre long. The seta is long, often as much as 5 c.m.; the capsule is spherical, and bursts to the base into four valves.

Treubia is of great interest to the Bryologist, for it combines reproductive and fruiting characters of Aneuraceæ with the leafy morphology of the Acrogyneæ. Many authorities try to avoid the breaking down of established systems by treating the lateral expansions as lobed portions of lateral wings. This seems a distorted description of the apparent structure, and does not tend to a clear understanding of the evolution of the hepatics. The leaves of this group of plants have without doubt arisen independently along many lines of descent, and also have arisen by the transformation of very different primary structures. In some instances they have arisen by the gradual modification of protective scales or from mucilage-secreting organs; or again as lateral expansions which have from the first, or subsequently, been segmented into the condition that we have generally called leaves. We must always remember that leaves of mosses and leaves of flowering plants are only alike in name and function. They can have no relationship one to another. They belong to different categories, and cannot truthfully be compared, except so far as their function.

If the term leaf is to be applied to definitely structured, lateral, assimilatory organs of Hepaticæ, then Treubia is leafy and not merely frondose. It is only a difference of words, with the addition of some recognition of evolutionary developments.

7. NOTES ON THE HUNTING STICKS (LUGHRANA), SPEARS (PERENNA), AND BASKETS (TUGHBRANA) OF THE TASMANIAN ABORIGINES.

Pl. IX., X., XI., XII., XIII., XIV., XV.

By Fritz Noetling, M.A., Ph.D., Etc.

(Read July 10th, 1911.)

INTRODUCTION.

In the papers previously published in the Society's journal I have conclusively proved, and it can now be considered as an established fact, that the stone relics of the Aborigines represent implements only, and not weapons. This is a fact of the greatest importance, and its significance will only be fully realised when we apply it to the study of archæolithic man in Europe. The Tasmanian Aborigines had made at least one great invention, viz., they had discovered that a certain kind of rock yielded sharp-edged flakes when broken. (1). They also found that these sharp-edged flakes could be used for most of the requirements of their simple life. But here again we come upon one of those curious psychological problems that are so difficult to explain. The Aborigines had undoubtedly discovered that these flakes were excellent cutting implements, as they have generally a fine edge, and often enough terminated in a sharp point. To us it seems easy enough to turn the good qualities of the sharp flakes to other uses than merely as tools. The instinct of self-preservation is paramount in all human beings, and, as has often been stated, it is the mother of all those inventions that have changed the life of our prehistoric ancestors into that of modern mankind. A modern mind cannot understand how it was possible that such a suitable material as the siliceous rocks from which the implements were manufactured, was not also used for weapons.

(1) This seems very insignificant to us, yet it was a great invention, when we consider that probably previously to the use of sharply-edged, artificially detached flakes, only thin columnar pieces of diabase or similar volcanic rocks which had a naturally sharp edge were used as implements by human beings.

To us it seems unintelligible, why the Aborigines did not fix a suitable flake to a piece of wood, thus producing a weapon far superior to the primitive wooden spear. Yet this was apparently an invention the Tasmanian Aborigine never made. His mind was just as unable to conceive the idea of providing the wooden spear with a stone head, as it was to chip the tero-na-watta on both faces, or to provide it with a handle, or to improve it by polishing the surface. It is a common theory that primitive man used as his earliest weapon a stick picked up by him during his wanderings through the primæval forest. The anthropoid apes are said to use a stick in self-defence. Now, there is no doubt that such a stick is an efficient weapon only at close quarters, unless, indeed, it is thrown at the aggressor. A modern man armed with a stout stick would, if suddenly surprised, await his enemy and attempt to disable him by a hard blow. Primæval man probably acted differently under similar circumstances; he threw the stick at his aggressor, and run away as quickly as he could. Speed of foot was still one of his chief means of defence. It is more than probable to assume that the primitive stick at first was simply hurled at the aggressor, and it is also more than probable that a methodical linear discharge of such a stick was a subsequent invention.

Now, if the Tasmanian Aborigines had neither weapons made entirely of stone, nor used stone as a supplementary material to give greater strength and efficiency to wooden weapons, what kind of weapons did they use? Fortunately, we are well informed on this point; in fact, the information is more complete than on many other features of their daily life, yet the records are again silent on some important points, as we shall presently see.

Ling Roth (2) has carefully collected all the information available, and the observations made by many explorers. These accounts, though somewhat differing in detail, agree in this that the Tasmanians possessed two kind of weapons: a short stick and a much longer spear. Both weapons were made solely of wood, and they were never provided with stone heads. Now it must be of the greatest interest to the student of Archæolithic civilisation, to know whether the accounts, as handed over to us, can be corroborated by the examination of actual specimens. Fortunately, the Hobart Museum has among its greatest treasures 7 authenticated spears and 3 short sticks. As these weapons have never

(2) Aborigines of Tasmania, 2nd edit., pages 67-72.

been properly described, and, as to the best of my knowledge, there is no pictorial reproduction of either sticks or spears, I thought that in the interest of science this information should be made available to students of archæology.

I desire herewith to acknowledge my obligation to the Trustees of the Tasmanian Museum for their courteous permission to examine and describe these valuable relics.

I. THE LUGHRANA (HUNTING STICK).

According to Milligan (3) the Tasmanian words for this implement, which he calls "waddie," a truncheon-like weapon used as a missile in war and hunting," were :—

(1) Lerga or lughrana (tribes from Oyster Bay to Pittwater).

(2) Lughrana (tribes about Mount Royal, Bruni Island, Recherche Bay, and the South of Tasmania).

The Norman Vocabulary (4) gives the name as
(3) Lillar,

while Dooe calls it lerga, and Roberts
(4) Lorinna.

Jorgensen states that the Northern Tribes call it
(5) Rocah,

while others call it
(6) Runna.

This is quite a number of names for such a simple implement, but we are able to reduce them to a smaller compass. "Lerga" and "lughra-na" are obviously the same word, and it is probable that "lillar" as well as "lorinna" were the names in certain dialects. We would therefore have lerga—lughrana—lillar—lorinna—a waddie, truncheon-like weapon used as a missile in war and hunting. The word "rocah," to which "runna" (Jorgensen) is apparently closely related, is, however, quite different from the above. We will presently see that Dove gives the word "rugga" for spear, and Jorgensen calls the same weapon "raccah." As all others who collected words of the Tasmanian language agree that the "spear" and the "waddie" were distinguished

(3) Vocabulary of the dialects of some of the aboriginal tribes of Tasmania Pap. and Proceed. Roy. Soc. of Tasmania, Vol. III., Part II., 1859, page 239.

(4) The Norman Manuscript. Pap. and Proceed. Roy. Soc. of Tasmania, 1910, page 340 (page 29 of the manuscript).

by two different names, it is more than probable that Jorgensen must be wrong if he calls the "waddie" "rocah" and the spear "raccah." In fact, if it were not for the testimony of Dove, who also uses the word "rugga" to designate a spear, I should feel inclined to think that this word is an error altogether.

As it is, I do not think that it means a spear—all the vocabularies agree as to the chief word for spear as we will presently see—it may be possible that it means a special kind of a "waddie." The evidence of the specimens preserved in the Tasmanian Museum seems to support such a view, but it is not sufficient to decide on anything definite. On the whole, I am not inclined to think that, though the hunting sticks may have differed in the finish, they were not distinguished by different names. For the present, I therefore consider the words "rocah—runna—rugga—raccah" as doubtful (5).

The lughrana has been designated by the early settlers as "waddie" or "throwing stick." The word waddie or waddy is apparently of Australian origin, and most probably borrowed by the early settlers from the New South Wales Aborigines. I am unable to say anything definite as to its origin, except that it is a foreign word which does not convey a better meaning to the general mind than the word lughrana. I therefore prefer to discard it altogether.

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(5) Mr. Ritz, with whom I frequently discussed these questions has kindly supplied the following remarks:—

"According to my classification of the Tasmanian speech-sounds, we have in the names given two ideas represented, viz., 'motion' and 'sending forth,' or 'motion from.' We have also pena (Roth, p. xxxvi., sub. spear (wood), which contains the idea of 'aiming at' or 'motion towards.' Wina is phonologically identical with pena. Simple motion is expressed by the liquids: r, l, n, m. 'Motion from' is expressed by gutturals: k, g, ng. 'Motion to' is expressed by labials: p, b, w. We may then classify the names of the spears, etc., as follow:—

"Simple motion: Lilla, runna, lo-rinna; also, muna lina (Roth., p. Lxvi., Lix.).

"Motion from: Lerga, lugh-rana, rocah.

"Motion to: Penna.

"As the spear or the simple stick might be denoted by any of the above names, the divergencies in the vocabularies were probably due to the accidental circumstance that in each case the aboriginal gave the word that occurred to him first. This does not exclude his having the other names in his vocabulary as well as the one given to his questioner on a particular occasion. Therefore, I cannot see the cogency of 'Jorgensen must be wrong' (at foot of p. 4), and must regard the whole argument on this point as doubtful.

"It is evident that any of the words for 'spear' did duty for the designation of any other things possessing the qualities indicated by the sounds."

I am unable to say how far Mr. Ritz's theories are acceptable or not; to me they seem to be interesting enough, but I must decline all responsibility for the views expresed by Mr. Ritz.

Worse, however, is the designation, as "throwing stick." No doubt the lughrana was "thrown," that is to say, it was passively thrown as a missile, but to call it a "throwing" stick is altogether wrong. The "throwing stick" or "womerra" is an implement used to impart greater force or velocity to a spear which was thrown by means of it. It is therefore an accessory implement which was used actively, and not passively like the lughrana. The retention of the word "throwing stick" might therefore lead to very grave misunderstandings, because those who are not intimately acquainted with the habits of the Aborigines might be led to believe that the Tasmanians used the womera. As the lughrana was chiefly, though not exclusively, used in hunting animals and birds, I think the word "hunting stick" is much more appropriate; it is certainly not misleading (6).

As far as my knowledge goes, only three lughrana have been preserved, and these are in the Hobart Museum. It is possible that a few more are in Paris and perhaps in the British Museum, or in possession of private individuals, but if they exist, they have neither been described nor figured. Two of the Hobart specimens, No. 4268 and No. 4269, were originally in Milligan's possession, and we may take it as granted that they are authentical. The third specimen, without a number, is said to have been found in some swampy land while a trench was dug, but, unfortunately the exact locality where it was found is no longer known. As it differs in a material point from Milligan's specimens, this uncertainty is greatly to be regretted. The following table gives the measurements of the three specimens:—

	No. 4268 (No. 1)	No. 4269 (No. 2)	No Museum Number. No. 3)
Length	633 mm.	584 mm.	660 mm.
Thickness	25 mm.	22.5 mm.	22 mm.
Circumference .. .	88.9 mm.	76.2 mm.	69 mm.
Length of shorter point ..	42 mm.	36 mm.	—
Length of longer point ..	76 mm.	82 mm.	—
Weight	265 gram. (9 oz.)	195 gram. (6⅞ oz.)	120½ gram. (4¼ oz.)

(6) It has been suggested to me that the word "missile-stick" would be a very appropriate designation for the lughrana. No doubt the lughrana was chiefly a missile, but as we shall see later on, it was also used in a different way, not as a missile. As it was apparently chiefly used for hunting purposes, I think the designation "hunting stick" preferable to that of "missile stick."

The most prominent feature of the above measurements is the shortness of the lughrana in relation to its relatively heavy weight. Specimens No. 1 and No. 2 weigh in the average for every 100 mm. (4 inches) in length 37.6gr. (about 1¼oz.), while the perenna (spear) gives only 15 to 18 grammes (slightly over ½oz.) for the same length (7).

Specimens Nos. 1 and 2 are exactly alike, so the description of one serves for the other as well. In general appearance the lughrana is a short stick, pointed at both ends, and apparently made of the wood of a shrub commonly known as tea-tree (8). It is of almost uniform thickness throughout, and both ends taper, forming a blunt conical point. The appearance of the ends is, however, very different. One end is smooth, the other rough and notched. The smooth point tapers rather suddenly, so as to form a short conical point; No. 4268 still shows the marks of chipping, while in No. 4269 they have been carefully smoothened off.

The rough point is rather peculiar, and its appearance is almost exactly like a pine cone. It tapers more gradually than the other end, and forms a rather long point; all over its surface for a distance from 76 to 82 mm. from the end it is made rough by numerous short, little cuts made with a tero-na-watta. Small portions of the wood have thus been broken off, and there was unquestionably an attempt to place the cuts in a regular ring round the end. The whole surface is smoothened, but the knots and knot holes were just scraped over without entirely being effaced.

Both specimens balance in the middle.

No. 3 somewhat differs from the other two. It is slightly longer than either, though this may not be of great importance. But the greatest difference consists in the appearance of the ends, which are both smooth. One end terminates in a short, smooth conical point, while the other tapers very gently, and ends in a smooth point, having no greater thickness than 6 mm., and a length of 125 mm. The thickness, 22 mm., is fairly uniform almost throughout the length. The surface is smooth, but it has unquestionably been affected by weathering. As already stated, the locality where it was found is not known, but I remember that the late Mr. Morton told me that a specimen was found

(7) No. 3 is omitted for obvious reasons, but it may be remarked that the wood from which the lughrana and spears are manufactured is the same.

(8) Probably melaleuca.

while a trench was dug in a swamp. The specimen here described is unquestionably the lughrana referred to by Mr. Morton, as it has quite the appearance of wood that has been under water for a long time and then became exposed to the air, for the surface shows cracks, and along these cracks the wood is slightly raised. Besides these cracks, there are numerous marks and cuts made with a European knife; it almost looks as if the finder had tried to test the hardness or the quality of the wood.

We will now examine how far the various accounts and descriptions given of the lughrana agree with the actual observations made on the specimens under discussion. The length of the lughrana is stated to be 2 feet by Henderson (9), 2 feet 6 inches by Thirkell, 2 feet 3 inches by Bligh, and 2 feet 6 inches by Lyne. Only Norman gives the length much smaller, viz., 1 foot 6 inches; but I feel inclined to believe that he understates the length, because he gives the circumference as 1½ inch (38 mm.), a measurement which is undoubtedly too small. These measurements agree exceedingly well with the length of the specimens here described, and it may be taken as certain that the length of the lughrana probably never exceeded 2 feet 6 inches (760 mm.), though the average length was probably not more than 2 feet (608 mm.).

The thickness is given as 1 inch by Backhouse and 1¾ inch by Lyne; this also agrees well with the above measurements. It is therefore certain that the lughrana was a short implement, and rather heavy for its size. Backhouse speaks of it as a "short stick brought suddenly to a conical point at each end and at one end a little roughened to keep it from slipping out of the hand." The tapering at both ends is confirmed by Norman and West. Both Thirkell and West point out that one end is roughened or notched, but Norman, who is otherwise so explicit, does not mention this.

All these accounts agree very closely with the appearance of specimens Nos. 1 and 2, the only somewhat different description is given by Norman. Calder further states that it was held by the thinner end, but he does not say that one end was notched or rough. Now, I hardly doubt that Calder as well as Norman would have noticed the difference

(9) All these quotations are taken from Ling Roth, Aborigines of Tasmania, 2nd edition, 1899, pages 65-82, where, under the heading "War," numerous references are given. It would be useless to quote again the titles of the original books, as a full list of literary references has been given by Ling Roth.

in the appearance of the ends if one had been rough or notched, and we must therefore assume that both Norman and Calder examined hunting sticks that were similar to No. 3, that is to say, thinner at one end than the other, but not notched. This would indicate that there were really two kinds of hunting sticks in use, viz., one kind having both ends almost of the same thickness, with one of them notched, while the other was smooth; the second kind having one end much thinner than the other, and both ends smooth. It is impossible to say whether these two kinds were used simultaneously, or whether they were manufactured by different tribes. It is also impossible to say whether they were distinguished by different names or not; as already said I do not feel inclined to think that such a small and rather immaterial difference was sufficient to give rise to different names.

One of the most interesting observations as to the way the lughrana was thrown is that of Backhouse, who states that they threw it "with a rotatory motion." This is confirmed by Breton, who says: "It can be thrown with ease forty yards, and in its progress through the air goes horizontally, describing the same kind of circular motion that the boomerang does, with the like whirring noise."

It is, therefore, absolutely certain that the lughrana was primarily a missile, which was thrown horizontally, or almost horizontally, with a rotatory motion like a boomerang. This can only be done if it is gripped at one end, and not in the middle. The lughrana was therefore, when used as a missile, thrown quite differently from the way the spear was thrown, and its character appears, therefore, to be quite different from the latter weapon.

Unfortunately, the statements as to its use are scanty, and somewhat conflicting. If it was used as a missile, was it used in that capacity in war as well as in hunting, or was it solely used in hunting expeditions, in order to kill animals and birds at a distance?

The various accounts seem to agree well on these points. The encounters between Aborigines and Europeans were numerous, and murders of Europeans only too frequent, but there is not a single instance on record that during these conflicts the lughrana was used. The killing of the enemy was always effected by means of the spear. In fact, the account of the first encounter between Europeans and Aborigines on May 3rd, 1804, near Risdon, lays great stress on the fact that the Aborigines were "armed (sic!) with waddies only (short, thick hunting clubs), while they drove a herd of kangaroo before them." It is emphatically pointed out, that this was the surest sign that on that particular occasion they had no hostile intentions towards the Europeans, because they were not armed with spears. The whole regrettable incident is stated to have arisen from a misunderstanding or lack of knowledge on part of the Europeans, who did not know that the "short,thick hunting clubs" were only used in hunting, and not in warfare (10).

There are, however, accounts which seem to indicate that the lughrana was used for other purposes. Henderson states that it was used to despatch the wounded victim, and Melville says: "If any quarrel took place among the men of the same tribe, it was the waddy that decided their affairs of honour." According to Breton, "it is the custom for one to receive a blow on the cranium, and then to return the blow on that of his adversary." The last statement is confirmed by Norman, though, according to him, the women chiefly settled the quarrels in the manner above described.

All these accounts indicate that the lughrana served a twofold purpose, viz., at a distance as a missile, in order to kill animals and birds, and, at close quarters, as a kind of club in personal quarrels, and to "despatch the wounded victim," at least, according to one authority. Unfortunately, it is not stated whether the "wounded victim" was an animal or a human being. There is no doubt that smaller animals, like a kangaroo or a wombat, could be killed by a blow with the lughrana; but was a wounded human being killed in a similar way? The skull of a Tasmanian could apparently stand a good deal of hammering, and we may well ask, "was it really used in that way to despatch the victim," or was it, perhaps, used as a stabbing instrument? Calder states that the mutilation of the body, and particularly of the head always followed the killing of a victim, and "this was done either by dashing heavy stones on the corpse or beating it savagely with the waddie."

Though, therefore, the lughrana was primarily a missile for hunting purposes, it seems to have been often enough used as a kind of club in personal quarrels, or to batter the body of a wounded enemy. It is, however, very doubtful

(10) J. E. Calder, Some Accounts of the Wars, Extirpation, Habits, etc., of the Native Tribes of Tasmania, 1875, page 6.

whether it was used as a stabbing instrument, though it seems to be well fitted for such a purpose.

There may yet have been another use for the lughrana, though there are no accounts of it. It seems well fitted to dig up roots and fungi; in particular, fern roots and the truffle-like Melitta australis. According to Brough Smith the West Australian Aborigines use a similar, though somewhat longer, instrument, and it is therefore not altogether improbable that the lughrana was used for a similar purpose. It may even be possible that the smooth-ended lughrana was used for digging roots, while the rough-ended was used as a missile.

The lughrana can, therefore, not be considered as a weapon, strictly speaking; there is not the slightest evidence to show that it was used in inter-tribal fights or in war, but there is at least one very emphatic statement that it was solely used for hunting purposes. We must, therefore, exclude the lughrana from the list of weapons, and we have to consider it as a special implement, belonging to that class of which the Australian boomerang is the typical representative. The general idea that the Aborigines of Tasmania did not know the use of the boomerang has to be considerably modified. They did use a short stick, which was thrown like a boomerang, and the only difference between it and the lughrana is in the shape; the character of the two implements, viz., a wooden missile thrown with a rotatory motion at a distant object is exactly the same.

This fact opens a wide view, and it may, perhaps, explain the curious accounts that recur ever and ever again of European tribes having used the boomerang. The boomerang seems to be such a peculiar instrument, which, according to a general belief, was solely restricted to the Australian Aborigines, that it was thought that any other race using such an instrument must, of course, be related to the Australians. But we can now give quite a different explanation; the boomerang is by no means an instrument special to Australia; it is only the highly-specialised form of a primitive implement that was common to all human tribes. I have above pointed out that we are very fond of imagining that primitive man picked up a convenient stick to defend himself with, and it is generally assumed that this stick was used as a club in a hand to hand fight. If we, however, assume that this stick was hurled with a rotatory motion like the lughrana, at a distant object, we shall probably be nearer the mark.

It is very probable that at first any stick of short length, just as picked up on the ground, was suitable, later on the ends were pointed, and one end was notched to ensure a firmer grip. It was probably soon discovered—though apparently the Tasmanians never made the discovery, or, if they did, never turned it to a practical use—that curved sticks were more suitable to be thrown with a rotatory motion than straight ones. This curved stick was capable of many improvements, without losing its character as a missile, notably with regard to its thickness; instead of being round like the primitive instrument it was flattened, and the natural result was the boomerang (11), or instruments like it. It is, therefore, hardly astonishing to find boomerang-like instruments pictured by the ancient Egyptians, or the similarly-looking trombash made of iron, and used by the negrōes of Central Africa up to the present day. All these instruments represent nothing else but highly specialised forms of the primitive human implement, the lughrana or hunting stick. This view is certainly more plausible and probable than to assume that there is in Australia a race of men of Indo-European origin, and that the boomerang was one of the weapons introduced by this race into Australia (12)."

It is very interesting to note, that while the Central African negroes substituted iron for wood, thus producing a very effective weapon, the Australian natives have only quite lately learnt to use metal in the manufacture of the boomerang. A paragraph in a weekly paper published in Sydney, seems to indicate that the Clarence River tribe on the Orara (N.S.W.) use strips of tin-plate in the manufacture of boomerangs (13). Of course, this statement requires further confirmation, but, if true, it would mean another interesting stage in the evolution of man's primitive instrument.

(11) Brough Smyth (Aborigines of Victoria, vol. I., page 311, has conclusively shown that that type of the boomerang, the wonguin, which returns to the feet of the thrower, is "usually regarded as a plaything." though it is occasionally used in battle, and sometimes for killing birds and small animals, it is not so handy as the short stick named konnung, and on page 302 Brough Smyth says: "A weapon of very similar character was in use amongst the natives of Tasmania.

The barn-geet, the war-boomerang, used in battle does not come back to the thrower.

(12) Ferguson, on the antiquity of the kiliee, or boomerang Transact. Royal Irish Academy, 1838. (I quote from Brough Smyth), as I have been unable to obtain this paper in Hobart).

(13) "Yalgun." Seen the tinerang yet? I have—among the remnant of a Clarence River (N.S.W.) tribe on the Orara. Billy cadges the raw material, which consists of a strip of tin plate from the local canning works, and having twisted the goods into the required shape, he does the same old tricks with it as he does with its wooden brother, the boomerang.—"The Bulletin," Vol. 32, No. 1,628, April 27, 1911, page 14

II. THE PERENNA (SPEAR).

According to Milligan (14) the following words were used for the designation of the wooden spear:—

(1) Perenna (tribes from Oyster Bay to Pittwater).

(2) Pe-na (tribes about Mount Royal, Bruni Island, Recherche Bay, and the South of Tasmania).

(3) Pœna, pilhah (North-West and Western Tribes).

The Rev. Norman supplies three more words (15), viz.: —(4) Arlenar. (5) Peearner. (6) Pleeplar.

And according to Calder, Dove uses the word

(7) Rugga;

Jorgensen the word

(8) Raccah;

and Roberts the word

(9) Preena;

while Scott in Milligan's Vocabulary (1890) uses

(10) Preana.

This is again a large list of words for a weapon about which there cannot exist the slightest mistake, but, as usual, this list can be greatly reduced.

In the first instance, pe-na and pœna are identical, as well as perenna, preena, preana, and peearner. In fact, to me it seems that there is no difference between the two words of the first and the three words of the second group, and that the word for spear can be spelled in any of the above variations.

From these differ, however, the words pilhah (Mill.) and pleeplar (Norman); it may be probable that both words are identical, but even if that be so it would be difficult to explain the different spelling.

But worse still are the words arlenar (Norman) and rugga (Dove), or raccah (Jorg.). The last two words are identical, but as exactly the same words have been used by the same authors for designation of the hunting sticks, their meaning is, to say the least of it, very unreliable.

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(14) l.c. under spear (wood).

(15) l.c., page 335 (page 9 of the manuscript).

All accounts agree, and the records are corroborated by the evidence of the specimens preserved that only one kind of spear was used; in fact, that the Aborigines used no other weapon but the spear. It is therefore very improbable to assume that the words pilhah — pleeplar, and arlenar represent different kinds of spears, but what their exact meaning is I am unable to say, unless we accept the very improbable theory that, besides the spear, they used another weapon of which there is neither record nor specimen preserved.

The words rugga or raccah may apply to a different kind of hunting stick, of which, as we have seen, two forms are known, and I think they had better be excluded altogether. (See above.)

We have, therefore, the following words for the designation of spear:—

(1) Perenna — peearner — preana — preena —pe-na — poena, a wooden spear.

(2) Pilhah—pleeplar, correct meaning unknown.

(3) Arlenar, correct meaning unknown, a very doubtful word.

In speaking of the spears I use the word perenna, leaving it to others to settle the question which would be the correct way of spelling (16).

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(16) The following contains Mr. Ritz's opinion on these words:—
These words may be classified, according to my theory, thus:—

1. Pe-na, peearner equal to pienna (where the two vowels may indicate a curve corresponding with the motion of the vocal organs from one position to the other.)
2. Pe-ren-na, where the "ren" would indicate speed, cf., "run" (Eng.); preana or preena would be variants of perenna.
3. Pilhah equal to pe-illa, equal to the moving thing (illa) aimed at (pe) something. We had "illa" before: arlenar equal to illa-na. Pleeplar equal to pilla-pilla, a very effective missile.

I am disposed to think that the Tasmanians used all these words indiscriminately for "missile;" the phonology does not support a distinction between a simple stick and a fashioned lance.

I think Mr. Ritz is greatly mistaken if he assumes that all these words were indiscriminately used for "missile," and that there was no distinction between a simple stick and a fashioned lance. He has apparently entirely overlooked that in all probability the hunting stick had been in use for immemorial times before the invention of the spear was made. But even if this theory is not accepted, there is a fundamental difference between the hunting stick and the spear. The former was thrown with a rotatory motion, the latter in a straight line, spinning round its longitudinal axis. However primitive the language may be, I cannot consider for a moment the idea that the aborigines did not distinguish carefully between two instruments, used for distinctly different purposes, and thrown in quite a different manner, quite apart from the view that the hunting stick was probably the older instrument.

The number of spears preserved is far greater than those of the hunting sticks. The Tasmanian Museum has now seven spears (17), which were originally in the possession of Milligan, and perhaps half-a-dozen more are owned by different private persons. The character of all the specimens that came under my notice is so similar that the description or picture of one specimen is sufficient to illustrate the features of them all.

In the following table I give the measurements and weights of the seven spears in the Tasmanian Museum, examined in detail by me:—

	No. 1	No. 2.	No. 3.
	(No. 4265 H.M.)	(No. 4267 H.M.)	(No. 4266 H.M.)
Length.	4.457 metres	4.432 metres	4.420 metres
Greatest Thickness.	21.5 mm.	21.6 mm.	16.0 mm.
Smallest Thickness.	5.0 mm.	6.2 mm.	5.0 mm.
Distance of centre of Gravity from pointed extremity.	1499 mm.	1575 mm.	15.12 mm.
Weight.	773¼ grm.	914⅓ grm.	666¼ grm.

	No. 4.	No. 5.	No. 6.	No. 7.
	(No. 4264 H.M.)	(No. 4260 H.M.)	(No. 4262 H.M)	(No. 4259 H.M.
Length.	4.077 metres	3.531 metres	3.520 metres	2.984 metres
Greatest Thickness.	16.7 mm.	12.5 mm.	23.0 mm.	13.0 mm.
Smallest Thickness	5.3 mm.	3.0 mm.	3.0 mm.	3.0 mm.
Distance of centre of gravity from pointed extremity.	1486 mm.	1257 mm	1283 mm.	1067 mm.
Weight.	666¼ grm.	283½ grm.	737 grm.	255.15 grm.

The general appearance of all the specimens I examined is much the same; they represent simply a straight shoot of Melaleuca spec., which was freed of bark and lateral shoots, and ends in a sharp, smooth point at the thicker end. The finish of all is exactly the same, except that one may be a little more knottv than another. If we go into details, we observe that the perenna shows an extraordin-

(17) The register implies that there were originally 10 spears, but three have mysteriously disappeared.

ary length; none of the above seven specimens is under 3 metres in length. This extraordinary length will only be fully realised if a perenna is held by a man of average height.

The above measurements agree very well with the statements made by most of the former observers, but Melville mentions that they were varying in length from 5 to 8 feet, while Henderson says that they were commonly 6 feet in length. I cannot help thinking that both these statements are not quite correct, because the majority of observers agree that the spears were at least 10 feet (3 metres) in length. The longest I examined has a length of 4.457 metres (14 feet 7½ inches), but according to La Billardiere they reached a length from 16 to 18 feet (5 to 6 metr. app.).

However that may be, we may safely assume that on the average the perenna had a length of 4 metres—13 feet (the average of the above seven spears is 3.917 metres), and though occasionally smaller or larger specimens were used, the minimum length did not go below 3 metres (10 feet).

The next remarkable feature is the small thickness; the thickest (No. 6) does not measure more than 23 mm. (0.9 inch) at its thickest part, while the thinnest (No. 5) is only half of this thickness. The thickest part is always just behind the point, and from there the perenna tapers almost immeasurably to the opposite end, which apparently does not exceed 6 mm. (¼ inch) in thickness, but comes down as low as 3 mm. ($\frac{1}{8}$ inch) (18). Widowson says that the spears were "as thick as the little finger of a man," but other observers, except Mrs. Prinsep, took very little notice of this feature. Yet it is an important one; the extreme thinness of the hinder end, in conjunction with the peculiar position of the centre of gravity, precludes the use of a woomera or throwing stick. Even if it were possible to grip the thin hind end in the hook of the woomera, the heavy pointed end would hang down to such an extent that it would be practically impossible to throw the spear. Hand in hand with the great thinness goes lightness; the heaviest (No. 2) weighs only 914 1-3 grammes (2lb. ¼oz.), and the lightest (No. 5) weighs only 283⅓ grammes (10oz.), the average being 613 grammes (1⅓lb. a.d. app.). Of course, it might have been anticipated that being no thicker than the little finger of a man, the spears were light, notwithstanding their great length, but nobody has apparently noticed

(18) I may mention that the ends of every one of the specimens examined were broken off, and they may, therefore, have been somewhat longer and also thinner at the end.

this fact. If we calculate the weight for a given unit of length, say, 100 mm. (4 inches), we find that it weighs:—

No. 1—17.343 gramme		Average 17.891 gr.
No. 2—20.622	,,	
No. 3—15.068	,,	
No. 4 - 16.335	,,	
No. 6—20.087	,,	
No. 5—8.0147	,,	Average 8.2801 gr.
No. 7—8.5456	,,	

Grand average, 15.145 grammes for every 100 mm. of length, which is therefore less than half the weight of the lughrana for the same length.

Another peculiar feature is the position of the centre of gravity; whatever the length or weight of the perenna may be, it balances slightly more than $\frac{1}{4}$ of its length from the pointed end. In other words, that the perenna was grasped with the hand in such a way that $\frac{1}{4}$ of its length was in front and $\frac{3}{4}$ behind it.

The accurate figures as to the position of the centre of gravity from the point expressed in a fraction of the length would be:—

No. 1 = 0.29733
No. 2 = 0.29330
No. 3 = 0.29233
No. 4 = 0.27436
No. 5 = 0.28113
No. 6 = 0.27435
No. 7 = 0.27966

These figures seem to indicate another interesting feature, namely, that the position of the centre of gravity shifted somewhat with the length; in the shorter perenna it was only slightly forward of the ratio 0.25000, while in the longer perenna it nearly approached the ratio 0.30000. This really means that the longer spears were grasped somewhat farther back from the point than the shorter ones. In round figures about 9/12 of the total length were behind the hand in the shorter and 8/12 in the longer perenna. The perenna is invariably pointed at the thicker end, and the greatest care was taken to produce a smooth, sharp point. Very little is known as to how the perenna were made, but the examination of the specimens, together with other observations, enable us to form an approximate conjecture.

There grows in the Tasmanian bush a kind of shrub popularly known as tea-tree (Melàleuca). This shrub grows up in long, straight shoots, and the wood is, when dry, of considerable hardness. These shoots were used in the manufacture of the perenna. It is not quite certain whether the shoots were pulled up with the root, or whether they were cut off in situ. In either case, the root end was cut off by means of a tero-watta. According to Lyne, the green wood was held over or passed through the fire "to soften and supple it." The bark was removed by means of a tero-na-watta, and the same instrument was used to smoothen the knots and knot holes. One of the specimens (No. 4265) shows the traces of the work of smoothening a spear in a particularly fine way, and I have taken a photograph of a portion of it. This shows that by means of such a primitive, clumsy instrument as the tero-na-watta, long regular splinters could be sliced off; the knot holes were smoothed by cutting off short chips. We must assume that the point was produced by slicing off long, narrow splinters, gradually bringing the thicker end to a tapering point. Scott states that the end of the perenna was hardened by being a short time in the fire, a statement which is corroborated by Lyne and Raynor. The latter is particularly explicit in stating that they pulled up the young shoots, burnt off the roots (19), and placed the thick end on the fire again till it was slightly burnt; then they would rub off the burnt part with a rough sandstone, and repeat the operation till they got a sharp point. If this account is correct in every point, it would appear that the tero-watta never came into use in the manufacture of the point except as a scraper (20), in order to scrape off the charred portion of the wood, and to smoothen it. Considering that three different observers, who are generally very reliable, and to one of whom we are indebted for some of the most important information, have stated that fire was used in the production of the point, we must assume that it really was so. On the other hand, though I very carefully examined the points of the seven spears with a powerful magnifying lens, I could not discover even a minute trace of charcoal. It must, however, be admitted that, though the marks of the tero-watta are very clear and distinct on the hinder portion of the perenna, none are visible on the point, which, as will be seen from the illustration, is as smooth as possible.

Inasmuch as the tero-watta was unquestionably used to shape the back portion of the perenna, I question to

(19) Be it noted that Raynor says "burnt off," and not "cut off" the roots. If this was the regular practice, the tero-watta would not have come into use as a chopper to cut off the root end.

(20) Of course, the word "sandstone" used by Raynor is not correct; it ought to read "flint," or tero-watta. If sandstone had really been used to smoothen the point, specimens of it would have been found on the old camp sites. The camping grounds are, however, singularly free of pieces of sandstone, and I never found even a small piece indicating that it was used for polishing.

some extent Raynor's statement. To me it seems that the point was rough-hewn by means of a tero-watta exactly like the point of the lughrana, as conclusively proved by specimen No. 1; after the rough work was done the point was held in the fire, and the slightly-charred surface carefully scraped off by means of a tero-na-watta, and eventually rubbed with grease to make it quite smooth (21).

I cannot quite understand what Backhouse means by stating that in straightening their spears the natives used their teeth as a vice to hold them. The shoots of melaleuca or leptospermum are very straight, and do not require straightening, but owing to the extreme length and the peculiar distribution of the weight, a perenna will assume a somewhat curved line if kept in a horizontal position, and this feature probably explains why, according to W. B. Walker, "at their places of rendezvous" the spears were "carefully tied to straight trees, with their points at some distance from the ground."

All eye-witnesses agree that the perenna could be thrown to a considerable distance; according to Mrs. Prinsep it could be thrown to the distance of 60 yards, while Lloyd says that 40 yards was the extreme range; Breton estimates the range to be from 40 to 50 yards: Calder gives 60 to 70 yards. All accounts further agrce that this primitive weapon could inflict severe wounds; Meredith, in describing the murder of one of his father's stockmen, states that a spear had been driven through the thick boot-sole into the foot of the murdered man; another had penetrated his loins several inches. According to West, a man named Franks was, while riding, attacked by Aborigines, "and within 30 yards a savage stood with his spear quivering in the air. This weapon, ten feet long, penetrated the flap of the saddle and the flesh of the horse four inches." According to Kelly, when the Aborigines attacked his party near Cape Grim, "one spear went through the side of the boat."

All these accounts prove one fact conclusively, viz., that the perenna was thrown with great force, and this is the more astonishing if we consider that no woomera was

(21) It must be particularly mentioned that the statements that the spears had jagged points, or that they were pointed at both ends, or even that the points were poisoned, are entirely unfounded. There is not a single specimen known which shows a jagged point, and the statement that they were pointed at two ends is probably due to the mistake of thinking that the naturally thin end of the fusiform spear was artificially made thin or pointed. Melville's statement of a fatally poisoned barbed spear is unquestionably erroneous, as quite out of harmony with the general customs of the aborigines.

used. Now, how were the perenna thrown? It is obvious that they were thrown differently from the lughrana. The latter was, as we know, thrown with a rotatory motion, like a boomerang, but it is obvious that the perenna could not possibly have been thrown in such a manner. The perenna must have been and was thrown in a straight line, but the force that sent it to a distance of 40, 50, even 60 yards, and made this crude weapon penetrate through thick leather must have been considerable.

Now, how was the perenna grasped, in order to make it such an effective weapon? The ordinary modern man would grasp it in his fist, as shown in Pl. xi., fig. 1, but it is very doubtful whether this way of grasping could supply it with such a great energy on being thrown. In fact, in dealing with the manner in which tools and weapons were grasped by archæolithic human beings, I have become rather suspicious of the way the hand of the modern man involuntarily grasps these same implements. I have come to the conclusion, that it is almost certain that archæolithic man did things and held instruments in quite a different way from that which a modern man would do or hold them.

Now, a most remarkable passage in Mrs. Prinsep's letters gives apparently the key to the problem. This passage runs as follows:—"They threw the spear for our amusement. This is merely a slender stick, nine or ten feet long, sharpened at the heaviest end; they poise it for a few seconds in the hand, till it almost spins, by which means the spear flies with great velocity to the distance of 60 yards, and with unerring aim."

They poise the spear in the hand till it almost spins! Now, how can we interpret this peculiar remark; if the spear was gripped by the closed fist it certainly could not spin. Therefore, we must assume that it was not held or grasped with the closed fist, with which I or any other modern man would grasp the pilum.

We may further take it that the words "till it almost spins" mean that it rotated round its own longitudinal axis, and not in a circle. Now, such a motion can be produced if the spear were held, as shown in Pl. xi., fig. 2. The front part of the spear rests on the middle finger, the hinder portion on the base of the first finger, which grasps the spear on its upper side. The thumb presses well against the lower side, and the moment it is thrown the thumb, by a quick upwards movement, imparts to it a rotating motion, exactly the same a bullet acquires by the rifling of the barrel, or as a "spin" is imparted to cricket ball by the peculiar action of the thumb and forefinger. This spinning motion probably enabled the perenna to travel to distances, which it would never have reached if thrown without it, and the long range which astonished everybody is thus easily explained by the peculiar way the perenna was held by the hand when thrown. Now, we also understand why the perenna shows such a small thickness. A perenna having the thickness of a lughrana could not well be held by three fingers, and the thumb could not impart to it the spinning motion it could to the thin perenna. The thinness was, therefore, the essential feature of the perenna; without it, it could not be thrown with a spinning motion, and without the latter it would never travel the distance it did, nor probably have the penetrating power.

I need hardly mention that the perenna was never provided with a stone head, and in this conjunction it must be mentioned that the so-called Tasmanian word, "poyeenta" or "poyeenna," which Milligan gives as designating the "point of spear," is most probably an adopted English word; the Tasmanian did apparently not distinguish between the different parts of a spear as we should do, and there was no reason to do so, because the perenna did not consist of head and shaft, but was made all in one piece.

Like the tero-watta, but unlike the lughrana, the perenna could not be improved upon or altered without losing its character. If it was made thicker it could no longer be thrown with a spinning motion, and, of course, there was a limit below which the thinness could not go. If ever it had been provided with a stone head, it would have been no longer a perenna, though it might still have been thrown with a spinning motion. As long as it remained as it was the woomera could never be used in conjunction with it, even if it had been invented by the Aborigines. Though there cannot be the slightest doubt that the pilum of the antique world evolved from the perenna of archæolithic mankind, this weapon had reached its highest perfection, and could not be improved upon without losing its essential characteristic features.

In conclusion, I may mention that the Aborigines were frequently in the habit of trailing the perenna along the ground, holding it between the toes, appearing to be unarmed, with the intention of deceiving the enemy. At a moment's notice the perenna was transferred to the hand, to be thrown at the enemy. Without doubt the perenna was well adapted for such a ruse, but it seems unlikely that it was habitually carried in this way, as this would greatly hinder the march through the bush.

III. THE TUGHBRANA (BASKETS).

Milligan gives the following words for basket:—

(1) Tughbranah (tribes from Oyster Bay to Pittwater).

(2) Trenah (tribes about Mount Royal, Bruni Island, Recherche Bay, and the South of Tasmania).

(3) Tille (North-West and Western Tribes).

And as usual the Norman vocabulary (22) gives four words, all different, viz.:—

(4) Tringherar.

(5) Poakalar.

(6) Meerar.

(7) Parnellar.

And as, if this list was not formidable enough, Calder mentions two more names, viz.:—

(8) Terri (D'Entrecasteaux).

(9) Tareena (Roberts).

Finally, Milligan, in the list of short sentences, translates the words: "The woman makes a basket" with "lowanna olle tubbrana," in which the last word stands for basket. Though "tughbranah" and "tubbrana" are apparently identical, as well as "trenah" and "tareena," to which might be added the word "terri," there remain seven different words to designate a basket. Even if one were to go as far as to assume that all the words beginning with a "t" were identical, and represented only different spellings or local dialects, there still remain four entirely different words.

It is impossible to say whether these words represent different kinds of baskets, or baskets used for different purposes, if they really apply to baskets. Norman, who is responsible for most of these words, does not even hint in his explanatory note that there were different kinds of baskets, or that those that were used for different purposes were dis-

(22) Pap. and Proceed. Roy. Soc. Tas., 1910, page 340 (page 25 of MS.).

tinguished by different names. In fact, his note seems to indicate that there was one kind of basket only, a view which is fully borne out by the specimens still preserved.

It is impossible for me to explain these words, and I must leave it to others better acquainted with the Tasmanian language than I am to explain them. In my opinion, the last three words of Norman (5, 6, 7) have probably nothing to do with baskets (23).

The Tasmanian Museum in Hobart possesses 10 baskets, the measurements of which are given below:—

	1	2	3	4	5
	No 1247	No. 4280	No. 4282	No. 1248	No. 4274
Diameter at top ..	20 cm.	15 cm.	16 cm.	15 cm.	18 cm.
Greatest Diameter	37 ,,	21 ,,	23 ,,	18 ,,	25 ,,
Height	35 ,,	26 ,,	23 ,,	21 ,,	25 ,,

	6	7	8	9	10
	No. 4276	No. 4279	No. 4277	No. 4278	No. 4281
Diameter at top ..	15 cm.	15 cm.	12 cm.	10 cm.	9 cm.
Greatest Diameter	19 ,,	20 ,,	17 ,,	16 ,,	12 ,,
Height	21 ,,	19½ .,	18 ,,	16 ,,	11½ ,,

(23) Again I am indebted to Mr. Ritz for an ingenious explanation of these words. Mr. Ritz says:—

My theory would explain the words as follows:—

1. Tughbrana, tubbrana equal to tuga, perina breone equal to eat fish.

The basket represented as swallowing the fish or oysters.

2. Trenah, tareena, terrl, tille, equal to terina, skeleton. The baskets were not solid, but open worked, for fishing. Those for carrying water (see Roth, p. 142) were, of course, nearly watertight; they were nitipa (Roth, p. xviii.) equal to ni-tapa equal to not dripping (tap, the noise of falling drops).

3. Tringherar is given by Norman also as meaning "to swim."

Poakalar, parnella—Norman gives to these words also the meaning of mussel.

Meerar is probably a form of peri-na or breo-ne fish.

In my "Speech of the Tasmanian Aborigines" (p. 33) I suggested that Norman was likely to mistake his own presumptions for the information given by aboriginals. Here we find several likely instances. He pointed to a fishing basket, and asked for its name. One would tell him it was used when the women went swimming (tringherar), another would say it was for mussels (poakalar, parnellar); and a third said it was for holding shell fish (mera-na, or perana). He quite seriously assumed that these words meant basket.

Incidentally we find a remarkable similarity to buckalow in poakalar, warkellar (p. 7 of Norman's MS.).

Mussel equal to round, swimming, or floating.

Again, the words are apparently not names of different things, but different names of one thing.

From the above measurements it will be seen that some of the baskets were of considerable size; the cubical contents of the largest (No. 1247) (24) being 26½ litre (26522 cub. cent.). The smallest (No. 4281) contains, on the other hand not more than 905 cub. cm., that is to say, less than one litre.

Though in general appearance remarkably alike, it almost seems as if two kinds were made, a spherical and a cylindrical kind. The largest (No. 1247) is a typical spherical basket, which is widest in the middle and narrower at the opening and the bottom. No. 4280 (see pl.) is of cylindical shape, maintaining its width throughout. Only two cylindrical baskets have come under examination, all the others are of the spherical type. This difference in shape may, however, only be accidental; at the same time it cannot be quite denied that the different kinds may have served different purposes, and this theory would explain the different names.

The plaiting is exactly the same in all the baskets, whether of spherical or cylindical shape; the only difference is that sometimes the meshes are smaller, sometimes larger, but the work is of the simplest kind.

A careful examination of the specimens has convinced me that they were made differently from the modern basket. The modern basket is commenced at the bottom; the Tasmanian basket was commenced at the top.

The basis of the tughbrana was a ring of twisted flat fibres of about 6 to 7 mm. thickness. The vertical strands were not twisted, but the flat fibres were nicely rolled. These were inserted into the basal ring, and kept in position by a thin twisted chord, which was firmly wound between the vertical strands round the basal ring. Each ring of the horizontal strands consists of two pieces of rolled grass, which were twisted round the vertical strands in a very regular way, which the figure illustrates very well.

The illustration also demonstrates how the vertical strands were joined, and how eventually the bottom was made.

The baskets were probably all made of a reed, juncus acutus, which grows in abundance in the swamps of

(24) This specimen, as well as No. 4, has been figured by Ling Roth, Aborig. Tasman. Plate to face page 153.

Tasmania, and which yields a very strong fibre. Bunce stated that they were made of the leaves of Anthoricum semibarbala, as well as Dianella. That may be so, but no specimen made of these plants has come under my examination; those in the Hobart Museum are all made of Juncus-fibre, as has already been noticed by Ling Roth (25).

The baskets are very strong, and even now, though years have passed since they were made, they are very elastic, instead of being brittle, as might be expected after this long time.

It is difficult to say how the baskets were carried; most of those that are in the Hobart Museum have a short string of twisted grass tied at two opposite points of the basal ring. This would indicate that they were carried by the hand and not on a long string across the shoulder. If they had been carried this way, the longer string would have again to be tied to the shorter string, an assumption which is not very probable.

We practically know nothing about the manufacture of the baskets, though several of the early explorers watched the operation. Bonwick says that he watched a woman making some string, and the chief point of his observation is, that the woman "began to twist the threads by rolling the material up and down her thigh." The strands of which the baskets are plaited look exactly as if they had been rolled in such a way.

The baskets were principally used to bring up shell fish collected at the bottom of the sea, and to carry the same afterwards to the camping grounds. It is very probable that chiefly the larger baskets were used for such a purpose, because the smaller ones hardly contained enough room for even a small quantity of oysters or haliotis. They were probably also used to collect the raw material (pebbles) for the manufacture of tero-na-wattas, or to carry to the camping grounds suitable specimens that were obtained at the quarries. To me it seems probable that the smaller ones were used to carry the tero-watta that were in use for the time being, as well as the material required to make fire.

In the 1st edition of the Aborigines of Tasmania, Ling Roth figures on Pl. I and Pl. II., two baskets said to be of

(25) Aborigines of Tasmania, 2nd ed., page 144.

Tasmanian origin, and now in the British Museum. These baskets were originally in the possession of G. A. Robinson, from whom Milligan obtained them. There cannot be the slightest doubt that these two baskets are not of Tasmanian workmanship. The plaiting is so different from the Tasmanian baskets, and discloses also a much higher style, that it would be most remarkable had the Aborigines practised simultaneously such different kinds of plaiting (26). Likewise, the woodcut, fig. 3, from a basket in the Museum of Oxford, is certainly not taken from a basket made by Tasmanian Aborigines, and Ling Roth's assumption "that a race who appear to have been lower in the scale of civilisation than many races whose industrial remains have lately become known to our times, should have known the stitches which form, in fact, the foundation of our modern point lace (27)" is unfounded. It is greatly to be regretted that the learned author of the Aborigines of Tasmania, who gives in the 2nd edition a wood cut of the pattern of basket work from Queensland, which is very similar to that of the Oxford basket, has not corrected his errors in the 2nd edition. Such statements as the above are very misleading, and are apt to throw quite a wrong light on the Tasmanian civilisation.

Ling Roth remarks that the plaiting of the Tasmanian baskets is similar to some fabric from the Lake Dwellings of Robenhausen and Wangen. I am unable to verify this statement; the only two illustrations of basket work from the Lake Dwellings I have at my disposal are two figures in Reinhard's "Der Mensch zur Eiszeit in Europa," which are apparently copies from Heierli, "Urgeschichte Der Schweiz." Both, figures 341 and 342, represent specimens of basket work from Wangen, but the pattern is unquestionably much superior to the Tasmanian one, and of quite a different workmanship (28). This might have been expected; the Lake Dwellers (Robenhausenian) had attained a much higher stage of civilisation than the Tasmanian Aborigines,

(26) Though Ling Roth had already expressed his gravest doubts as to the authenticity of these baskets (2nd ed., 1899, page 144), these more than doubtful specimens still seem to figure as Tasmanian baskets. In an article on the early history of Tasmania ("Tasmanian Mail," December 12, 1908, by Ida Lee, one of these selfsame baskets is figured as a "relic of the natives of Tasmania in the British Museum." It seems about time that the authorities of the British Museum removed these two questionable baskets, or at least marked them with a great query.

(27) Aborigines of Tasmania, 1st ed., page x.

(28) The pattern of plaiting given by Mortillet (musee prehistorique, Pl. LXVII., fig. 739, from Wangen) is exactly the same as that described by Reinhard.

and it would, therefore, be more than remarkable had the latter already reached such a high perfection in basket plaiting as to be equal to the Lake Dwellers.

I am unable to say whether the baskets found in the Lake Dwellings were manufactured like the Tasmanian ones, viz., commenced at the top. However that may be, I consider the Tasmanian baskets as the most primitive type of human basket work (29). The great probability that the tughbrana was commenced at the top, and not at the bottom, renders this kind of work absolutely different from any later work. It would be of the greatest interest to ascertain when the invention was made to plait the baskets in the modern way.

Though, outside the scope of this paper, I may mention that the Tasmanians possessed a kind of pitcher called moirunah, and made from sea-weed (Fucus palmata). The only specimens that are known are in the British Museum and in France (30). A wooden "spatula" was used to loosen the Haliotis from the rocks to which it firmly adhered There is no moirunah in the Hobart Museum, and as to the "spatula," I do not think that any specimen at all has been preserved. Neither can I find a name for this implement, and I do not think that it was more than a short stick, ending in a chisel-shaped edge.

One word about the so-called canoes, the mallana or nunganah. The accounts agree that they were nothing but bundles of reeds tied together, but the figures of models in the British Museum, and similar models in the Hobart Museum (31), are so suggestive of a real canoe having stem and stern, that I cannot help thinking that their original shape has been greatly improved upon by those who made the models. Those in the Pitt Rivers Museum seem to be more like the real mallana, and more in harmony with the state of the Tasmanian civilisation than the canoe-shaped models in the Hobart and British Museum.

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(29) According to Brough Smyth, Aborig. Vict., vol I., page 346, basket of exactly the same pattern as the Tasmanian ones, and similarly in shape, are still manufactured by the Queensland aborigines. The figures of baskets made by the aborigines of Victoria, page 343, 344, and 345, particularly fig. 159, make it more than probable that the so-called Tamanian baskets in the British Museum, and in the Oxford Museum, are really of Victorian origin.

(30) See Ling Roth, Aborigines of Tasmania, 2nd ed., page 142.

(31) Ling Roth, Aborig. Tas., plate to face page 153.

CONCLUSION.

Modern researches have shown that stone implements, which cannot be distinguished from the rougher tero-watta, have been found as far back as the Middle Oligocene (Fagnian). Unfortunately, there has lately arisen a discussion as to the authenticity of these specimens. Verworn (32) holds in opposition to Rutot that these specimens were made by natural agencies, and not by human beings. Not having seen the locality where the specimens were found, I cannot speak with the same authority as Verworn, who advances some seemingly strong arguments in favour of his theory. All I can say is, that I cannot distinguish the Archæolithes from the Fagnian, which Dr. Rutot kindly sent me, from the Tasmanian tero-watta, and unless absolute proof is forthcoming that natural agencies can produce tero-watta-like specimens, I maintain with Rutot the artificial origin of the Fagnian specimens.

However, to be quite on the safe side, I will begin with those specimens whose nature as human handiwork nobody now doubts: the Archæolithes from the Upper Miocene (Cantalian). As these implements are exactly like the tero-watta, we may fairly assume that they were used for the same purposes as the former. The chief purpose for which the tero-watta was used was unquestionably the manufacture of the lughrana (hunting stick) and perenna (spear). All other purposes were subordinate to this one. We may therefore conclude that the Archæolithes from the Cantalian were used for a similar purpose, and, what is more, as, during the Upper Miocene, a mild if not warm climate must have prevailed in Europe, the necessity of warm clothing did not exist. The race that hunted the Hipparion and manufactured the Cantal Archæolithes was probably quite as naked as the Tasmanian Aborigines. There was therefore no necessity for the use of a scraper in order to prepare skins for clothing.

Now, a difficult question arises; we know that the Aborigines used, together with the true weapon, the perenna, an implement which cannot quite be considered as a weapon, namely, the lughrana (hunting stick). It may have been used as a weapon, and a true weapon has eventually evolved from it, but the lughrana was, in the first instance, made and used for hunting purposes only. It is further very probable that the lughrana is the nearest an-

(32) Korrespondenzblatt, Deutsch. Gesell. f. Anthrop. Ethnol. and Urgesch. XLI. Jahrg. No. 5 and 6, 1910.

proach to the stick, which primitive man hurled alike at human enemies and animals required for food.

Now, did the human beings who made the Cantal Archæolithes already manufacture spears of the perenna type, or had they not made that invention yet, and solely used their stone implements in the manufacture of hunting sticks (lughrana)? The question is an intensely interesting one, as the lughrana is the primary implement, the perenna the later invention Now, when was the invention of the perenna made? If, as Dr. Rutot and I hold, the Archæolithes from the Middle Oligocene were made by human beings, it is very probable that these human beings used them for the manufacture of the hunting sticks only, and it is, perhaps, possible that the Cantalians had not advanced further.

If this theory be correct, the invention of the perenna (spear), i.e., the weapon which was thrown with a spinning motion in a straight line at a distant enemy, must have been made some time between the 1st Glacial Period (Guenzian), representing the Kentian industry and the beginning of the Middle Interglacial Period, representing the Strepyian industry. The Chellean industry at the end of the Middle Interglacial period had already learnt to provide the spear with stone heads, and had therefore, in all probability, discarded the wooden spear (33).

If we knew for certain which of the Archæolithic industries, from the Fagnian to the Mesvinian, used the hunting stick only, and which used the wooden spear besides it, a great stride in our knowledge of the development of the human race would have been made. To judge from the Archæolithes from the Mesvinian, Maffelian, and Reutelian, which my friend Dr. Rutot sent me, I have no doubt that the representatives of these industries already used the wooden spear. If that be so, the invention of the wooden spear as a weapon would have been made either in the 1st Glacial Period (Guenzian) or in the 1st Interglacial Period, both of which are now considered as Pliocene, forming the end of the Tertiary Period in Europe (34).

According to this theory, the human beings of the warmer Tertiary epoch, i.e., the Oligocene and Miocene

(33) This may have already commenced during the Strepyian.

(34) It is quite possible that a lucky find may solve this question; if human bones have been preserved it is to be hoped that some day the remain of a lughrana-like instrument or of a perenna-like spear may be found.

used for an enormous period, that can only be counted by millions of years, nothing else but the hunting stick, which was thrown with a rotatory motion, and which, as I said, cannot be considered as a weapon, strictly speaking. When the first ice sheets covered Northern Europe, perhaps the first invention of a real weapon, the wooden spear, which was thrown in a straight line, probably spinning round its own axis, was made. Wooden spear and hunting stick were again the only weapons of the human race for an enormous period, though, if measured in absolute time, the earlier part of the Archæolithic stage, which was characterised by the use of the hunting stick only, was incommensurably longer than the latter part in which hunting stick and wooden spear were simultaneously used.

The period when the wooden perenna was superseded by the stone-headed spear can be very accurately fixed; this must have taken place about the time when the Palæolithic implement took the place of the Archæolithe, namely, during the Middle Interglacial Period in the Strepyian industry. It is very probable that at first spear heads of an Archæolithic type were used, and specimens of this type are still used on the Admiralty Islands and in Queensland. It is, however, very probable that owing to its unequal balance the Archæolithic spear head was not long in favour, and was soon superseded by the Palæolithic head.

Probably at the same time as when the wooden spear was provided with a stone head, the shaft was made stronger, and it was no longer gripped like the perenna, but with the whole fist. The spinning motion of the perenna naturally became impossible, and the spear was thrown in a straight line, without rotating round its longitudinal axis. It would go beyond the scope of this paper if I were to follow up the evolution of weapons; it only seems to me that the period during which a certain type of weapons was in use quickly became shorter till it is now only as many months in use as it was formerly centuries, and at a still earlier period thousands, even hundreds of thousands of years. In connection with this we notice a peculiar feature; primitive man fought his battles at a long range (35), which, of course, was measured by yards only;

(35) The Tasmanian aborigines did not like a hand to hand fight; in preference they sent a shower of spears from an ambush at the unsuspecting enemy, but they did not come to close quarters except to dispatch the wounded when the enemy took to flight. For this reason I do not believe that primitive man used as its first weapon a club, that is to say, a weapon ending in a heavy knob. A club is essentially a weapon to be used in a close combat, but primitive man, like the Tasmanians, did not fight at close quarters, so the club was of no use to him.

modern man fights again his battles at a long range, with the difference, however, that the distance between the combatants is now almost as many thousands of yards as it used to be yards. Between the two stages falls the period of close combat; this period must have commenced with the invention of the dagger and the sword, the axe, and the club, the weapons suitable for a close fight. Perhaps this invention coincides with the Magdalenian, though I should feel inclined to date it somewhat later. Ever since, probably all through the Neolithic and Bronze age, human beings fought their battles hand to hand. All the great battles of the antique world were fought at close quarters, and so were those of early middle ages. Only with the inventions of gunpowder the combatants separated again, and the distance gradually increased, and is apparently still increasing. There will, however, be an end to this ever-increasing distance; at present the range of some of the big guns is such that it is impossible to discern a small or even large object at that distance. There must, therefore, be an end to this increase of the horizontal distance, and I think we are pretty near that end. What would be the use of a gun having a range of 30 or 40 miles if the object to be fired at is below the horizon, and cannot be seen? But what is going to happen then? Are we to expect that the pendulum swings back and the combatants again come to close quarters? I hardly think so, even if an invention were made that one man could annihilate a whole army at close quarters, the other side would take the greatest care that that one man would never come to close quarters. I almost think that as fighting in the horizontal plane has come to its practical limit, the next movement will be the shifting of the plane, and instead of in the horizontal plane the fighting will be carried out in the vertical plane, which very likely means coming to close or relatively close quarters again.

APPENDIX.

THE DUTERRAU ENGRAVINGS.

Mr. J. W. Beattie, who is so indefatigable in hunting up old records and other relics connected with the early history of Tasmania, has kindly drawn my attention to some quaint old engravings, which bear on the subjects discussed in the above paper.

These engravings were "designed, etched, and published by Bn. Duterrau" between July 15th, 1835, and March 23rd, 1836, in "Hobart Town, Van Diemen's Land."

As Bn. Duterrau has been careful enough to add even the day of the month when he published his engravings, we know that they were made after the Black War (1830), probably just before the Rev. George Augustus Robinson brought the last 203 survivors to Flinders Island. The first engraving published, July 15th, 1835, is entitled "Tasmanian Aborigines," and represents a group of ten Aborigines (7 men and 3 women) cordially receiving Robinson, who is wearing a quaint sort of a cap. The Aborigines are depicted as naked, except for a loin-cloth, which is unquestionably an invention of the artist, and a concession to public taste. All the women have the hair closely cropped, three of the men have the peculiar wig-like head dress, while four have it in apparently its natural curly state.

Four of the men are simply armed with spears, while two others who are squatting down are apparently making spears. It will be noticed that two of the men are holding their spears in the left, three of them in the right hand. Robinson is grasping a native's left hand with his left, while his right is held up in teaching or preaching position.

Now, I shall presently show that, in all probability, these engravings must be reversed, and we have therefore three men holding the spears in their left hand, two in the right, while Robinson's right grasps the right of the native, and his left is lifted. Unless we assume that Robinson was naturally left-handed, we must accept the view that the print of the engraving ought to be reversed.

Now, the second engraving published on August 24th represents exactly the same two figures in exactly the same attitudes, with that difference, however, that while in the above engraving they are separated by two women, Robinson and a man, and two dogs, they are in the second close together, each sitting, so to say, on a large bundle of spears, which are absent in the above engraving. This seems to indicate that the pictures were not taken directly from life, but were composed in the artist's home, from rough sketches made elsewhere. This may somewhat reduce the value of the engravings as evidence, because it is hardly necessary for me to say, that memory even supported by a sketch is deceptive, and in the process of composing groups from sketches errors are very likely to creep in. This view also accounts for a certain discrepancy in the proportions which will be noted in the different groups.

The second engraving represents two "Aborigines making and straightening spears." The two men are represented sitting cross-legged, with three bundles of spears under their legs, a small fire burning between them. The right hand figure holds a spear under his right arm, closely pressed to the body by arm and hand, while the left hand holds a cutting implement. We know that this implement must have been a tero-watta and it is very suggestive that it is completely concealed by the fingers. This proves that the tero-watta, which was used, was of such a small size that it did not even show above the first finger. The view expressed by me in a previous paper that the tero-watta was on the whole an implement of small size, is therefore fully confirmed by this engraving. But what is more important still, the position of the bent-in thumb suggests that it must rest on one face of the tero-watta, pressing it with the opposite face against the curved first finger. The position of the hand and fingers, as drawn in this picture, is therefore completely in harmony with the view time and again emphasised by me, that tne tero-watta and all other Archæolithic implements were grasped in such a way that the thumb invariably rested on the flat face, which I therefore called Pollical face.

The most remarkable feature of this figure is, however, quite a different one. The left hand holds the implement used for making the spear, not the right one! If it could be proved to a certainty that the position of this Aborigine is drawn correctly, the conclusions that could be drawn would be far-reaching. However, I rather feel inclined to think that the artist has made a most unfortunate mistake; we may safely assume that he first made a pencil sketch on paper, and then transferred that sketch exactly as he had made it on the copper plate, forgetting that by doing so the print must naturally become reversed. What he ought to have done was to transfer his pencil sketch on the copper plate, such as it appeared in a looking-glass, but not as he had designed it (1).

The second Aborigine sits like the former, cross-legged, and full front, gripping a spear between his two fists, while his teeth are holding it like a vice. The inscription says: "Straightening" the spear. The only reference I could find that the spears were straightened with the teeth is in Backhouse's book, page 172, and I confer that I was somewhat doubtful as to this practice.

(1) The scrawly character of the legend greatly supports this theory. In order to appear correctly on the print he had to draw it inversely on the plate. To judge from the almost childish scrawl, this has caused him a good deal of trouble, and therefore the theory that he did not take the pains to engrave the human figures inversely on the plate is more than probable.

Now, unless we assume that the Duterrau engraving, representing an Aborigine holding a spear with his teeth, is an invention pure and simple, a view which is certainly not supported by other evidence, we must admit that the Aborigines used their teeth in connection with the manufacture of the spears. To me it seems probable that the repeated process of placing the wood in the fire and scraping it, afterwards, curved or bent the straight shaft, and that in order to straighten it, the teeth gripped it (like a vice!), while the two hands, by slowly effecting an upward pressure, gradually bent it straight.

This view is greatly supported by the position of the hands. The back is turned outside, the muscles of the arm are in rather a strained position. Now, as everybody can observe for himself, it is very easy for the arm to exercise an upward pressure by simply moving the elbow outwards, if in the position as depicted by Duterrau. A downward pressure is much more difficult to exercise, because the points of the elbows will have to be brought together; if the Aborigine straightened the spear by a downwards pressure, which, by the way, would involve a severe strain on the lower jaw, he would have gripped the spear in such a way that the back of the hand was turned towards his own face, because in such a position the arm can easily exercise a downward pressure.

It is further interesting to note that, unlike the terowatta, which were made whenever required, the spears were made in advance for further use, the two men having made nearly 50 spears.

The fire burning between them apparently confirms the view that it was required in the manufacture, otherwise there does not seem to be any reason why there should be a fire.

These two men are depicted without the curious head dress, in their naturally curly hair, both showing rather a strong beard.

It is obvious that these two figures are the same as those shown in the larger group, and the only question that could arise is, which represents the original sketch. I almost feel inclined to think that Duterrau actually saw the two Aborigines making spears, and having sketched them, afterwards composed the group in which these same two figures are so prominent. It will, however, be noticed that in the second engraving the front portion of the spear held by the

Aborigine is longer than the posterior, while the reverse is shown in the group. Likewise, the left portion of the spear held by the man with his teeth is much longer in the group than in the second engraving. This unquestionably shows a certain amount of carelessness of observation, and reproducing observed facts, and this may tend to minimise the value of those recorded.

Engraving No. 3, published on March 23rd, 1836, bears the curious inscription: "A wild native taking a kangaroo, his dog having caught it, he runs to kill it with his waddy."

Now, we know for certain that dogs were unknown to the Aborigines previous to the arrival of the Europeans. The hunting scene, as depicted by Duterrau, can therefore not have taken place in older times, and the "wild native" must have caught his kangaroo by other means than by a dog before "killing it with his waddy." The chief interest of the engraving is, however, the fact that the "waddy" (lughrana) was used to kill animals. The sketch of the "waddy," as given by Duterrau, fully agrees with the shape of the specimens in the Hobart Museum, even the notches at one end are distinctly depicted. The hand, however, grips the lughrana, not at notched end, but fairly in the middle, and from this we may conclude that the animal was killed with a blow.

There is, however, another curious feature connected with this scene; the "wild native" grasps his lughrana with the right, while the left gets hold of the kangaroo. Now, if we assume that this engraving, not inversely etched on the plate, but transferred directly, the "wild native" grasps the kangaroo with his right, and holds the lughrana, with which he means to deliver the blow, in his left. *

The last etching, published on the same date as the former: "A kangaroo caught by a wild native's dog," is of very little interest. It practically shows nothing but two very crudely-designed figures of a kangaroo, which a dog, apparently a collie of most ferocious appearance, having claws like a bear, has caught by the ear.

The only interest is in a very crude figure of an Aborigine holding a spear in his left and a hunting stick in his right hand, of which the legend says: "The native then seizes the kangaroo and kills it with his waddy."

This engraving seems to contradict the view that spears were not used in hunting expeditions; but though Duterrau has depicted this "wild native" in the position of throwing the spear at the kangaroo, the legend, which says that "He kills it with his waddy," does not make it appear as very probable that his dog, having caught the kangaroo, the "wild native" throws first his spear at it, and then "runs to kill it with his waddy." I rather feel inclined to think that the "wild native" is shown, though in a picturesque attitude, armed with spear and hunting stick, is not represented quite truthfully. This certainly applies to the loin cloth which our "wild native" is wearing on his hunting expedition, which is rather a concession to the public of 1836 than a true fact.

Again, it appears to me very probable that the engraving ought to be reversed, because the wild native is holding the spear in his left and the hunting stick in his right.

We can sum up the value of the Duterau engravings as evidence regarding the Tasmanian Aborigines as follows:

(1) On the whole these engravings are somewhat fanciful compositions, which were probably made in the studio from rough sketches drawn from life.

(2) It is very probable that all the engravings were transferred directly, instead of inversely, on the copper plate. Hence the prints are all reversed. This detracts somewhat of their value with regard to any conclusions that may be drawn as to the use of the hands. Yet, even if it is admitted that the pictures ought to be reversed, it seems that the Aborigines used their left hand as often as their right, and were therefore ambidextrous (2).

(3) The use of a loin cloth, with which the Aborigines are provided in all the engravings, is a concession to the public, but not an actual fact.

(4) Notwithstanding these drawbacks the engravings are of great value, because they prove at least two facts which have hitherto been without corroboration, viz.: (a) That the spears were straightened by being gripped with the teeth and bent with both hands, moving probably in upward direction; (b) that the hunting stick was used to kill an animal by a blow. They further confirm the view as to the holding of the stone implement (tero-watta), and the hunting stick (lughrana), as depicted, fully agrees in shape, even as details are concerned with the specimens described in this paper.

P.S.—Since the above was written the original oil painting from which No. 1 engraving was made, and which is now in the possession of the Misses Cleburn, has come to light. This painting fully confirms my conjecture that all the engravings should be revised, because in the original the man holds the scraper in his right and not in his left as it appears in the engravings.

(2) This theory is confirmed by certain tero-watta which can only have been used with the left hand, if they were held in such a way that the thumbs rested on the Pollical face

THR

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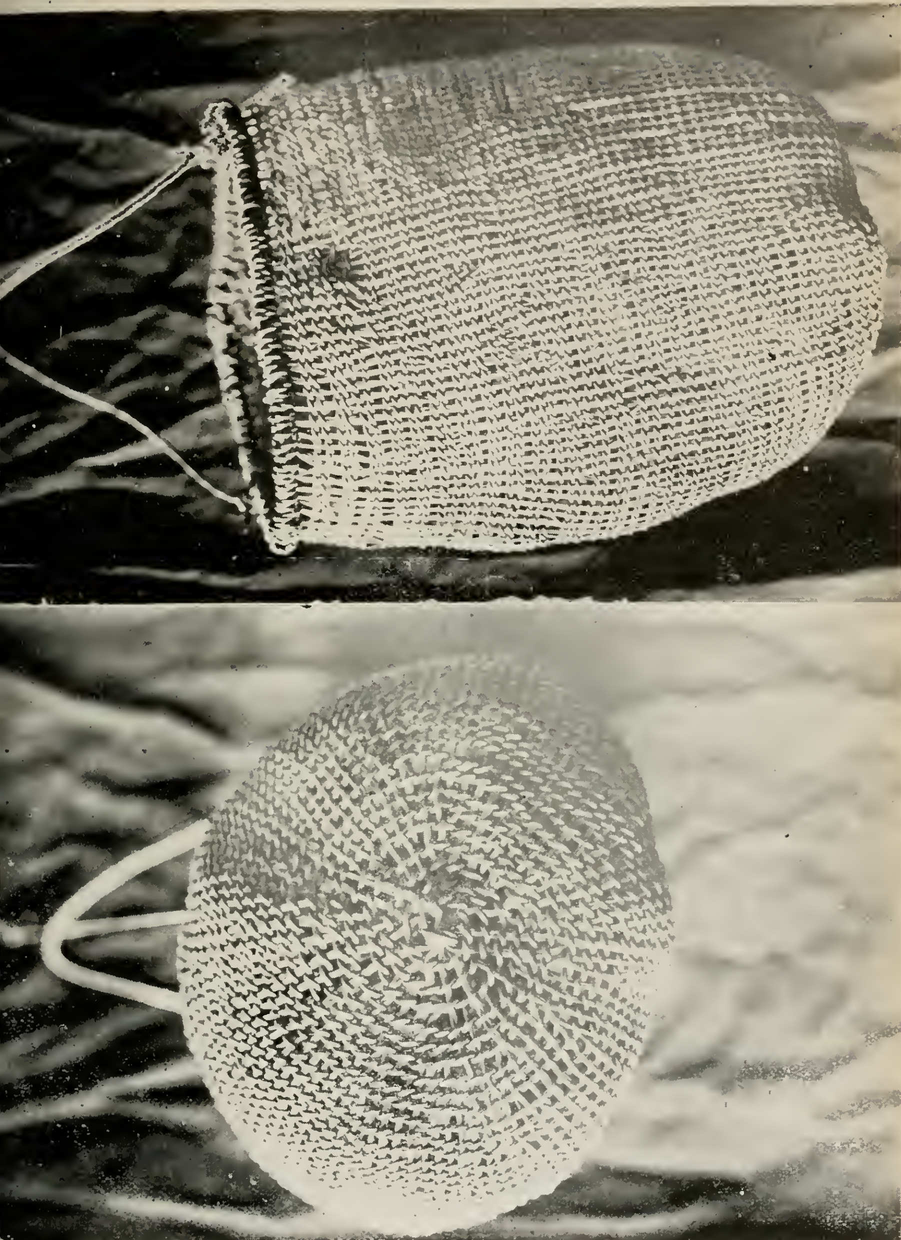
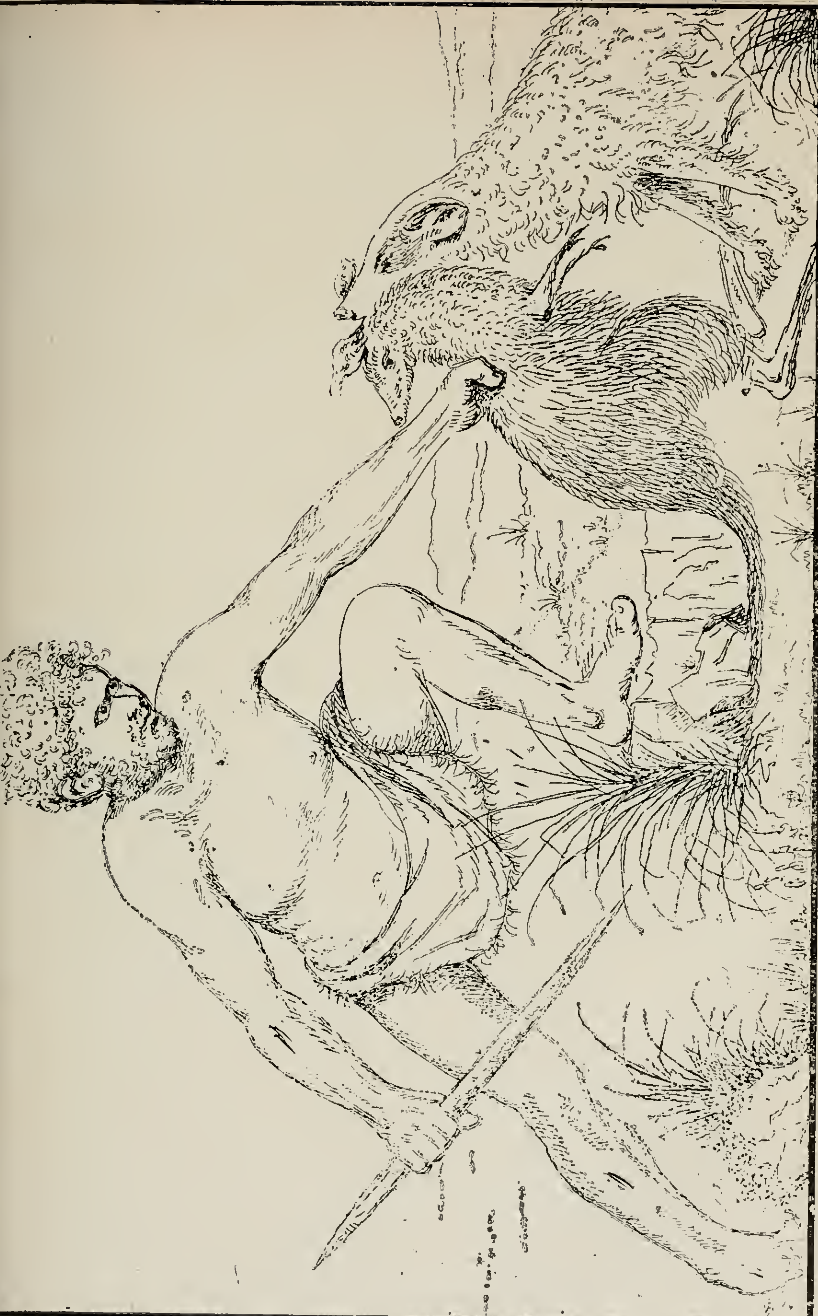
THREE HUNTING STICKS (TASMANIAN MUSEUM)
THE POINTS OF NO. 1 AND 2 NAT. SIZE.

POINTS OF SPEARS AND MIDDLE PORTION OF A SPEAR.



BASKETS.

TASMANIAN ABORIGINIES.



A Wild Native killing a Kangaroo, his Dog having caught it; he runs to kill it with his Waddy
Designed etched & published by Dr [illegible] March 23 1836 Hobart Town Van Diemen's Land

Aborigines making & straightening Spears

Drawn Etch'd & Publish'd by B. Duterrau Aug 24th 1835 Hobart Town Van Diemen's Land

ON THE CONNECTION OF SWIFTS WITH WEATHER.

By H. Stuart Dove, F.Z.S.

(Read September 11th, 1911.)

For years I have been watching the movements of the "Spine-tailed Swift" (Chætura Caudacuta), that species which comes down to us from China and Japan, and, after spending a few summer months here, departs again to those more northern climes. While living among the mountains of Northern Tasmania, it occurred to me that the appearance of this Swift was often coincident with that of a cyclonic disturbance, and this observation has been confirmed of recent years. During the summer of 1910-11 I kept notes of the various appearances of this species, together with weather conditions of same period, and, as anything which may tend to throw light upon the habits of this migrant should be placed on record, I give extracts from my journal, first remarking that in some seasons the Spine-tails appear to visit us in very small numbers, and are scarcely seen, while in other seasons, such as the summer just passed, they appear so frequently and in such numbers that they thrust themselves upon our attention.

LAKES ENTRANCE, EAST GIPPSLAND, VICTORIA.

December 8th, 1910.—A great company of Spine-tailed Swifts appeared this morning for the first time this season, circling and wheeling at heights varying from the tree-tops to practically out of sight; they were first noticed shortly before 9 a.m., and appeared to come from E.N.E., as in the case of the great company of Wood Swallows. *

Again, near the end of the same month (December), two companies of the Spine-tails were seen, before and after stormy weather.

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* (Artamus tenebrosus, Lath.)

These Wood Swallows appeared on the morning of 5th Sept., 1910 and continued their migration towards W.S.W. (See "EMU" October, 1910).

On 6th January, 1911, a party of these birds was observed in the midst of a thunderstorm, flying towards N.E., the wind at the time being N.W.

On 1st February, 1911, while proceeding by launch up the Tambo River, E. Gippsland, we noticed many Spine-tailed Swifts flying backwards and forwards over the river, some at a low elevation; the day was sunny, and extremely warm, with a light breeze from the east. Two days afterwards the sky became overcast, and we had a gale from the eastward.

February 8th, 1911.—Large numbers of the same species were seen high in the air, early in the morning, in fine weather; next day, 9th February, broke fine, but rain came on, and continued steadily until noon, while on 10th February heavy squalls of wind and rain passed over south eastern Victoria.

WEST DEVONPORT, TASMANIA.

March 11th, 1911.—Numbers of the Spine-tailed Swift appeared on 8th, 9th, and 10th March, during disturbed thundery weather, with rain, and, during part of the time, a high, tearing south-east wind. Great floods again in Victoria and parts of Tasmania.

March 20th.—The Swifts again appeared yesterday, when it was raining almost the whole day. They passed leisurely over from west to east, near the sea, and at a good elevation.

March 23rd.—Swifts again seen coursing leisurely about; soon afterwards rough weather set in.

April 16th.—Perfect morning, cloudless sky, light sea breeze (N.N.E.); Spine-tailed Swifts passed over the shore scrub at a low elevation, making west as a general direction. I said to the friend with whom I was walking, "There are the Swifts; our fine weather will not be of long continuance." That very night great piles of cumulus appeared in the eastern sky, and drifted gradually overhead, bringing a heavy downpour.

April 25th, 1911.—On this day the Spine-tailed Swifts were seen "migrating," passing to the N.W. over the beach, at a height of 60 or 80 feet; weather cold, showery, squally, wind veering N.W. to S.W.

April 27th.—This afternoon the Swifts passed to the north-west in a long, straggling party over the beach and the sea, at a height of perhaps 60 feet; wind south-west, strong, cold. This was their last appearance, and the latest date I have ever seen them; I believe it constitutes a record for Tasmania, if not for the Commonwealth.

Now, the Spine-tailed Swift is very fond of ants in the winged state, and these insects constitute a large portion of its food while with us: I have noticed that the male and female ants of various species attain the winged state, and "swarm," or issue, in vast numbers from the nest, generally during the moist, muggy weather which precedes an atmospheric disturbance; the termites, or so-called "White ants," will often swarm during a light, warm rain. It has, therefore, occurred to me that the frequent appearance of the Spine-tail Swift either during, or shortly before or after, disturbed atmospheric conditions, may be due to its winged food occurring more plentifully at these times. There may be other conditions of which at present we know nothing, affecting the sudden appearances and disappearances of this most interesting species, but the theory here advanced seems a reasonable one, and I shall be glad if it is the means of inducing other observers of our migratory fauna to give particular attention to the habits of this swift.

FURTHER NOTES ON THE HABITS OF THE TASMANIAN ABORIGINES.

Pl. XVI., XVII., XVIII., XIX., XX.

By Fritz Noetling, M.A., Ph.D., Etc.

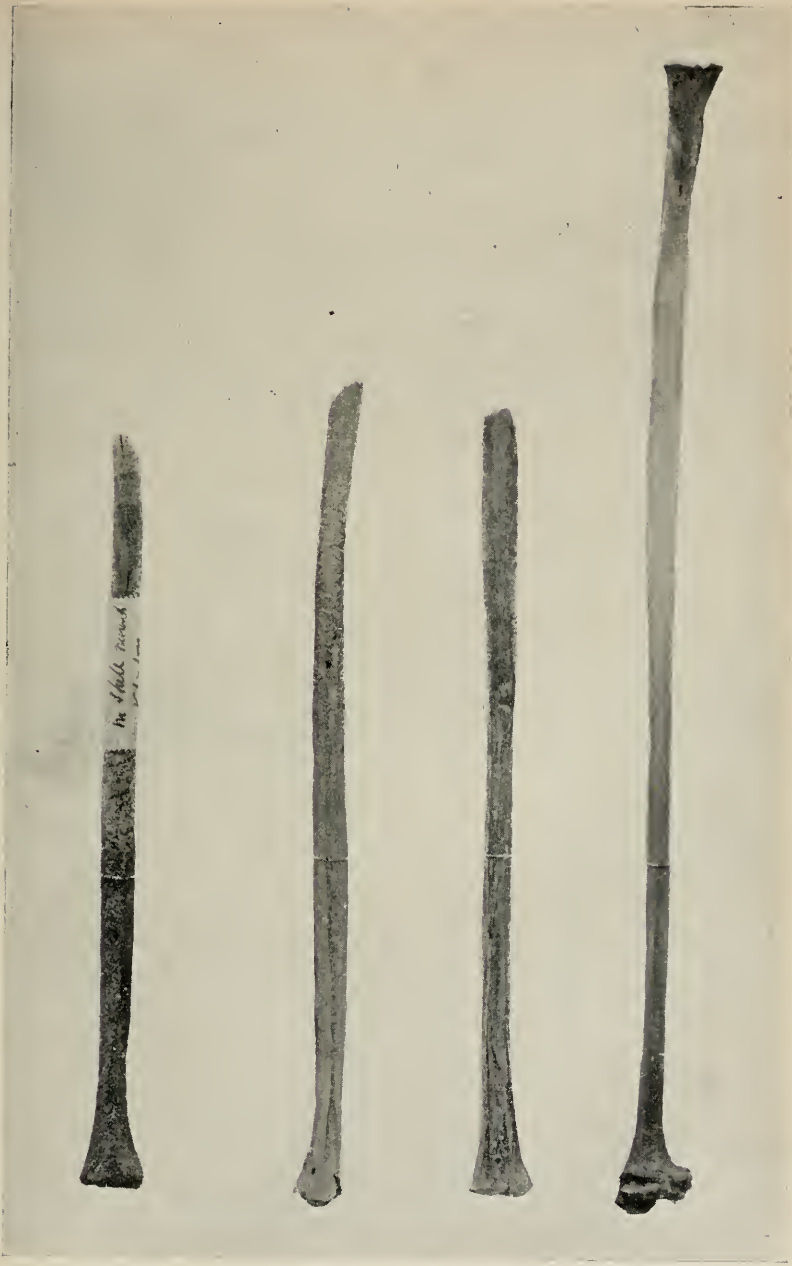
(Read October 9, 1911.)

1. DID THE TASMANIAN ABORIGINES MANUFACTURE BONE IMPLEMENTS?—Pl. XVI.

The question whether the aborigines used bones of animals, either entirely or in fragments, for implements is of great importance. It has hitherto been assumed that bone as a material for implements did not come in to use earlier than the Magdalenian stage. If this be so, all the earlier industries, which, of course, include the archæolithic stage, did not use bone, either as a material from which implements were manufactured, or, indirectly, as a tool to press off small flakes, in order to sharpen the edge. The Tasmanian industry, which, as we have seen, represents the typical archæolithic stage, should, therefore, not know the use of bone. It would constitute one of the greatest anomalies in the evolution of mankind, if it were a fact that the aborigines did include bone among the materials from which they manufactured their implements. I can safely say that there are few persons living who have so carefully studied and examined the camping grounds as I have, but never did I find a single piece of bone that could even, with the greatest stretch of imagination, be considered as an implement; in fact, the almost total absence of bones or fragments therefrom on the camping grounds has always struck me as rather remarkable.

Yet there is a general belief among the amateur collectors that the aborigines manufactured a kind of scoop from bone, and such specimens are greatly valued. Among the great treasures of the Hobart Museum there is a bundle of bones labelled, "Bone implements manufactured by the aborigines."

I had always my doubts as to the authenticity of these bone implements, and I am now in the position to con-



SO-CALLED "SCOOPS" AND THE FIBULA OF MACROPUS BILLARDIERI.

clusively dispel the view that the aborigines ever manufactured implements from bone.

When excavating together with Mr. T. Stephens the great shell deposit in a cave near Rocky Cape, I collected a fairly large number of bones, mostly consisting of kangaroo, opossum, wombat, seal, and numerous bones of birds. The bones were mostly in a fragmentary state, and the larger ones, apparently femur and humerus of kangaroo, were evidently intentionally broken. None of the splinters showed even the slightest trace of use. I had, however, the good luck of finding several of the "scoops," and their appearance seemed to exclude the view of an artificial origin. As these specimens show considerable length, I was pretty certain that they could not come from any other part of the body but the extremities, probably the posterior ones, of a kangaroo; I, therefore, compared the leg bones of a kangaroo, and I could prove the complete identity of the so-called "scoops" and the fibula of the kangaroo.

From Pl. XVI. it will be seen that the fibula of the kangaroo is rather a thin, slender bone, which closely lies on the tibia. The distal end of the fibula is cylindrical, but it becomes deeply concave in the proximal part. The thinnest and most fragile portion of the fibula is almost in the middle of its length, about there where the concave rather broad proximal portion contracts very quickly, and becomes flat, before merging into the cylindrical distal portion.

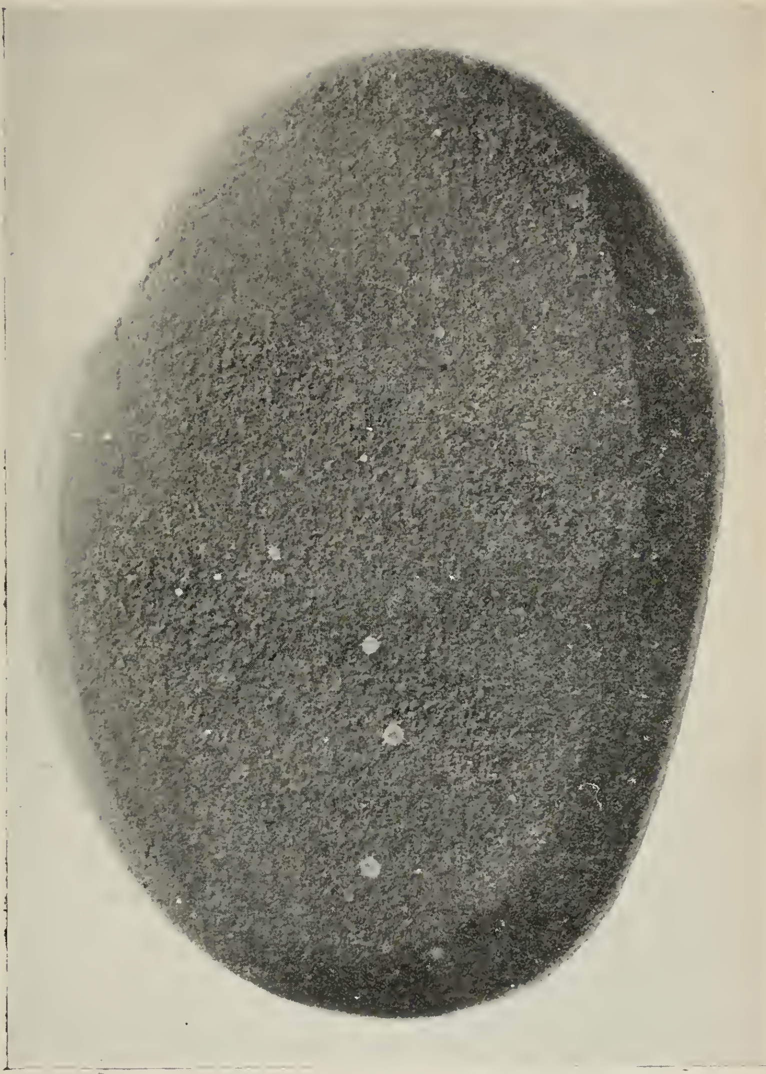
It can, therefore, hardly be surprising that when a kangaroo comes to grief it is usually the fibula that breaks first. I have seen many a kangaroo hunted, and if they fell over a cliff, the fibula was regularly broken, and always at the same place, that is to say, where the bone was weakest. The bone was, therefore, broken into two parts, the cylindrical distal, and the hollowed out, concave proximal part. The concave part, formed the "scoop," so highly treasured by amateur collectors, and its shape was the more suggestive of artificial work, particularly because it was strongly attenuated.

Once more the well-known fact that an actual observation, however simple it may be, is worth more than all the finest theories, is shown to be true. The numerous amateurs who collected these specimens jumped at once to the conclusion that these were "scoops" manufactured by the aborigines, but not one of them did look at the leg bones of a kangaroo.

It must be considered as rather fortunate that, before the statement that the aborigines of Tasmania did use bone in the manufacture of their implements, has been spread through the literature, it could be proved to be absolutely wrong. It is rather remarkable that such a theory should ever be credited even among amateurs, because the question might well be asked: For what purpose could these "scoops" be used? The general belief is that they were used to scoop out the marrow from the bones, and this once more proves how wrong it is to judge from our customs and habits those of a far inferior race. It does not follow that because we use a special instrument to scoop out the marrow from the bones of the big ruminants that the aborigines did the same; in fact, if one thing is certain it is, that they did not. If they wanted the marrow the easiest and quickest way to get it was to smash the bone, and this they did, as has been proved by the broken fragments in the cave deposits. The idea that a primitive human being like the Tasmanian sat down holding in one hand the cooked marrow bone, and in the other the scoop, daintily scooping out the marrow, is intensely comical. Its absurdity becomes more conspicuous still, when we consider that the end of the marrow bone had to be cut off by means of a stone, and that the marrow was obtained quicker and easier by breaking the whole bone at once, than by knocking off one end, and afterwards scooping out the marrow.

I have dwelt at some length on this absurd theory, because it is very illustrative of the way how the most ludicrous interpretations of archæolithic remains can arise.

The archæolithic civilisation did not know the use of bone as a material for the manufacture of implements, and the Tasmanian industry forms no exception from this rule; in fact, we know now for certain that even the most careful examination has failed to discover any specimen of bone that had been used as an implement. It might be argued that in Europe, where the archæolithic civilisation is of great age, the bones had become decayed, and there being no bone implements does not prove that they were not used. From our investigations we know now that the archæolithic civilisation of Tasmania did not know the use of bone as material for implements, and we can, therefore, conclude that this also applies to the same type of civilisation in Europe. Those who held that the use of bone is a more modern invention, which the primitive industries had not yet made, were, therefore, perfectly right, and this view is fully borne out by the researches in Tasmania.



GROUND "SACRED" STONE, OLD BEACH.

2. DID THE ABORIGINES KNOW THE ART OF GRINDING?—Pl. XVII.

It has always been most emphatically asserted that the art of grinding was unknown to the aborigines. My collections have, however, proved that the operation of grinding was not unknown to them. It appears, however, that they never, under any circumstances, used it in the manufacture of tero-watta, but strictly limited it to the manufacture of the flat, so-called "sacred" stone (1). I never found a single tero-watta which even shows the faintest indication of being ground or polished, but I have found numerous sacred stones, which show more or less distinct traces of having been subjected to the process of grinding. I described some specimens in a previous paper (2), but, though the indications may, perhaps, not be quite so convincing, the specimen Pl. XVII. from the Old Beach gives us an absolute proof. This specimen was found by Mr. E. S. Anthony, who kindly presented it to me. It is an oval, very flat diabase pebble, measuring 5 x $3\frac{1}{2}$ to $1\frac{1}{4}$ inch, and weighing 1lb. 8oz. avoir. Both the upper and lower sides are flat, but while the lower side is rough, probably on account of weathering, the upper side has been most elaborately polished and ground. The grinding even extended to the peripheral portion, and fine sharp edges were produced. Three rough marks, extending obliquely across the upper side, form a conspicuous feature, particularly as the surface between them is slightly convex. In my opinion, these marks are incidental, and they represent a portion of the original crust, which was not quite removed when the pebble was ground.

The specimen is well preserved, except for a large fragment broken off from the margin. Now such sharp faces and edges as this specimen exhibits can only be produced by grinding; any other explanation is impossible. Modern man would assume that the stone was ground on another one; at least, if he were to reduce such a stone he would proceed in such a way. The Tasmanian may have proceeded differently; he rubbed and ground the specimen

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(1) I prefer to use the term "sacred" instead of "magic" in describing this peculiar group of stones, because it better expresses their nature than the word magic.

(2) Some implements of the Tasmanian aborigines. The Tasmanian Naturalist, vol. 1, No. 3, 1907.

with another stone till desired result was produced. This is unquestionably indicated by the two large "mortars" in which the concavity has been produced by grinding the stone with another one of globular shape.

As a contribution towards the psychology of the Tasmanians, the fact that they had, at least, a rudimentary knowledge of the operation of grinding is of the greatest importance. The question may well be raised, if the operation of grinding was known to them, why did they not use it to improve the tero-watta, but limited it strictly to the sacred stones? The aborigine who ground and polished the specimen (Pl. XVII.) was, apparently, quite aware that he improved it. Under these circumstances it is more than surprising—I always speak from the point of the modern mind—that it never struck him to apply the same process to a tero-watta. It might be argued that the material of the tero-watta was too hard for grinding. This is by no means the case, as I have proved by experiment. Chert or hornstone is easily ground and polished on sandstone; in fact, almost easier than diabase. This proves, in my opinion, that the difficulty of grinding a hard rock cannot have been the reason why the tero-watta were never ground.

The reason must have been quite a different one. The aborigines were not above a certain logical reasoning, as is proved by the production of red ochre (3). But, on the other hand, they were absolutely incapable of conceiving new ideas for the improvement of their implements. This has been amply demonstrated by the peculiar tero-watta described in a previous paper (4). If they could not make such a simple invention as to continue the trimming of the whole edge on both faces, it is not very probable that they applied a process, which was restricted to the "sacred" stones for the improvement of the tero-watta.

There may, however, be another, perhaps, more weightier reason still. I have shown that the sacred stones must in all probability be considered as specimens connected with certain rites or religious notions (5). It may be possible that an operation which was used in the produc-

(3) Red Ochre and its use by the Aborigines of Tasmania. Pap. and Proceed. Royal Society Tas., 1909, page 30.

(4) A Peculiar Group of Tronattas. Pap. and Proceed. Roy. Soc. Tas., 1909, page 1.

(5) Some Implements of the Tasmanian Aborigines. The Tasmanian Naturalist, vol. 1, No. 3, 1907.

tion of these specimens was not to be applied to profane purposes. This is merely a suggestion, which may be accepted as a plausible explanation or not.

However that may be, it is beyond doubt that the Tasmanians had already acquired a rudimentary knowledge of grinding, but they applied it to one purpose only, viz., to the manufacture of the sacred stones, and never to any other.

This is one of the numerous strange facts which we meet with in studying the Tasmanian race. In my opinion, this points to one direction only. The Tasmanian race had already reached their highest point of evolution; it was impossible for them to go further; they could not conceive new ideas, or make new inventions, and had the race still existed for another thousand years, at the end of that period they would have exactly been where they were at its beginning. It is unquestionable that the incapability of the Tasmanian race to adapt themselves to new ideas or surroundings accelerated its extinction.

This view has now been proved by so many observations that we may take it as certain that the Tasmanian aborigines represented a race of mental stagnation. They may have been distantly related to the races now inhabiting the Australian continent, but it is absolutely inconceivable how, in the face of these facts, a recent writer (6) could consider the Tasmanian aborigines as an insular type of the Australian aborigines.

Let us consider the logical consequences of this theory. Dr. Basedow admits that the Tasmanian aborigines came to the island previous to its separation from the mainland, and, as a necessary corollary, previous to the arrival of the dingo.

If the Tasmanian aborigines were only an insular branch of the Australian race, we must assume that at the time of their migration to the south-eastern corner of Australia, now represented by Tasmania, the whole of the Australian race was in the archæolithic stage. There is no getting away from this, because the Tasmanians represent that stage, and never got beyond it.

On the other hand, we find on the Australian contin-

6) Basedow, Der Tasmanier Schaedel, ein Insulartypus, Zeitsch. f. Ethnologie, vol. 42, pt. III., 1910, page 175.

ent the palæolithic as well as the neolithic stage. I have very little knowledge of the Australian stone implements, but it appears to me that in the Australian implements there is no lower stage represented than the Solutreen, of which probably the West Australians may be the type. The confirmation of this view may, perhaps, be of great importance, but luckily it does not bear directly on the present question. One fact, however, is absolutely certain, the civilisation of the Australian aborigines represents a much higher stage than that of the Tasmanian aborigines. In other words, the Australians developed (7), while the Tasmanians remained stagnant. At the first glance my theory of the mental stagnation of the Tasmanian aborigines seems to confirm such a hypothesis, but on closer examination it will be seen that such view is untenable. If the mental qualities of the Australian race had the germ of further development, why did only the Australian branch reach a higher stage while the Tasmanian one remained stationary? This question must first be conclusively answered before Dr. Basedow's theory can be accepted.

It is impossible to assume that the struggle for existence is responsible for this. Let us examine the physical conditions under which the so-called two branches lived. The average temperature of the Australian continent is decidedly higher than that of Tasmania; the climate is, therefore, considerably warmer in Australia than in Tasmania. On the other hand, except the northern tropical portion, Australia is much drier than Tasmania. The search for drinking water is certainly more arduous in the Australian continent than in Tasmania. Food was, if anything, probably easier to find in Australia than in Tasmania. Neither in Australia nor Tasmania large carnivorous animals existed as enemies of the human race. If anything, Tasmania can boast of the most ferocious of the two, the tiger and the devil. Human enemies were the same, both in Australia and Tasmania and we have it on record that the Tasmanian tribes lived in constant internecine war.

The absence of large animalic enemies, the constant intertribal feuds, the plentifulness of food being the same

(7) It matters not the least whether the Australians had already reached the higher stage when they arrived in Australia, or gradually acquired it since their arrival. I feel, however, inclined to believe they represented already a higher stage when they invaded the Australian continent, than its original inhabitants, viz., the Tasmanian race.

for the aborigines of Tasmania as those of Australia, the only real difference is a warmer, more congenial climate in Australia than in Tasmania. Whether the comparative scarcity of water in Australia is of any real importance may be somewhat doubtful, because it is very probable that the natives carefully avoided all those tracts where water is scarce.

Though the Tasmanian climate was more severe than the Australian, it is, therefore, not very probable that the struggle for existence was more arduous in Tasmania than in Australia, and, though an occasional drought may have affected the Australians, the same struggle was not harder in Australia than in Tasmania.

The conditions of life of the Australian aborigines and those of Tasmania were, therefore, pretty much the same, except for the difference in the climate. Is it possible, or even probable, that this difference of climate accounts for the difference in the evolution of the two branches? I fairly doubt it, and it is generally assumed that those races living in a cooler climate are superior to those living in a warmer one. If Dr. Basedow's theory were correct, this general experience would be erroneous, as far as Australia and Tasmania are concerned.

I, therefore, think that, notwithstanding their similarity of the skull, the Tasmanian aborigines are different from the Australian aborigines. Both may be derived from the same stock or root, but it is more than probable that the Tasmanians represent the older, the Australians the younger branch.

We know now for certain that the separation of Australia and Tasmania took place after the disappearance of the glaciers in Tasmania, and after the extinction of the gigantic marsupialia, which, as the palæontological evidence of the Mowbray swamp proves, must have lived up to, what we would call in Europe, historical times. The occupation of the present island by the aborigines has, therefore, taken place in quite recent times, and I estimate the period that has since lapsed at not more than 7,000 years.

Even admitting, for the sake of argument, that this period were too short, is the time since the separation long enough to produce such serious changes in the cranium of the aborigines as Dr. Basedow assumes? I fairly doubt it; but, what is more, why should the mere fact of separation produce osteological changes in the Tasmanian aborigines and not also in the Australian natives? This statement of Dr. Basedow is absolutely without foundation, and he proves himself by it a worthy disciple of Herr Klaatsch, who enjoys such an unenviable notoriety for superficial work.

In conclusion, I may mention another theory promulgated by Dr. Basedow, viz., the origin of the superciliary ridges. Dr. Basedow believes that they represent a secondary feature, probably due to the intense glare of the sun in Australia. But what about the Tasmanians? Why should they develop such strong superciliary ridges? There is not such a strong glare in Tasmania as in Australia; the rays of the sun are much more oblique in lat. 43deg. than in lat. 35deg. to 15deg., and there was, therefore, no need for the Tasmanians to develop such strong ridges in order to protect their eyes from the glare. The logical consequences of Basedow's theory are almost too ludicrous for words. If he were right all races living under the tropical sun should develop strong superciliary ridges. This is certainly not correct, as far as India is concerned, as I can vouch from my own experience, and I fairly doubt whether the glare of Australia is worse than that of the Indian desert or Baluchistan. All the African and American races living between 43deg. north and 43deg. southern lat. should develop superciliary ridges, if Basedow's theory were correct. The Italians, the Spaniards, in fact, any European living south of 43deg. north lat., should develop strong superciliary ridges to protect his eyes from the glare, but I am afraid that we would fail to discover them. I do not think that many will share Basedow's view as to the origin of the superciliary ridges, and I am probably correct if I assume that far the majority will consider them as what they really are, viz., primitive features.

Since the above was written a very severe criticism of Dr. Basedow's paper has been published in the same journal, Vol. 43, 1911, Pt. II., page 287. Professor Dr. von Luschan, one of the greatest living authorities on craniology, points out that Basedow's paper is scientifically valueless, full of errors, mistakes, and wrong deductions. It is hardly surprising that Professor von Luschan notices Basedow's ludicrous theory, and he says:—"Basedow does not tell us how it could happen that the skull of the Tasmanian became broader and the hair more curly because Tasmania became separated from the Australian continent." Professor von Luschan calls Basedow's paper

"Eine Entgleisung." Literally, this word means "a derailment" (of a truck), but, metaphorically used, it is a polite form of saying that the contents of a paper are scientifically without value; in fact, that they are not worth the paper on which they are printed.

3. DID THE ABORIGINES MAKE TERO-WATTA OF APPARENTLY INTENTIONAL FORM?—

Pl. XVIII., XIX., XX.

I have repeatedly pointed out that one of the chief characteristics of the tero-watta is the absence of every intentional or conventional shape. The specimens here described appear to be contrary to this rule; at least, it is very difficult to imagine that their outline is purely accidental. If this be so, we may well ask: What do these specimens represent? It is impossible to suppress the notion that No. 1, Pl. XVII., does not represent a four-footed animal? The outline is so suggestive that any other interpretation does not appear probable; the two lobes representing the feet are so thin and fragile that if they were ever used for any purpose whatsoever where the slightest pressure was required they would break at once; yet they are most carefully chipped all along the edge. More curious still is, perhaps, No. 2, Pl. XVIII. The view that it represents a double concave scraper, and that the bill between the two concave edges is accidental, is at once disproved by the fact that the point of the bill is carefully chipped and rounded off. If we assume it to be a borer, we may ask why was the point of the drill, which ought to be sharp, carefully rounded off, and what would be the good of such a short, rounded-off point when any sharp splinter could perform the operation of boring much more effectively? If we go through the whole number of specimens that have come under notice, we will see that in every instance there are weighty reasons speaking against their use as implements or tools, and equally weighty reasons pointing towards the assumption that their outline is not purely accidental, but is rather shaped to represent a certain object. If this view be correct, the specimens have to be considered as figure stones, that is to say, stones which were shaped with the view of representing a certain object. These objects are:—

(1) A four-footed animal.

(2) The human face.

(3) A bird's head.

(4) A snake.

Specimens of this kind are pretty rare, but from the first their peculiar shape induced me to set them aside from the others.

Prof. Schweinfurth described in an interesting paper on the cave deposits of Sicily (8) certain specimens as figure stones (Pierres figures, pierres figurees), which reminded me at once of the Tasmanian ones. Prof. Schweinfurth states that the Sicily specimens mostly represent heads of animals. Much rarer are complete figures, but he did not notice any specimens representing human heads or figures.

The figures he gives of these specimens are, however, not very convincing, and it requires some stretch of imagination to recognise in figures A, B, C, D, E, F, Pl. X., the heads of birds. The most convincing are figures L and M, Pl. XI., though even here a certain imagination is required. However, I do not wish to discuss the probability of Prof. Schweinfurth's views; all I want to point out is, that if the hypothesis with regard to the Sicily specimens be correct, the view I have taken with regard to the specimens here described is still more so, because their outline is much more suggestive than that of the Sicily specimens.

I am fully aware that it is a very delicate subject I am dealing with, and I particularly wish to point out that I do not consider it more than a working hypothesis. I shall be very pleased if anybody else can suggest a better one, because if we accept it we admit that the Tasmanian aborigines had already developed a certain sense of art. This feeling induced them to reproduce in the unwieldy stone certain objects with which they were familiar, animals in the first instance.

(8) Ueber das Hoehlen Palaeolithikum von Sicilien, Zeitschr. f. Ethnologie, 1907, vol. 39, pt. 6, page 879.

If we muster the animals of Tasmania we find that the large four-footed animals so abundant in Europe are totally absent. There are only two animals in which the four extremities are well recognisable to the eye, viz., the Tasmanian tiger (Thylacinus) and the Tasmanian devil (Sarcophilus ursinus). In the wombat (Phascolomys wombat) the extremities are very short, almost hidden by the body, and the kangaroo, either resting, but particularly when chased, looks more like a two-footed animal than a four-footed one.

Among the birds, the now extinct emu, with its sharp beak, must have been a conspicuous object. I hardly need to say that the snake, so common in Tasmania, must have been an object of terror to these naked savages.

Last, but not least, there are those specimens which suggest the profile line of a human face, which certainly represent a remarkable feature. When a child draws a human face in profile the most conspicuous part besides the circular head is the nose, added in the shape of a triangle. The specimens here described show this feature in a marked way, and, as I said above, it is impossible to suppress the notion that these specimens really represent what they appear to be.

Whether the hypothesis here promulgated be correct or not, one fact appears to be pretty certain, the outline of these specimens is not accidental, but the result of deliberate work, with the intention to produce a certain shape.

Pl. XVIII.

This is the finest specimen which, so far, has come under my notice. It measures 100 mm. in length, and its greatest height is 71 mm. The thickness is small, the greatest thickness barely exceeding 10 mm.

The rock used is a fine grained hornstone, of dark grey colour, apparently finely stratified. It is covered wi[illegible] rather a thick patina of light grey colour, having a yellowish tinge. The outline of this specimen cannot be better described than representing that of an animal, having a thick body, a rather short, but thickly-set, head, and short, squat legs, and the posterior portion of the body shortly rounded off.

The upper edge forms almost a straight line, and is very carefully chipped from the posterior end to the middle of the head. Here a piece of the original crust of dark brown colour is still preserved, but there is no marginal chipping up to the anterior end. From here the lower edge is most carefully worked for its whole length. It is obvious from the chipping that the deep anterior concavity of the margin is intentional and not accidental. The chipping is particularly careful at the lower edge of the anterior extremity, and but somewhat less on its posterior side. Unfortunately, a fragment broke off the posterior extremity, though, to judge from the patina, this damage must have been caused at the time of manufacture. It appears, however, that the posterior side was much less carefully chipped than the other edges.

The indical face is flat, and shows the traces of a few large flakes being broken off. The pollical face is flat, but somewhat wrinkled; no bulb of percussion is visible, but it appears probable that it was situated at the point of the posterior extremity.

Well may we ask, what was the use of this remarkable specimen, supposing it were used as an implement? The three concavities might suggest the use as a scraper for spears or hunting-sticks. Assuming this being so, it is impossible to account satisfactorily for the fine chipping of the lower edge of the anterior extremity. This could not have possibly served any useful purpose, because of the thinness of the anterior extremity, which is only 3 mm. At the base where it measures 23 mm. in width, the thickness is slightly larger, being 6 mm., while the total length is 26 mm. If any strain or hard pressure were brought on this thin piece of stone, either at its lower or its lateral edges, it would break off at once. This fact makes it impossible to assume that this part of the stone ever served any useful purpose. The same applies to the posterior extremity.

The upper edge may have been used as a scraper, but, admitting this, we may rightly ask: Why, then, was the lower edge so carefully hollowed out and chipped to such an extent that even the thin piece separating the anterior and the middle concavity was chipped at its lower edge, when all this work was to no useful object whatsoever?

The study of the tero-watta has revealed some strange features of the mental state of the aborigines. Admitting that they did a good deal contrary to modern expectations, it would certainly go too far to assume

FIGURE STONE (ANIMAL ?)

FIGURE-STONE (HUMAN FACE ?)

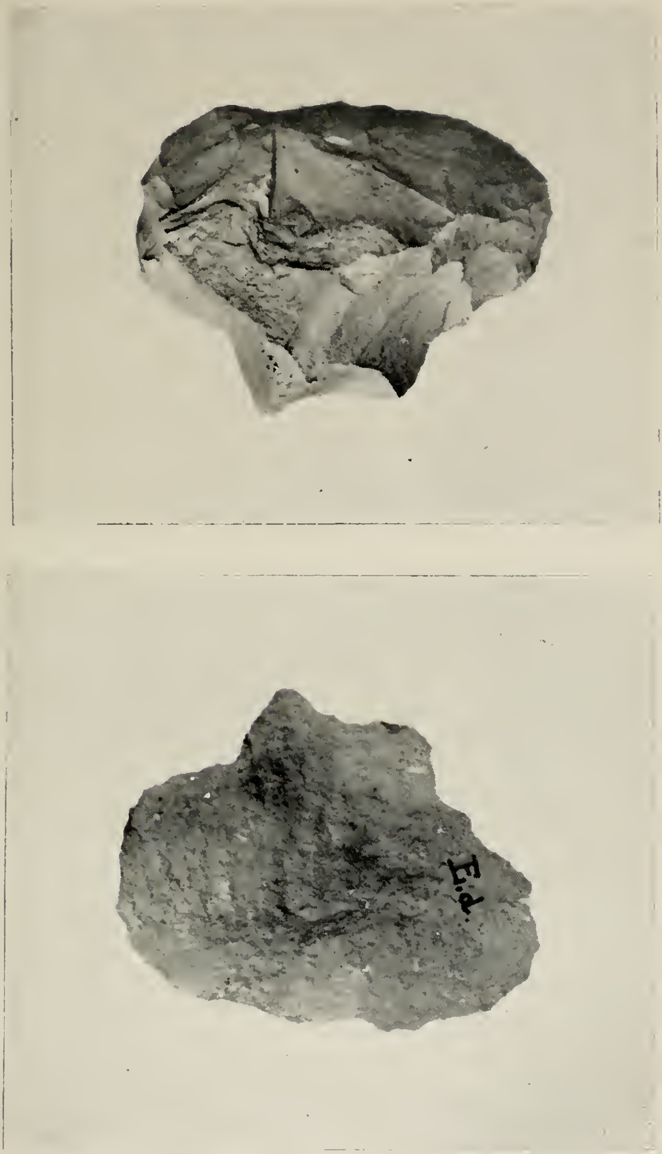

FIGURE-STONE (HUMAN FACE ?)

N.B.—In the original the left block is reversed; it ought to be turned as shown here.

that they deliberately spent a good deal of work, knowing all the time that it was useless.

I rather think it more probable that the outline of this specimen was deliberately shaped to represent a certain object, viz., a four-footed animal, in all probability a wombat.

The group of specimens now to be described are, perhaps, the most interesting of the whole number, because if the hypothesis of the figure stones be correct they must represent human faces. It may be argued, but the face, i.e., the profile, is not that of an aborigine. Granted that this is so, but can we expect that a primitive race could shape such primitive implements from so unwieldy a material as stone an exact likeness of a human being? That would be exactly the same as if we were to expect a child that makes its first attempt with a piece of chalk to draw an accurate portrait of his father. Nobody could reasonably expect this, and we must, therefore, not expect that the specimens here considered as human faces must represent an accurate profile line of an aboriginal face. It were rather surprising if the aborigines had already acquired such a high perfection in art, considering their otherwise low state of civilisation.

I wish it, however, to be well understood that I do not say that these specmens represent a human face. All I contend is that the outline of these specimens is intentional and not accidental, and if the hypothesis of the figure stones is correct, there can be only one interpretation of these specimens: they were manufactured to represent a human face.

Pl. XIX.

This very interesting specimen was found at Mona Vale. Its length is 84 mm., the greatest breadth 52 mm., the thickness 34 mm. The nature of the rock cannot be quite correctly ascertained; but, to judge from the colour of the patina, and from that of the original crust, as well as the fine conchoidal fracture, the rock must be a hornstone of either grey or dark blue colour. It differs, however, from other hornstones, that large numbers of small perforations or holes, which on the surface are filled with a ferruginous matrix, are irregularly distributed through the substance. The whole surface is covered with a patina of light brown colour, while that portion of the original crust which still adheres to the indical face is of dull red colour.

The outline is the profile of a human turned towards right. On the right side we see, as a most conspicuous feature, a little above the middle, a short, rather broad prominence, rounded off at its end. Above it the edge is deeply concave, but bulges out again, and comes round in an elliptical curve to the left side. Below it the edge is a little less concave, then turns into a fairly straight line, which, before reaching the proximal edge, forms another concavity. The left edge is convex at its distal, concave at its proximal position. The proximal edge is straight.

The pollical face is flat and smooth; no distinct bulb of percussion can be seen. The indical face is strongly convex, and is a good deal worked. The flaking is, however, limited to the right and the left side, while towards the distal end the original crust is still preserved.

The right side conclusively shows that the prominence has been deliberately made by striking off flakes above and below it. The result is a ridge in its middle, which runs from the edge towards the left. The three concavities were eventually produced by three blows of different strength, the top one being the strongest, the lower one the weakest. It can further be seen that these blows were effected after the production of the prominence. The left side has been well chipped, particularly towards the proximal end. The chipping of the indical face has not reduced the thickness of the stone, and it is clear that the reduction of thickness was not desired; otherwise the thin ridge formed by the flaking of the left and right side could have easily been struck off by a simple blow.

I will attempt to discuss the probable uses of this peculiarly-shaped specimen. The most natural suggestion is that it served as a borer, the prominence being, apparently, well suitable for such a purpose. If we examine it, however, more closely we perceive that it is most unsuitable for boring, its end being too blunt and rounded off to be used for making a hole. But supposing it did serve as a borer, what good was it to the left edge, which was quite useless? The original crust proves conclusively that the flake cannot have been much larger than it is now, and that, therefore, the removal of an inconvenient part cannot have been the object of trimming. Further, if the prominence was a borer, why was the lowest concavity of the right edge made? Surely, that little convex part above it cannot have been used as a borer. Perhaps the weightiest objection against the borer hypothesis is the breadth of the prominence. We are sufficiently well informed about the habits of the aborigines to know that they did not possess any object that required a borer to produce holes from 10-12 mm. in diameter.

The second hypothesis would assume that the prominence was accidentally produced by the edge above and below it being used as a scraper to polish spears. Against this view speaks the fact that the prominence was intentional and not accidentally left. We further know that the lowest concavity was produced by a special blow, and whatever it was meant for it is certain that it cannot have served as a scraper. The same that has above been said about the left side applies to the scraper theory.

If we admit that this specimen was not used as an implement, the only possible view we can take is that it is a figure stone, representing the profile line of a human head.

Pl. XX.

This specimen was found at Melton Mowbray. The length is 57mm., the greatest breadth 44 mm., the average breadth is a little smaller, viz., 31 mm., the thickness nowhere exceeds 21 mm., the weight is

The rock is the typical hornstone of dark blue colour, occurring at Johnston's quarry, Melton Mowbray, and there cannot be the slightest doubt that this specimen was made from a piece of rock that was obtained from the quarry. There is no patina, but a rather thick portion of the original crust still remains on the indical face. The crust is of light greyish colour, and its surface rather ferruginous.

The outline would be elliptical, if the continuity of the curve were not interrupted by a broad two-pointed prominence. The right edge is slightly curved, and passes gradually into the more strongly curved upper and lower edge. The left hand edge is deeply concave in its upper, somewhat less so in its lower portion. Both times the concavity forms an oblique angle. Between these there is a prominence measuring 26 mm. in length, terminating in two points, of which the upper one is longer and sharper than the lower one, which is slightly rounded off. Between the two points the edge is slightly concave.

The pollical face is very smooth and flat, but there is no bulb of percussion. The indical face is rather convex, and well worked. Upper, right, and lower edge are most carefully chipped. The outline of the left edge was produced by a few large flakes being struck off in such a way that a ridge runs to the extreme end of the two points.

When we examine this specimen we perceive that a good deal of work was spent to produce the regular curve of the continuous upper right and lower edge, and, noticing this, we may well ask, Why was the work not completed all round? Two blows would have been sufficient to strike off the prominence, and a most perfect oval would have resulted. Though the right edge could have served as a scraper, the left edge was perfectly useless. I do not think that anyone will assume that it served as a borer. If it was a borer, why the careful trimming of the other edge? The three concave portions are certainly not accidental. We see distinctly that the upper middle and lower one were produced by striking of a single flake. The flaking was probably executed by means of a sharply-pointed hammerstone, because the spot where the point struck the pollical face can distinctly be seen on the edge.

This specimen affords a still greater problem than the above, and before proceeding further I want once more to summarise the facts that cannot be disputed. These are :—

(1) There is the evident intention to produce a specimen of elliptical shape by the careful trimming of at least 5-6th of the circumference.

(2) The lower, right, and upper edge are most carefully worked.

(3) The peculiar outline of the left edge has been produced by three separate blows, and the three concavities are, therefore, not accidentally produced.

What conclusion can we draw from the above observations? The fact that the left edge is much less carefully chipped than the rest of the circumference might suggest the view that it represents a reject which was dropped before it was finished. I fully admit the weight of this argument, but we may well ask is it possible to imagine that after such an amount of work had been spent in shaping it, it was dropped when two more blows had been sufficient for completion? If the theory of the unfinished reject were correct, it would add one more fact illustrating the curious condition of the mind of the aborigines by doing certain acts which are impossible to our mind. There is no doubt that the theory of the unfinished reject deserves consideration, yet I am not quite convinced of its correctness. The flaking of the left edge shows that its outline was intentionally produced, but it did not serve any useful purpose. I, therefore, think that this specimen has to be considered as a figure stone representing the profile line of a human face.

NOTES ON MARSUPIALIAN ANATOMY.

1. ON THE CONDITION OF THE MEDIAN VAGINAL SEPTUM IN THE TRICHOSURIDÆ.

Pl. XXI.

By T. Thomson Flynn, B.Sc.
Professor of Biology, University of Tasmania.

(Read October 9, 1911.)

There has been at various times some little discussion on the state of the median vaginal septum in various members of the genus Trichosurus; up to the present however, the only member of the group which seems to have been examined is the common phalanger Trichosurus vulpecula. Brass (1) in 1880 described the organs of this animal, and in his specimen found the two median cul-de-sacs separated from one another by a complete partition. In 1899 Forbes (2), in a foot note on a communication on the anatomy of the Koala, speaks of the median vaginal apparatus as a "common vaginal chamber formed by the coalescence and fusion of the two diverticula present in Phascolomys and Phascolarctos."

In 1900 the question was finally decided for T. vulpecula by Hill (3), who, as a result of his own observations on the genital organs of both virgin animals and those which had given birth to young, conclusively showed that in the former the septum was complete, in the latter it was incomplete, causing a more or less complete coalescence of the two cul-de-sacs, and that this condition of incompleteness of the septum arose almost certainly as a result of and most probably during the first act of parturition. Under these circumstances, the incomplete nature of the septum in

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1. Brass A. "Beiträge zur Kenntniss des weiblichen Urogenital systems der Marsupialen." Leipzig, 1880.

2. Forbes, W.A. "On some points in the Anatomy of the Koala (Phascolarctos cinereus)." P.Z.S., 1881.

3. Hill, J.P. "Contributions to the Morph. and Dev. of the Fem. Urog. Organs in the Marsupials." I.) P.L.S., N.S.W., 1899.

a specimen of T. vulpecula would be sufficient evidence of the fact that the animal had borne young. In spite of this, Van den Broek (4), in his work on the anatomy of the female genital organs in the Marsupials, in a table given on page 277, mentions a specimen of T. vulpecula with such an incomplete septum, and yet queries the possibility of the animal having borne young. On page 274 the same author says:—"Doch ist diese Unterbrechung des Septum nicht ein bestimmtes morphologisches Merkmal gewisser Geschlechter von Beuteltieren, denn bei demselben Geschlechte findet man bei der einen Art die Scheidewand vollständig, bei der andern Art unvollständig. So fand ich selber bei Didelphys folgendes. Bei einem Didelphys marsupialis mit Jungen im Beutel war das Septum inkomplet, bei einer anderen kleineren Didelphysart, ebenfalls mit Jungen im Beutel war es komplet; bei einem 6 cm. grossem Beuteljungen von Didelphys cancrivora bestand eine vollständige Scheidewand. Doch selbst bei verschieden en Individuen derselben Art findet man das eine Mal ein unterbrochen-es Septum, das andere Mal ein kompletes."

The above statement has some significance, in view of the condition of this portion of the female genital system in Trichosurus caninus, a specimen of which I recently received, through the kindness of Professor Welsh, of Sydney. It had in its pouch a mammary foetus measuring approximately from snout to root of tail along the dorsal curvature, 13 cm. (As it reached me minus its head only approximate measurement can be given).

On examining the organs of the adult animal I expected to find the septum broken as a result of parturition, but was considerably surprised to find it quite entire. (Pl. XXI., Fig. 1.) Recently, I have had the opportunity of examining the female organs of Trichosurus vulpecula var. fuliginosus, the Tasmanian "black opossum." The median vaginal apparatus of this animal agrees with that of the common Phalanger, the median septum being thin, often transparent, and perforated by a larger or smaller opening after parturition. In extreme cases the two cul-de-sacs have quite fused, the septum being only represented by extremely minute rudiments. For the purposes of comparison the median vaginal apparatus is shown in fig. 2. It will be seen that there are other points of difference in the two figures, besides the condition of the septum. Thus

4. A. J. P. v. d. Broek. "Untersuchungen über die weiblichen Geschlechtsorgane der Beuteltiere." Petrus Camper. Dl III, Afl. 2.

we have in T. caninus the internal lining raised into a number of large, rounded folds, separated by deep grooves, the whole having a reticulate appearance. In T. vulpecula, on the other hand, the reticulate appearance is confined to the area around the os uteri, and even in this region is not very conspicuous, the remainder of the cul-de-sac being occupied by a series of slightly raised long, narrow folds. The presence of the strong septum as well as the other points mentioned give the median vaginal apparatus in T. caninus an appearance of strength absent in other members of the genus.

A study of the table given by Van den Broek (5) summarising the existing knowledge of the changes occurring in the median vaginal apparatus consequent on parturition, brings strongly forward the general rule that where the young are borne through a direct median passage, the septum between the two cul-de-sacs is more or less incomplete. A possible exception to this is Sarcophilus (6), but it is as yet doubtful whether the young are here not borne through the lateral canals, although the main evidence points to the median passage as their means of exit. At first, as stated in his work on the anatomy of the female organs in Perameles, Hill (7) believed that the young of Trichosurus were borne through the lateral canals, "here comparatively short and simple in their course," but later he communicated the discovery of a pseudo-vaginal passage in that genus similar to that in Perameles and Dasyurus viverrinus (8). These points may be summarised as follows:—

(1.) Marsupials which possess in the virgin completely separated median vaginal cul-de-sacs. and in which, after parturition, these cul-de-sacs are in communication, bear, so far as is known, their young through a direct median passage.

(2.) In some genera of marsupials (e.g., Didelphys and Trichosurus) one species may still have an entire septum after parturition, while another may not.

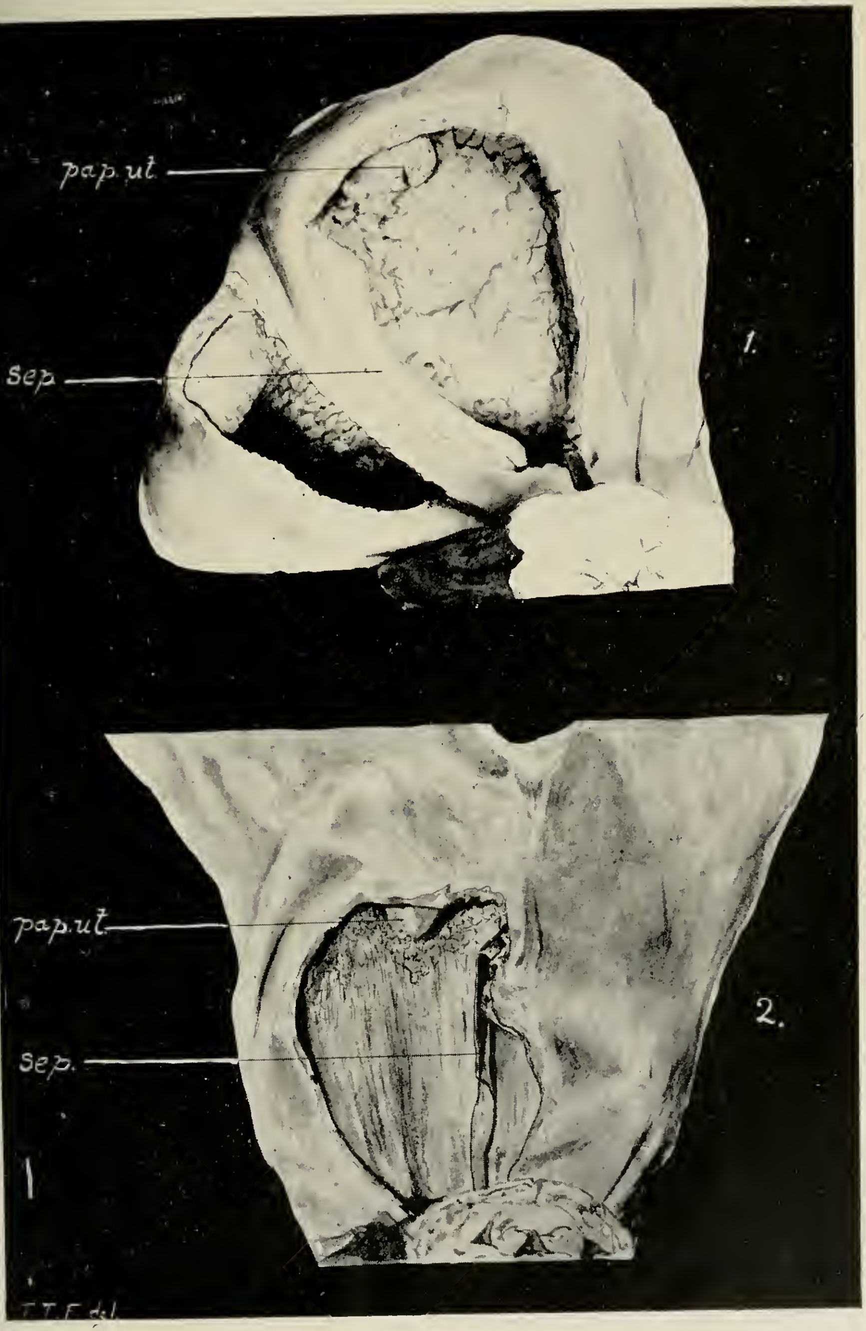
Hence, it is possible that in a genus of marsupials in

5. Loc. cit., p. 276-278.

6. Flynn, T. T. "Contributions to a knowledge of the Anat. and Dev. of the Marsupialia." I. P.L.S., N.S.W., 1910.

7. Loc. cit.

8. Hill, J.P. "Contribution to the Morph. and Dev. of the Fem. Urog. Organs in the Marsupialia." II.-V. P.L.S., N.S.W., 1900, p. 5 6.


pap. ut.
sep.
1.
pap. ut.
sep.
2.
T.T.F. del.

one member of a genus parturition may take place through the lateral canals, while in another member it may not.

EXPLANATION OF FIGURES.

Fig. 1.—Median vaginal apparatus of Trichosurus caninus. The two cul-de-sacs have been opened from the ventral side.

Fig. 2.—Median vaginal apparatus of Trichosurus vulpecula var. fuliginosus, opened from the ventral side.

Pap. ut.—Uterine papilla.

Sep.—Septum between cul-de-sacs.

THE OCCURRENCE OF GIGANTIC MARSUPIALS IN TASMANIA.

By Fritz Noetling, M.A., Ph.D.

(Read November 14, 1911.)

It had hitherto been generally believed that the gigantic marsupials were restricted to the continent of Australia, and did not occur in Tasmania. Jack and Etheridge (1) mention their wide distribution on the continent, and Professor Stirling (2) is of the opinion "that this great marsupial appears to have had an immense range, and to have probably wandered over the whole Continent of Australia." R. M. Johnston (3), who is better acquainted with the geology of Tasmania than anybody else, states that "in Tasmania no remains of the extinct marsupials, such as Diprotodon, Nototherium, and Thylacoleo, have as yet been found either in the ossiferous cavern breccias or in the older alluvial beds."

It seems rather strange that nobody took the view that remains of such animals ought also to occur in Tasmania. Howitt (4) had already, in 1898, expressed the opinion that Tasmania was connected with the mainland in geologically recent times, and Hedley (5), in 1903, holds the same view. Jack and Etheridge had, in 1892, shown that Diprotodon existed in Queensland in post-pliocene times, and as, according to all writers, this animal roamed all over the continent, it would appear very remarkable that it should have avoided the south-east corner of Australia, viz., Tasmania, when during that time the present island formed still a part of the continent.

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(1) Geology and Palæontology of Queensland and New Guinea, 1892, page 668.

(2) Fossil remains of Lake Callabona. Mem. Roy. Soc. South Aust., 1900, Vol. I., Pt. II.

(3) Geology of Tasmania, page 325.

(4) On the Origin of the Aborigines of Tasmania and Australia. Report on the seventh meeting of the Australasian Association for the Advancement of Science, Sydney, 1898, page 723-758.

(5) The effect of the Bassian Isthmus upon the existing marine fauna. A study in ancient geography. Proceed. Linn. Soc. of N.S.W., 1903, Pt. IV., Pag. 876-883.

Consequently, the discovery of remains of a gigantic marsupial in Tasmania should not have created the general surprise they did. When, in 1910, the news that bones of a gigantic marsupial had been discovered in the Mowbray swamp, near Smithton, became known, the discovery was at first somewhat discredited. However, confirmation soon came, and the remains were purchased by the Launceston Museum from their discoverer, Mr. Lovett. Mr. Scott, the Curator of the Museum, has since described them under the name of Nototherium tasmaniense (6), but I am somewhat doubtful whether a new species is justified. After having seen the wonderful remains of Diprotodon australis in the Adelaide Museum, I think a more careful comparison of the Tasmanian remains with those from South Australia should have been made before a new species was created. More weighty reasons for the establishment of a fourth species, in addition to the three already known, should have been advanced, than those given by Mr. Scott; in fact, if we consider that both N. inerme Owen and N. dunense De Vis. differ so little from Nototherium Mitchelli Owen that they are probably nothing more than varieties, the characteristics on which this fourth species, N. tasmaniense, is established. are altogether unsatisfactory. However that may be, it matters very little whether Nototherium tasmaniense is identical with N. Mitchelli or not, or if even the generic determination be uncertain. The main fact that the remains of a gigantic marsupial, which belongs either to Nototherium or Diprotodon, have been found in Tasmania, is indisputable.

Early in 1911 I had an opportunity of visiting Smithton, and, thanks to the kindness of Councillor S. Moore of Smithton, I was able to examine the exact spot where the remains were found. The Mowbray Swamp is about 1-2 miles west of Smithton, and, apparently, fills up a shallow depression of the surface. Probably it represents an old river course, which once had an outlet to the sea, but which subsequently became blocked up by sand. At present the "swamp" is divided from the sea by a narrow strip of sand, on which low dunes are rising towards the coast. There is hardly any natural fall from the swamp towards the sea, and the vegetabilic mould, or, better said, peat, which fills up the depression, is completely waterlogged. The thickness of the peat layer is not exactly known yet, but along the edge of the swamp, where drainage work has been intensive, it reaches about 25ft. to 30ft. To me it seems very probable that the deepest point of the firm bottom,

(6) The Tasmanian Naturalist, 1911.

on which the peat rests, is below sea level, and this would account in some way for the sluggishness of the fall. When drained this peat forms a rich agricultural soil, and it is during the course of such drainage works that the remains were discovered on Mr. Lovett's farm.

The fertility of the soil can be judged by the fine timber that grows on the swamp, but a great deal of clearing and, above all. draining will have to be done before the soil can be used to its full extent.

The clearing has revealed a very peculiar feature of the surface. Everywhere, where the bush is lighter, small conical mounds, rising abruptly from the surface, will be noticed. Generally these are not of great height; the highest I saw was about 30ft., but the majority are not more than 10ft. high. These mounds look like little volcanoes, and the likeness appears greater still, as on the top there is a crater-like opening filled with water, in which gas bubbles constantly rise. The water flows over the edge, down the slope, and the immediate neighbourhood of such a mound is particularly swampy. In order to prevent this, and to regulate the outflow, several of these mounds have been opened by a trench, extending right into the centre. This trench has not only revealed the existence of a pipe, reaching from the bottom to the top of the mound, but it also permitted its structure to be studied We see that the mounds consist of body of peat, which rises above the general surface, and dips from the centre in all directions. On the surface of this cone layers of calcareous tuffa are observed, while layers of fine mud, containing numerous shells of fresh water mollusca, are seen to be interstratified with the peat.

There is only one explanation to account for the existence of these cones, the vegetabilic matter in the depth of the swamp is still decaying, and as the result of the decomposition gases are liberated. These gases rise and lift the peat till it assumes the shape of a bubble, which eventually bursts. The gases have now a free exit, but the pressure is still sufficient to make the water rise to the top, where it flows over.

It was, of course, of some importance to ascertain the nature of the gas rising in the water, whether it be sulphuretted hydrogen, a hydro-carbon, or carbonic acid. Sulphuretted hydrogen is easily detected by its unpleasant smell. The entire absence of such smell proved that the gas could not be sulphuretted hydrogen (7). The gas was, apparently, not combustible, as a lighted match applied to the bubbles had no effect, but when I put it into the bottom of the pipe of a cone which had been opened by a cut, just over the point where the bubbles rose, it went out immediately. This proves, in my opinion, that the gas must be carbonic acid, which, being heavier than air, collected at the bottom of the narrow pipe, and slowly flowed out along the trench. The water is quite cold; unfortunately, I had no thermometer to measure its temperature, but even in January it was cool. This proves that it cannot rise from a great depth. To all appearances the water is of good quality; it has no taste whatsoever, and is of crystal clearness. However, when exposed to the air for some time, scuds of brownish colour commence to form. These indicate a considerable percentage of iron, which becomes oxydised when the water is exposed to the air. The presence of some iron salt is proved by another observation. A piece of peat which was taken from the ditch where the bones were found became, after being dry, completely covered with whitish crystals. These crystals proved by their sweetish, astringent taste to be sulphate of iron.

Considering the large quantity of the efflorescence, sulphate of iron must be present in considerable quantities, and this accounts for the ferruginous scuds forming when the water is exposed to the oxydising influence of the air.

The water must also contain a considerable amount of carbonate of lime, as proved by the deposit of tuffa.

Whether water that, though, apparently, perfectly tasteless and quite clear, contains so large a percentage of iron and lime is a good drinking water, and not injurious to health, remains to be seen.

The peat is composed of rotten vegetabilic matter, in which trunks of large trees are irregularly embedded. It appears that most of the vegetabilic matter is too decomposed to allow for a determination, yet trunks of fern trees could be distinctly recognised. A great deal of inorganic substance, probably sand and clay, is mixed with the organic matter, and when dug it represents a black substance, of, rather, heavy weight, which might be used as fuel.

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(7) Mr. Moore has, however, informed me that there are certain springs which emit such a smell.

Imbedded in the peat are irregular streaks and layers of a soft calcareous mud, full of the shells of fresh water molluscs.

At Mr. Lovett's farm a trench of about 10ft. depth had been cut, and partly imbedded in such a shell layer were found the bones of, apparently, two individuals, a larger and a smaller one. Unfortunately, Mr. Lovett had not noted the position of the bones when they were found, but there is no doubt that they were not washed together by running water; at least, those of the bigger animal belonged to one individual. This fact is so far of importance, as it proves that the animal must have perished where it was subsequently found. If this view is correct, there is every probability of finding further remains, because it is not very likely that the specimen found was the only one that existed.

I collected a large number of the shells, because their determination must be of the greatest importance in fixing the age of the strata in which the remains of this marsupial were found. As I have been able to compare the specimens with the types in the Tasmanian Museum, Hobart, the specific identification is correct. I found :—

(1) Vitrina (Paryphanta) Milligani, Pfeiff.
(2) Helix (Flammulina) Hamiltoni, Cox.
(3) Bulimus (Caryodes) Dusfresnii, Leach (eggs only).
(4) Succinea australis Fer.
(5) Physa tasmanica, Ten. Woods.
(6) Bithynella nigra, Quoy and Gaimard spec.
(7) Cyclas tasmanica, Ten. Woods.
(8) Pisidium tasmanicum, Ten. Woods.
(9) Ostracodum gen. et spec. indet.

The occurrence of Physa tasmanica, some 8ft. from the surface, associated with the remains of a gigantic marsupial, is of particular interest. Tennison Woods (8), who first described this species, states that it is so similar to Physa fontinalis, of Europe, that it is almost impossible to distinguish the two species. He was, therefore, at first inclined to consider Physa tasmanica as an imported variety of Physa fontinalis. The discovery of Physa tasmanica in the beds of the Mowbray Swamp has now conclusively proved that it is an indigenous, and not an imported,

(8) Pap. and Proceed. Roy. Soc. Tasmania, 1875, pag. 74.

species. If it were imported the gigantic marsupials would have existed a considerable time after 1803, because we must assume that it took some time before Physa fontinalis spread from the southern part of the island to its north-western corner. I do not think that anybody would accept such an absurd theory, and we can, therefore, take it as granted that Physa tasmanica is autochthonous.

On the other hand, if Physa tasmanica and Physa fontinalis are really so similar that they are hardly discernible the problem becomes very interesting. I hardly think that anybody will assume that Physa fontinalis migrated from the temperate zone of Europe, through the tropics of Asia into temperate Tasmania, without the slightest morphological change; in fact, if such a migration actually took place it would be more probable that the Tasmanian form would be widely different from its European ancestor—unless we believe that having passed the tropics, and reached the temperate zone, it assumed again the shape and form of that, living in the temperate zone on the northern hemisphere.

If such a theory were possible, or even probable, it would revolutionise our entire view of the geographical distribution of animals; but I think that before it could even be seriously discussed further proof would be required.

I rather feel inclined to think that the similarity of the two species represents one of the numerous examples of convergency of form, developing under the influence of the same climatic conditions. After the disappearance of the glaciers in Tasmania, certain molluscs developed the same tendency towards a certain form, as did similar molluscs in Europe under similar climatic conditions. This is, however, a problem which is outside the province of this paper.

With the exception of Bulimus (Caryodes) Dufresnii, the largest land snail of Tasmania, whose eggs could unmistakably be recognised, and Helix (Flammulina) Hamiltoni all other species occur in enormous numbers. All these species live at present in Tasmania, and range amongst the commonest species. With the exception of the three species, all the others are fresh water molluscs, but Vitrina Millegani, Helix Hamiltoni, and Bulimus Dufresnii like a moist, cool habitat.

We must, therefore, assume that the beds in which Nototherium tasmaniense was found are of quite a recent age; in other words, that the gigantic marsupials must have lived in Tasmania up to quite recent times. The simultaneous occurrence of higher organised mammals, which have become extinct, and lower organised mollusca, which are still flourishing, is of the greatest theoretical importance. It proves conclusively that lower organised animals are much less susceptible to changes than higher organised ones. The lower organism is, apparently, better fitted to adapt itself to changes than the higher one; changes which resulted in the complete disappearance of the gigantic marsupials had not the slightest effect on the molluscs. On the other hand, this simultaneous occurrence of extinct mammals and living molluscs conclusively proves that in determining the age of certain beds from fossils alone, we must be strictly guided by one class only. It has been proved over and over again that the results as to the age of certain beds derived from the study of, say, the echinoderms, are somewhat at variance with those derived from the study of molluscs; and, again, those derived from the study of pelecypoda and gastropoda differ from those obtained from the study of cephalopoda. Generally, the lower classes are indicative of a somewhat older age than the higher classes, because they are more persistent than the latter.

The present land and fresh-water molluscan fauna must, therefore, have already been in existance when the gigantic marsupials roamed over the Australian Continent, and when Tasmania was still connected with the mainland. It would, however, be completely wrong to argue that it must be of great age, because these giants have since died out.

Stratigraphically, the beds in the Mowbray Swamp are also of very recent age; in fact, they were formed when the present physiographical features of Tasmania had been practically formed, except that in all probability the elevation of the swamp above sea level was then higher than it is now.

It almost seems significant that the remains of the gigantic marsupials were discovered in such a part of Tasmania that probably was connected longer with the mainland than others, but this is, perhaps, merely accidental. For the present we are unable to say whether the gigantic marsupials had a wider distribution in Tasmania, or whether they were restricted to the northern part. So far no remains have been found at other localities, and R. M. Johnston, the indefatigable geological explorer of Tasmania, would have most probably discovered them had they existed. However, this does not prove that they do not exist; the numerous silted-up lakes and tarns of the highlands of Tasmania may still contain many a surprise.

However, for the present we are forced to assume that the distribution of the gigantic marsupials in Tasmania was limited, and restricted to the northern parts; in fact, it almost appears as if they had just arrived from the mainland, when they commenced to die out, without finding time to spread further. It is certain that the migration from the mainland must have taken place at a time when Tasmania was still connected with the mainland; in fact, the occurrence of the gigantic marsupials is a further proof—if such was required—of the existence of a land bridge between Tasmania and Australia. It is absolutely impossible to understand how the gigantic marsupials could have otherwise reached Tasmania, unless we assume that they originated spontaneously in Australia and Tasmania.

Further, unless we assume that the gigantic marsupials existed in Tasmania a long time after the separation of the island from the continent, we must conclude that this event took place in very recent times. But, what is more, the gigantic marsupials must have already been extinct before the arrival of the Tasmanian aborigines, because there is not a tittle of proof that they were known to them. Thus the separation of Tasmania and Australia must have taken place so recently that the theory advanced by me in a previous paper is fully borne out (9).

The sequence of events in the most modern geological times of Tasmania may be summarised as follows:—

Later Post Glacial Period.	Fresh water and land Molluscan Fauna the same as at present. Flora the same as at present.	(*a*) Last stage of the isthmus between Tasmania and Australia.	1. Immigration of the gigantic Marsupials (?) 2. Extinction of the gigantic Marsupials. 3. Immigration of the Tasmanian Aborigines about 7,000 years ago. (Probably commencing rise of temperature.)
		(*b*) Destruction of the isthmus between Tasmania and Australia.	Subsidence of the surface, probably accompanied by volcanic eruptions along the north coast of Tasmania. (Younger volcanic period (?)
Present Period.		(*c*) Complete replacement of the old isthmus by the sea.	1. Immigration of the Aryan race, 1803. 2. Extinction of the Tasmanian Aborigines, 1878. 3. Exclusive population of Aryan origin in Tasmania since 1878.

(9) The Antiquity of Man in Tasmania. Pap. and Proceed. Roy. Soc., 1910. See also Noetling, Das Alter der menschl. Rasse in Tasmanien. N J.M.G P., 1911. Beilageband XXXI., page 303.

The fact that in Tasmania the gigantic marsupials occur together with the recent molluscan fauna and the recent flora is of the greatest importance with regard to the determination of the age of these animals in Australia.

On the authority of the late Professor Tate, the beds which contained the remains of Diprotodon australis in Lake Callabona (S.A.) were declared to be of pliocene age. I have not been able to ascertain how Tate arrived at his view of the age. Professor Stirling found only one species of mollusca, viz., Potamopyrgus spec., and some specimens of plant remains, two of which could be identified with living species. I almost presume that for no other reason than because they contained the remains of extinct animals these beds were considered as Pliocene, as they were undoubtedly younger than those Tate considered as Eocene and Miocene. Tate's views as to the age of the tertiary beds in Australia are, however, no longer tenable. The Eocene disappears entirely, and most of the strata he thought to be of Eocene age have to be considered as Miocene; in fact, if not of still younger age. I cannot enter here into the discussion of this question; all I can say is that Tate's view of the pliocene age of the Diprotodon beds is not supported by unshakable palæontological evidence. Whatever their age may be, in Tasmania the Nototherium occurs in beds that, without doubt, are of post-glacial age.

In view of the above fact, it is remarkable to note that as far back as 1892 Jack and Etheridge (10) have expressed a similar view with regard to the remains of Diprotodon in Queensland. On page 608 the authors say:—"On the other hand, in Queensland there is no evidence that they went back to the tertiary epoch, although it is quite possible that they did. Such direct evidence as we have, consisting of the association of the mammalia with fresh water and land shells of species still living, would lead to the conclusion that the former (viz., the gigantic marsupialia) were in the Queensland area, confined to the post-tertiary deposits."

If the authors, however, state that "There is abundant evidence to show that in the southern colonies the extinct mammalia existed in pliocene times," I am afraid that they were somewhat influenced by Tate's views. I do not deny that it is possible that the gigantic marsupials first appeared in pliocene beds, but the evidence on the strength

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(10) The Geology and Palæontology of Queensland and New Guinea. 1892, pag. 608.

of which these beds were declared to be Pliocene is not very convincing. On the whole, I do not think it very probable that these giants lived all through the Pliocene, the Pleistocene, and the late post-glacial epoch, up to times which, according to the European standard, might be termed præhistorical. I have come to the conclusion that the gigantic marsupials, in particular Diprotodon and Nototherium, existed during the cold Pleistocene period, when enormous glaciers covered a large portion of Tasmania and the Australian mountains. The giant marsupials were not exactly Arctic animals, but they preferred a cool, pluviose climate to a warm and dry one. When with the general rise of temperature the glaciers melted away the giant marsupials probably followed the receding glaciers, and they kept longest in those parts where the glaciers remained for the longest time, viz., in Tasmania. With the complete disappearance of the glaciers, the giant marsupials also became extinct.

It is noteworthy that Jack and Etheridgé hold somewhat similar views. They assume that the changes of climate which followed the subsidence of the land were sufficiently great to have a disastrous effect on the now extinct fauna (11). It matters very little whether we attribute the changes of the climate to the disappearance of the glaciers or the subsidence of the land, which latter, in my opinion, took place in post-glacial times. Messrs. Jack and Etheridge, as well as myself, concur in the view that the extinction of the giant marsupials was the result of climatic changes; only that I go a little further, and assume that the gigantic marsupials were the representatives of the glacial period in Australia, in the same way as Rhinoceros tychorhini (the woolly Rhino) and Elephas primigenius (the mammoth) characterised the pleistocene glacial period in Europe.

(11) L.C. pag. 609.

NOTES ON DUTERRAU'S "RECONCILIATION" PICTURE, Pl. XXII.

By Fritz Noetling, M.A., Ph.D.

Read November 14, 1911.

In an appendix to my paper on the lughrana, etc., read before the Society on July 10, I discussed the evidence afforded by the Duterrau engravings (1). I came to the conclusion that all the engravings ought to be reversed, because, by the mistake of the engraver, they were transferred on the copper plate as originally drawn. Naturally the prints became reversed, and the man making the spear appeared to hold the tero-watta in his left and not in his right. Just when the final proof of my paper had been received Mr. Beattie kindly informed me that he had seen the original oil painting, of what I termed No. 1 engraving (Pl. XIII.), at the house of the Misses Cleburn, and that this oil painting fully confirmed my conjecture. I had just time to add a hasty postscript to my paper, stating that my views were correct, but I think it will be useful to make a few more remarks.

From the inscription on it we know that the engraving was made in 1835. Inasmuch as the oil painting is somewhat more explicit, I feel inclined to think that it is of a later date. It is very probable that Duterrau made at first several sketches before he finally made his selection. One of these sketches he etched and published in 1835, and subsequently he painted the picture under discussion. All we can, therefore, say is, that in all probability it was painted after 1835, perhaps towards the end of the thirties, or early in the forties.

Notwithstanding its great shortcomings, the picture is of considerable value as a historical document. It measures about 6 x 4 feet, and on the back is written: "The Reconciliation: Sketch of a national picture measuring 14 x 9 feet." This "national" picture has actually been painted, but its whereabouts are now unknown. Perhaps these notes may help to discover it. The designation of this picture as a "national" picture is a curious illustra-

(1) See Antea pag. 93.

DUTERRAU'S RECONCILIATION PICTURE.

tion of the sentiments prevailing in 1835. Everybody presumed that henceforth black and white, Europeans and Tasmanian aborigines, would live as brothers and sisters in one united "nation." To-day we can only smile at the simplicity that thought it possible that one nation could arise from the union of Aryans and one of the lowest races of non-Aryan origin that has ever been known. We may be thankful, in the interest of those that inhabit present Tasmania, that these views, however lofty they may appear to the philanthropic idealist, were never realised. It is regrettable that the intensely interesting Tasmanian race took such a sad and untimely end, but in the interest of the purity of the white race it is perhaps better so.

A comparison of Pl. XIII. (the original engraving) and Pl. XXII. (the sketch in oil) shows marked differences, though on the whole they represent the same arrangement of persons. Both pictures prove that they are compositions, made in the artist's studio, from sketches he made after life, either in Robinson's house, or in his own in Hobart (2). The engraving contains 11 figures. viz., three women and seven men (not including Robinson), and three dogs, while the oil sketch contains 15 figures, viz., five women and nine men (not including Robinson), three dogs, and a kangaroo. The additional figures appear in the background only. The main group of nine figures (including Robinson), viz., three women and five men, is, however, exactly the same in both. Robinson half turned to the right, grasps the Tasmanian's right with his right hand, while the left hand is slightly raised in a teaching attitude. The features of the aborigines are unquestionably considerably idealised. The man, whose hand Robinson is grasping, shows an almost noble profile, and the three females are by no means bad-looking. It is therefore pretty certain that the features are not realistic. The same probably applies to the colour of skin, which in the oil sketch appears to be rather a pleasing dark grey. I do not think that I need to explain the meaning of the picture, which speaks for itself, except that in the "sketch" the man on the extreme right appears in a more natural position, his left hand holding the shell necklace, while in the engraving he holds a portion of a spear. It is noteworthy, however, that apparently the females are convinced, and ready to accept the new doctrine. The men, with one exception only, are either indifferent or directly hostile, attempting to restrain the females. It would be interesting to know whether this

(2) This renders it very probable that the "National" picture deviates as much from the "Sketch" as the latter does from the 1835 engraving.

conception of the reconciliation arose in the artist's mind only, or whether the females did play the role attributed to them in the oil sketch.

We will now examine those features of the painting that are of interest with regard to the aborigines.

It is pretty certain that the loin cloth, consisting of a kangaroo skin (with the fur inside) is a concession to the public taste of 1835. We know for certain that the aborigines did not cover their sexual organs.

Ornaments are worn by both sexes: these consist of shell necklaces, and a human lower jawbone, suspended by a string, and worn round the neck. It seems remarkable that only the men wear the jawbone, while the women wear only pearl necklaces. This may be accidental only, the more so because in the engraving not one of the figures wears a neck ornament. These are apparently subsequent additions to the figures of the oil sketch and this would somewhat reduce their ethnological value, because it would show that the artist painted them, not as he had seen them, but as he thought that they would make a pleasing effect.

Only three men wear the customary head-dress, i.e., the ringlets produced by rubbing a mixture of red ochre and fat into the hair. It is noteworthy that these three men show the nearest approach to correct features of the aborigines, in particular the flat nose.

All the other men wear short curly hair. The question may well arise, did the aborigines, when brought to Hobart, lose the habit of smearing the hair with ochre, probably because they had none, or did they, in their free state, only occasionally resort to this practice, while generally the hair was left in its natural state? It is pretty certain that once the hair was well rubbed with the mixture of red ochre and fat, which hardened in time, it could not be removed unless the head was shaved. Now such a head-dress must afford a good shelter to vermin, and it is perhaps probable that, if worried too much by it, the aborigine had his head shaved, and not until it had reached a certain length it was again treated with the ochre mixture. The men wearing the natural hair would therefore represent individuals whose hair is growing, but had not reached the sufficient length for the ornamental head-dress. If this view were correct, the remarkable sentence, "He shaves his hair with a flint," would perhaps be not so wrong after all, because the men did shave their hair occasionally. Unfortunately, we are unable to decide this question for ever. We are equally unable to say whether the operation of rubbing ochre and fat into the hair was a sort of distinction, or a sort of rite, to be performed on certain occasions only. The evidence of the picture only proves that the custom of rubbing a mixture of red ochre and fat into the hair existed, but that apparently the majority of men wore the hair in its natural state.

All the women, except one, have the hair closely cropped; in fact, it may be questioned if even this female is not supposed to have the hair very short; in fact, in the engraving she does not differ from the other women in that respect. The picture therefore fully confirms the statement that the women habitually wore the hair closely cropped. Thus we will never know whether the hair of the Tasmanian females would attain a greater length than that of the males or not. (3)

Nothing need to be said about the spears, except that the man who is scraping one, seated to the left, is holding the tero-watta with his right hand.

One word about the dogs. Two breeds can distinctly be discerned, viz., two collies and a greyhound. Did the aborigines distinguish these two breeds, which are so very different, by different names, or was any breed of dog to them a kaceta?

The place of the fire which burns near the man straightening a spear with his teeth, is in the oil sketch occupied by a kangaroo. As it is not known that the aborigines kept pet animals, except dogs, which were rather their assistants in hunting than pets, the tame kangaroo is probably an introduction by the artist to enliven the group.

I am greatly indebted to the Misses Cleburn, who kindly permitted this oil sketch to be photographed by Mr. Beattie, and the reproduction is published here for the first time.

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(3) I regret to say that hitherto I entirely overlooked a passage in Appendix I. of Ling Roth's "Aborigines of Tasmania." This appendix is apparently an extract from the "Penny Magazine" of June 21, 1834, dealing with the Duterrau Portraits. It says:—"His wife, Truganina, . . stands beside him, with her head shaved, according to custom, by her husband, with a sharp-edged flint." This fully confirms my interpretation of the mysterious sentence: Tugganna pugheranymee trautta. We may further conclude that the noble savage whose hand Robinson grasps, is meant to represent Wooready, his trusted servant, from Bruni Island.

As the above paper practically concludes my investigations on the Tasmanian Aborigines, it will perhaps be advisable to give a list of the literature which has been published on the subject. The older literature has been completely enumerated by Ling Roth, and it is superfluous to reiterate it here. The following list contains only those papers that have appeared in the Society's journal and those published after 1899, the year of publication of the 2nd edition of Ling Roth's "Aborigines of Tasmania."

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NOTES ON MARSUPIALIAN ANATOMY.

II. ON THE FEMALE GENITAL ORGANS OF A VIRGIN SARCOPHILUS SATANICUS.

Pl. XXIII., XXIV., XXV., XXVI., XXVII.

By

T. THOMSON FLYNN, B.Sc.,

Ralston Professor of Biology, University of Tasmania.

In a former communication to the Linnean Society of N.S.W. (10) I described the genital apparatus in a female specimen of Sarcophilus satanicus which had borne young, and in that paper drew attention to various phenomena, especially in connection with the median vaginal apparatus, which could only be satisfactorily explained by the examination of similar organs in the virgin animal. In the ordinary course of events, such an undoubted virgin would be difficult to procure, and, even if obtained, would be hard to recognise as such in our present knowledge of these organs in this genus. Nevertheless, since my last paper was written, through the kindness of Mrs. Roberts, C.M.Z.S., of Hobart, such a virgin has been placed at my disposal. Mrs. Roberts has had for some time past in her gardens at Beaumaris a number of Tasmanian "devils," among which was the specimen in question. It came to the gardens as a baby attached to the teat about August, 1909, and had lived ever since in a cage with its mother and an old male. Last March quarrels occurred through the young female resenting the attentions of the male, with the result that she was severely handled, and had to be removed to another cage. Soon after she seemed to rally, but after a little while sank and died. A post-mortem showed that external injuries were not the sole cause of death, for the internal organs were very much diseased.

I wish to place on record my grateful appreciation of the kindness of Mrs. Roberts in putting this specimen at my disposal.

My best thanks are due to the following gentlemen for the loan of literature:—Professors J. T. Wilson and W. A. Haswell, and Acting Professor Johnston.

I am able to confirm and considerably augment my account of the genital organs in this genus.

I have nothing to add to my previous account of the external form of the female organs, except that in this specimen the Fallopian tubes seem slightly more convoluted than in the former specimen.

STRUCTURE OF THE VIRGIN UTERUS.

Fig. 1 represents a transverse section of the uterus of the virgin. From side to side the whole uterine body is flattened, containing consequently a correspondingly flattened lumen. Each dorsal and ventral wall of the uterus is raised into a rounded fold, separated from the lateral uterine wall by deep grooves. With the exception of these grooves the inner surface of the uterus is quite smooth.

A somewhat enlarged section of the uterus is shown in Fig. 2. The inner covering of the uterine wall is a thin epithelium consisting partly of cells with rounded nuclei partly of columnar cells mixed indiscriminately. It measures in thickness about .012 mms. The uterine glands have an epithelium continuous with the inner uterine epithelium, but no columnar cells are present in it. The glands are slightly convoluted tubes with an average diameter of .009 mms. The mucosa is well defined and sharply marked off from the muscularis. It contains large numbers of uterine glands, but in no case did I find these to penetrate the muscularis. The tissue of the mucosa is of the nature of a loose network, the nuclei of whose cells become aggregated for the most part round the uterine glands, the vessels, and just below the uterine epithelium. The vessels which supply the mucosa, after piercing the muscularis, lie for the most part in the lower portion of this layer, although fine capillary branches supply the inner portion of the mucosa. For the most part the vessels have extremely thin and ill defined walls. The average thickness of the mucosa is .71 mms.

The muscularis is a definite layer, .032 mms in average thickness, consisting of circularly running plain fibres only.

It is interrupted only by the incoming blood vessels. There is a well marked serosa continuous with the broad ligament.

A comparison with the uterus of a female with pouch young (described in my last paper) shows that the chief difference in the latter is the greater growth of the mucosa, it being about one and a half times as thick. This increase in growth takes place for the most part on the lateral walls of the uterus, forming here two large cushion-shape masses. Other points noticeable in the multiparous uterus are:—The inner epithelium is entirely columnar, the mucosa is considerably more vascular, almost the entire space between the uterine glands being taken up by vessels, the uterine glands have increased in number and diameter, the muscularis has increased in thickness, and has become invaded to some extent by uterine glands.

MEDIAN VAGINAL APPARATUS (SINUS VAGINALIS).

The general arrangement and structure of this portion of the female sexual organs of Sarcophilus is worthy of considerable discussion. In my last communication, I ventured the remark that the structure may not be present in the virgin, but may arise at the time of the first pregnancy. I was led to this conclusion by the fact that in the comparatively large and late pouch young described in that communication no sign of the median vaginal apparatus, as such, was present; while in a pouch young of Perameles, a section of whose organs was figured by Hill (5, p. 77) there are indications that the foundation of the median vaginae have already been laid. Van den Broek, again, describes the presence of a well marked median vaginal apparatus in a pouch young of Didelphys, for he says:—"Dagegen fand ich bei Beuteljungen von Didelphys schon sehr fruh den Sinus Vaginalis in einen Entwicklungsgrad, der jenem des erwachsenen Tieres relativ wenig nach steht." this being quite the opposite to what had been already described by Brass. I was also led to the abovementioned conclusion by the peculiar irregular arrangement of the two median vaginal cul-de-sacs with regard to the vaginae, and again by the fact that the anteriorly directed portion in each median vaginal cul-de-sac is quite narrow and canal-like, an occurrence which led me to give it, for the time being, the special name of the median vaginal neck. The presence of these median vaginal necks distinctly marks off the arrangements of the female organs in Sarcophilus from that of any other described marsupial. At the time of my last paper I had not seen Van den Broek's paper on the marsupials (3), and did not know that he had already called that portion of the sinus vaginalis (median vaginal apparatus) into which the uterine necks opened, the fornix. In Sarcophilus the fornix is drawn out on each side into a narrow tube. It had been pointed out (3) that the position of the uterine openings into the median vagina may vary; for example, in the Didelphidae the uterine necks penetrate the median vagina from the anterior side, in Phascolomys they are placed at the antero-lateral corners, in Antechinus apicalis quite laterally, while in the Macropods they enter the median vagina from the dorsal side. In Sarcophilus each uterine neck opens at the apex of a large papilla into each median vaginal neck. This papilla differs from all other marsupials except Phascolomys (3) in having lobed edges. Since this opens into the anterior prolongation of each cul-de-sac, the entry of the uterus is quite from the cranial end, and it can be placed in the same group, therefore, as the Didelphidæ.

Van den Broek, who examined a single specimen of Sarcophilus, but not apparently in detail, says (3, p. 51):—"Dasyurus ursinus und Antechinus apicalis kommen in der Gestalt ihres Sinus vaginalis Phascolomys sehr nahe, doch unterscheiden sich, besonders Antechinus vom Letztgenannten dadurch, dass die Uteri mehr horizontal verlaufen und folglich mehr von der Seite her in den Sinus vaginalis ausmunden." This statement indicates that Van den Broek had not observed the presence of the median vaginal necks in this genus.

Morphologically, the median vagina of the virgin differs from that of the multipara mainly in the fact that the cul-de-sacs are much smaller and considerably shorter. The median vaginal apparatus in Sarcophilus has a number of peculiar features which mark it off from the similar portions of the sexual organs in other marsupials: it is considerably less developed in proportion to the vaginae than is usual, its shape is irregular, it cannot be said to be triangular, as in the Didelphidae, nor tongue-like as in Phascolomys, nor cylindrical as in Macropods. It retains more than any other—having as it does a complete and strong septum containing muscle fibres,—the character of two separate outgrowths approximating, but hardly sufficiently to form a homogeneous structure. Van den Broek has said:—"Man konnte in dieser doppelten Herkunft Anlass finden statt von einem 'Sinus vaginalis' von 'Sinus vaginales' zu sprechen," and this statement applies above all to the condition in Sarcophilus.

MICROSCOPIC STRUCTURE.

Muscularis. — In common with Hill and Van den Broek, I find that, as in the rest of Marsupials, the muscular tissue of the median vaginal apparatus consists of circularly running nonstriate fibres. On this point, however, the latter says (3, p. 60): "Muskelfasern kommen niemals in Septum vor, die. . . Muscularis circularis in der Wand des Sinus umschliesst immer die beiden Hohlraume des Sinus als einen einheitlichen Raum." Hill, however, (5, p. 53) says of the two cul-de-sacs in the virgin Perameles that "they are separated by a common partition wall, and each is surrounded by a delicate layer of circular nonstriate fibres." He refers to Fig. 7, but the presence of these fibres in the septum is more plainly and unmistakably shown in Fig. 10 of a section through the median vaginal cul-de-sac of a specimen with two 17.5 young in the pouch.

In Sarcophilus I find that each sinus is contained in its own portion of circular muscle, which wholly surrounds it, and is, therefore, present in the septum. The presence of these muscle fibres in the septum of both Perameles and Sarcophilus gives the two median vaginal cul-de-sacs an independence which is absent in other marsupials. In Sarcophilus I find that the epithelial lining of the vaginae, median cul-de-sacs, and of the necks is of practically the same character throughout, consisting of an overlying layer of flattened cells which is not at all continuous, below which is a layer one or two cells thick of cells containing rounded nuclei. The epithelium of the urogenital sinus is similar. I have already (10) shown that the posterior ends of the median vaginal cul-de-sacs are embedded in a mass of deeply staining connective tissue, continuous with a central mass of similar tissue passing backwards between the lateral vaginae, and coming into relation with them about the point where the two lateral vaginae meet the urogenital sinus. This rod of connective tissue is similar to that already described by Hill in Perameles (5, p. 54). This mass of tissue is distinctly defined in the specimen under discussion, being first a rhomboidal mass situated between the two inpassing ureters. Later, in company with the two vaginae, it comes to lie just outside the circularis of the urethra as a deeply stained triangular mass. The circularis of the urethra develops an upper portion, which grows over the two vaginae, and encloses them together with the deeply staining tissue. The muscular layer here, then, is in section of a somewhat elliptic shape, crossed by another muscular layer, the dorsal portion of the original circularis of the urethra. In this way two spaces are formed, an upper and smaller containing the vaginae with the intervening deeply staining tissue, and a lower large one containing the urethra. The muscular layer between these two spaces disappears, and so is formed the circularis of the urogenital strand. The vaginae have up to this time each kept its own circular muscle layer. The muscle layer of the vaginae consists each of an outer and inner circular layer portion, with an intervening mass consisting of connecting tissue with a few circular fibres. Just before the vaginae enter the urogenital sinus their muscular layer dwindles away. The two vaginae enter the two dorsal grooves of the urogenital sinus, the presence of which gives the sinus somewhat the shape in section of a V, the limbs of the letter being formed by the grooves just mentioned. The cloaca is covered with a stratified epithelium.

REMAINS OF THE WOLFFIAN DUCT.

In sectioning there was found in the course of the left vagina round the bladder, embedded in its wall, between the inner and outer portions of the circularis, a short, blind discontinuous tube, representing, I take it, the remains of the Wolffian Duct (Fig. 3). It extends through a total length of 450 micra, thirty sections of 15 micra each. Here and there it shows a lumen, at other places it is a solid cell strand. It is thickest in the middle, and tapers off at each end, leaving no trace. It is not found in the similar position in the right vagina, nor in the multiparous specimen, nor have I found it in the pouch young. It is to be regarded as an individual variation. Although remnants of the Wolffian duct have been found persistent in the papilla uteri, it has never, so far as I know, been found in the position above described.

THE CLITORIS.

Extremely little has been done on the clitoris of Marsupials. Those who early described the genital organs in the Marsupialia were content to describe its external form, without paying any attention to its minute structure. Home made the statement that the clitoris in marsupials is comparatively large. Owen is responsible for the assertion that in those genera of marsupials in which the male has a divided penis, the female possesses a correspondingly split clitoris. Brass concurred in this view, but it was shown by Hill not to be the case in Perameles. Van den Broek later showed in detail that a relation exists between the sinus vaginalis (median vaginal apparatus) and the clitoris, in that a split clitoris is found in those marsupials in which the median vaginal cul-de-sacs are completely separated, while in those forms possessing a single median vagina the clitoris is correspondingly simple and undivided. This latter case is characteristic of the Macropods.

As regards the minuter structure of the clitoris, it was first attempted in the case of Perameles by Hill, and was continued by Van den Broek for the genera Phascolarctos, Phascolomys, Halmaturus, Didelphys, and others. As a result, the latter writer was able to deduce some generalities on the structure and insertion of the clitoris in this group. Histologically, he found that all marsupials possessed a median septum clitoridis complete or incomplete, and that the erectile tissue of the clitoris formed either a single or a paired mass. The insertion of the clitoris may take place by means of an intervening septum clitoridis, which gradually disappears cranially, or, as in Didelphys, no such preputium may be present, when the insertion takes place by mere approximation of the ventral wall of the clitoris on the wall of the cloaca, the epithelial septum so formed afterwards disappearing cranially.

In Sarcophilus, I find that the clitoris possesses structurally features of interest which, in the present early state of our knowledge of this organ in marsupials, make it worthy of description.

EXTERNAL FEATURES OF THE CLITORIS.

The organ with its surroundings is shown in Fig. 4.

Looked at from above the clitoris presents two well-marked lateral portions, united towards the apex, and diverging considerably towards the base. The line of union is shown just in front of the apex by a fine, narrow, superficially placed longitudinal groove. Apically, this groove bifurcates so that the clitoris here seems to consist of three lobes, which at the apex are slightly free from one another. The two grooves separating them pass backward and downwards towards the ventral side of the clitoris, and pass forward underneath, being gradually lost. Ventrally, therefore, the clitoris also presents the appearance of being divided into three lobes by two shallow grooves.

The two main portions of the clitoris (looked at from above) become widely separated cranially, and fuse with the ventro-lateral wall of the cloaca. Arising between these from the clitoris floor, and separated from the lateral clitoris folds by somewhat deep grooves, which extend ventro-laterally below the lateral clitoris folds, is a fold which, being caudally weak and of no great height, on being traced in a cranial direction, becomes considerably stronger, but not so strong as the lateral clitoris folds. Sections show that this fold is continuous with the median apical lobe of the clitoris This fold splits anteriorly into two divisions, separated by a deep narrow groove, which, passing forward, loses itself in a very deep pouch, downwards into which also pass the folds enclosing it. Into this pouch two other pairs of folds pass; one pair is continuous with two from the urogenital canal, and themselves secondarily folded, lose themselves in the pouch. Bounding the pouch laterally and passing into it is a pair of transverse folds, one on each side. Each of these arises from about half-way up the lateral side of the urogenital canal, and forms a swellen fold of mucous membrane which almost touches its fellow of the opposite side. The floor of this pouch is much sunk below the general level of the floor of the urogenital canal, and is considerably thinner.

MICROSCOPIC STRUCTURE OF THE CLITORIS.

The following account is based upon the examination in serial sections of 15 micra in thickness, both transversely and horizontally, of the clitoris in the virgin, and in an old adult female with pouch young:—

Immediately behind the apex of the clitoris, the cloacal wall is raised into a median cloacal fold. This is separated from the clitoris by a narrow but deep transverse groove, curved in accordance with the shape of the apex of the clitoris. Histologically this fold is found to be extremely vascular, containing a small proportion of erectile tissue, and is fairly abundantly supplied with nerve tissue. It is partially divided by epithelial septa similar to those of the clitoris.

Sections through the caudal portion of the clitoris a little in advance of the apex show that the organ is here divided into three lobes by two complete septa. Each of these septa is caudally converted into the shallow grooves separating the three free apical portions of the organ. These septa, as will be shown later, correspond to the lateral septa, such as occur, e.g., in Phascolarctos (3, Pl. 4, Fig. 4). This portion of the clitoris is shown in Fig. 5. From each of these main lateral septa passes off into the lateral clitoris lobes, secondary lateral septa, each passing ventro-laterally to end in a slight swelling, making the septum somewhat club-shaped. No such secondary septa occur in the central lobe. In this central portion we find in the mid-line extending upwards a short distance from the ventral clitoris wall a short epithelial septum representing what is left of the original median septum of the clitoris which is found in the pouch young of this genus, and which persists in those marsupials (e.g., Perameles, vide 5, p. 56, Pl. 5, Fig. 9), in which the clitoris remains in the adult in a somewhat primitive condition.

The clitoris wall is seen to be composed of a stratified epithelium continuous with that of the cloaca. This epithelium consists of a basal layer of cells with large nuclei arranged at right angles to the surface, these being overlain by four or five layers of flattened pavement cells.

The lobes of the clitoris are extremely vascular, containing numerous large thin-walled vessels. Where the rudiment of the median septum appears an alteration occurs in the main lateral septa. Each of these consists of two portions—a dorsal portion, and a ventral, the division between being marked by the origin of the secondary lateral septum. A change takes place in such a way that the ventral portion gradually disappears, leaving only the dorsal portion in connection with the secondary lateral septum. For a time the lowest portion of the ventral division persists, but in time also disappears, the clitoris possessing now two diverging septa composed mainly of the secondary lateral septa, which now extend somewhat in length ventro-laterally. Round the ends of these septa the vessels of each lobe enter into communication.

In close proximity to the ends of these septa appear the masses of erectile tissue. These are somewhat more differentiated than is the case in any marsupial as yet described. Each mass is caudally somewhat cylindrical, tapering off to a point, and consists of large thin-walled cells with muscle fibres, the whole surrounded by fibrous tissue. Pursued cranially they become somewhat flattened, and then semi-lunar surrounding the ends of the two septa. Relations have now been established between the ventral clitoris wall and the epithelium of the cloaca. This relation, in the absence of a preputium clitoridis, is extremely simple, and consists of a mere approximation and fusion of the two epithelia, the result being the formation of an epithelial septum, similar to the other septa of the clitoris (Fig. 7).

This fusion commences from the mid-ventral line. This process of formation of relations between the clitoris and cloaca differs from that of any marsupial so far described. As Van den Broek points out in Halmaturus and Phascolarctos, the fusion takes place caudally through the medium of a preputium, which disappears cranially, while in Didelphys, in which there is no preputium, the clitoris wall being placed laterally upon the cloacal epithelium, the former dwindling away, and only leaving the latter, the result being the formation of a median ventral space between clitoris and cloaca, bounded laterally by the lines of combination of the two.

The points where the septa of the clitoris meet the upper surface of that organ are indicated by longitudinal shallow furrows. These now invade the septa, gradually converting them by the diverging of their epithelial walls into deep narrow grooves.

A glance at Fig. 7 will show the internal structure of the clitoris at this height. The organ has lost its superficial trilobed structure, the two lateral lobes having overgrown the central one, which has correspondingly diminished in size. The latter is separated from the former by incomplete septa, which are being converted into grooves. These grooves pass ventro-laterally from the bottom of the single longitudinal groove of the upper surface of the clitoris, this representing the upper portion of the median septum of other marsupials. The ventral portion of that septum remains in the section as evidence of the combination of two lateral portions of the clitoris. For a fair distance on either side of this latter portion of the median septum the approximation of the clitoris on the cloacal wall is evident. Here and there small evanescent septa are formed as ingrowths of the clitoris epithelium. Erectile tissue is present in two curved bands round the ends of the two septa. Outside these latter the vessels are mainly arranged in somewhat horseshoe-shaped masses. Ventral to the vessels on either side of the ventral median septum is abundance of nerve tissue.

As the fusion of the two approximating epithelia continues, the clitoris sinks by degrees further and further into the submucosa of the urogenital canal. As soon as this is complete, absorption of the septum so formed takes place, beginning at the upper end, the last remaining remnants being on the mid-ventral side.

The general clitoris tissue and that of the urogenital canal form now a homogeneous mass, except that the clitoris is immediately recognisable by its vascularity. (Fig. 8).

The erectile tissue here consists in section of two lateral well-marked curved bands, separated in the mid-line by a septum consisting of fibres crossing from the upper to the lower sides of the outer fibrous sheath. This septum is by no means continuous, but is interrupted at intervals to allow the lateral portions of the corpora cavernosa to communicate. (Fig. 9).

The lateral portions of the clitoris, up till now the most important, begin to lose their identity in the urogenital canal, and there is a corresponding increase in growth of the central clitoris fold. The vessels of the lateral portion pass into the main vessel of the central fold. This fold now becomes longitudinally divided by a groove, caudally shallow, but cranially deepening considerably. The two portions of erectile tissue diverge considerably, and sink deep into the wall of the urogenital canal. Anteriorly they are found to end in the bulb-shaped swelling illustrated in Fig. 11.

In sectioning the clitoris of a pouch young of Sarcophilus, I find that apically the organ is completely divided by a septum. The septum followed cranially in sections bifurcates, and thus divides the clitoris into three parts. The lower ends of the two lateral septa so formed become withdrawn, and the arrangement is that shown in Fig. 10. From this the adult structure is developed. The arrangement of middle portion of the adult clitoris (Fig. 7) is directly derivable from that in the pouch young by the conversion of the upper portion of the median septum, and the lateral septa into grooves. The growth of the middle fold so formed, the completion of the lateral septa, and the appearance of secondary lateral septa, lead to the arrangement at the apex of the adult organ, while the further development of the median septum, and after it a groove, in the middle portion, with the dwindling away of the lateral portions, presents us with the arrangement shown in the cranial portion of the adult clitoris (Figs. 5 and 6).

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EXPLANATION OF PLATES.

Fig. 1.—Photomicrograph of a transverse section of the virgin uterus of Sarcophilus.

Fig. 2.—Photomicrograph of the section of uterus a little enlarged.

Fig. 3.—Photomicrograph of the left lateral vagina, showing the remains of the Wolffian duct. The larger open space towards the right of the figure is the vagina, while the much smaller canal towards the left represents the persisting Wolffian duct.

Fig. 4.—Reproduction of a drawing (from photograph) of the clitoris of Sarcophilus and the region surrounding it.

Figs. 5, 6, 7, 8, and 9.—Photomicrographs of transverse sections of the clitoris of Sarcophilus at various heights, from the apex forwards.

Fig. 10.—Photomicrograph of a transverse section of the clitoris of a pouch young of Sarcophilus.

Fig. 11.—Photomicrograph of a horizontal section of the terminal cranial portion of the clitoris in Sarcophilus.

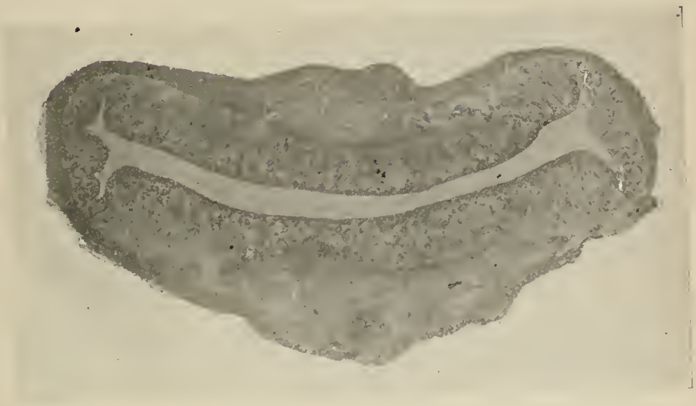

Fig. 1

Fig 2.

Fig. 3

Fig. 5

FIG. 11

FIG. 4

FIG. 6

FIG. 7.

Fig. 8

Fig 9

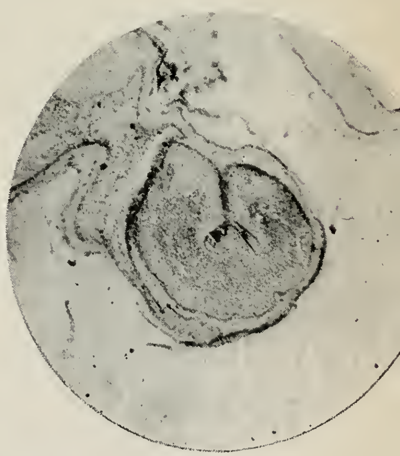

Fig. 10

ANNUAL REPORT

OF THE

ROYAL SOCIETY OF TASMANIA

FOR THE YEAR

1911

TASMANIA:

Printed at "The Mercury" Office, Macquarie Street, Hobart.

ANNUAL GENERAL MEETING

The Annual General Meeting of the Royal Society was held in the Society's Rooms, Museum, on Monday, 18th March, 1912.

In the unavoidable absence of His Excellency, the President, the Hon. G. H. Butler, occupied the chair, and the Annual Reports for 1911 were submitted.

ANNUAL REPORT FOR 1911.

The Council of the Royal Society have the honour to present the Report for 1911 to the Annual General Meeting of the Society.

Eight monthly General Meetings and two Special General Meetings were held during the year. Nine Ordinary Meetings and five Special Meetings of the Council were held during the same period.

Eleven Fellows were elected, while seventeen Fellows left the State, or allowed their membership to lapse.

The total number of the Fellows of the Society was one hundred and thirty-eight, including nine Life Members. The number of Corresponding Members was sixteen. Of these latter Sir Joseph Hooker, the renowned botanist, died within the year.

During the year a Bill to incorporate the Society and to confer upon it powers as to holding property, litigation, and to make and alter Rules, was passed by Parliament.

The Rules of the Society have been revised by a Special Committee, and are now in operation.

A Sub-section on Psychology and Education was added to Rule 51. The Sub-section Biology was brought into action about the middle of the year.

On the representation of the Council of the Society, the Hon. the Minister for Lands and Surveys approved of the recommendation to alter the names of Oyster Bay to Fleurieu Bay, and West Hunter Island to Fleurieu Island, the original names. The Survey Department is considering the right of further alterations, and of placing names where they do not now exist upon the charts.

In order that the Society may allow the use of their room at the Museum for such meetings and gatherings connected with the objects of the Society, as the Society may from time to time allow, the permission of the Trustees to do so was obtained.

The Library received a gift of more than ordinary value in eleven handsome volumes dealing with the Geology and Natural History of the Harriman Alaska Expedition. They were presented by Mr. E. H. Harriman, of New York, and forwarded by favour of the Smithsonian Institute.

During the year thirteen papers were read, as enumerated in the table of contents. One illustrated lecture was delivered. The Reports of Sections are appended.

A Balance-sheet, duly audited, showing the receipts and expenditure for 1911, is attached.

The Report was adopted without amendment.

ELECTION OF MEMBERS OF COUNCIL.

The following members were duly elected:—Hon. G. H. Butler, Dr. Arthur Clarke, Dr. Fritz Noetling, Dr. Gregory Sprott, Prof. T. T. Flynn, Messrs. Samuel Clemes, J. A. Johnson, M.A., E. L. Piesse, B.Sc., Leonard Rodway.

ELECTION OF MEMBERS.

Dr. Brooks, Mrs. E. M. Brooks, Dr. H. N. Butler, Dr. W. L. Crowther, Messrs. R. A. Black, C. J. Clark, J. R. Chapman, C. J. Inglis, and H. J. Spencer, C.E., were elected Members of the Society.

AUDITOR.

Mr. H. W. W. Echlin was reappointed Auditor.

REPORTS OF SECTIONS.

REPORT OF EDUCATION AND PSYCHOLOGY SECTION, 1911.

This Section held its first meeting in the Library on 1st August. There were eleven members present. Mr. S. Clemes was elected Chairman and Mr. J. A. Johnson Secretary of the Section.

Three subsequent meetings were held during the year, there being an average attendance of nine members. All those who attended are directly engaged in the work of Education. It may be pointed out that the subjects discussed are of general interest to all who have the cause of Education at heart, and, therefore, any Member of the Royal Society may follow and take part in the discussions. It is to be hoped that the membership of this Section will not be confined to schoolmasters.

The subjects discussed during the session were the Herbartian theory of "Apperception" and "Many-sided Interest." The discussions were opened by the President, the Secretary, and Mr. S. R. Dickinson, all the members present taking part in the debates.

The following is a complete list of members:—Dr. Purdie, and Messrs. S. Clemes, E. I. Gower, S. R. Dickinson, G. V. Brooks, R. O. M. Miller, W. Clemes, J. A. McElroy, G. A. Gurney, L. Dechaineux, and J. A. Johnson.

REPORT OF BIOLOGICAL SECTION, 1911.

For the year ending December, 1911, the Section consisted of ten members. Three meetings were held. At the first, held in the Royal Society's room, June 30, the scheme of work was set out. Prof. T. T. Flynn was appointed Chairman, and L. Rodway Secretary of the Section. Prof. Flynn gave an inaugural address, pointing out the lines in which the Section might work to the best advantage.

A meeting was held at the Biological Laboratory of the University, September 20, at which Prof. Flynn gave a lecture on the lowest forms of animal life to be found in the neighbourhood of Hobart, illustrating the lecture by diagrams and some forms under the microscope, after which instruction was given in the elementary stages of microscope technique.

On October 16 Mr. H. M. Nicholls gave a lecture at the University Laboratory on the life history of "Venturia inæqualis," causing the disease of apples commonly known as black spot. The lecture was illustrated by lantern and microscope slides. Specimens of the injury done to apple trees by red spider were exhibited by Mr. A. O. Green.

ROYAL SOCIETY OF TASMANIA

RECEIPTS AND EXPENDITURE, ~~1910~~ 1911

GENERAL ACCOUNT

RECEIPTS.

1911.		£ s. d.
Subscriptions—		
96 Fellows at 30/-	£144 0 0	
22 Country at 20/.	21 19 9	
		165 19 9
Sale of Copies of Society's Proceedings		1 19 0
		167 18 9
Balance (debit)		16 16 8
		£184 15 5

EXPENDITURE.

1911.		£ s. d.
Salaries—		
Secretary	50 0 0	
Attendant	6 0 0	
		56 0 0
Papers and Proceedings—		
Printing	84 5 8	
Postage	5 7 11	
		89 13 7
Library—		
Books and Magazines	6 4 0	
Card Index	2 5 0	
Insurance (£2000)	4 10 0	
		12 19 0
Expenses of Meetings—		
Fuel and Gas	3 12 9	
Printing Notices and Postage and Advertising	12 1 11	
Lantern	1 1 0	
		16 15 8
Miscellaneous Expenses—		
Auditor's Fee	1 1 0	
Stamps	2 14 4	
Sundries	4 16 10	
Bank Charge and Cheque Books	0 15 0	
		9 7 2
		£184 15 5

GENERAL ACCOUNT (Continued)

	£	s.	d.		£	s.	d.
1910.				1910.			
Credit Balance, January 1	107	8	3	Ordinary Expenditure	236	17	3
Ordinary Receipts	195	4	9	Petterd Collection—Probate Duty advanced by the Society	121	4	0
Petterd Collection of Minerals—Subscriptions for Cases, &c.	113	0	0	Balance carried forward to 1911	57	11	9
	£415	13	0		£415	13	0
1911.				1911.			
Credit Balance, January 1	57	11	9	Petterd Collection—Subscriptions transferred to the Trustees of the Tasmanian Museum and Art Gallery	113	0	0
Petterd Collection—Probate Duty refunded by the Trustees of the Tasmanian Museum and Art Gallery	121	1	0	Ordinary Receipts and Expenditure—Debit balance (as above)	16	16	8
				Balance carried forward to 1912	48	19	1
	£178	15	9		£178	15	9

THE MORTON-ALLPORT MEMORIAL FUND ACCOUNT

	£	s.	d.		£	s.	d.
Balance in hand, 1st January, 1911	9	7	1				
Interest received from Trustee	9	5	0				
	£18	12	1	Balance in hand, 31st December, 1911	£18	12	1

LIFE MEMBERS' COMPOSITION FEES ACCOUNT

	£	s.	d.		£	s.	d.
Balance in hand, 1st January, 1911	1	0	0	Balance in hand, 31st December, 1911	1	0	0

I have this day examined the Books and Vouchers of the Royal Society of Tasmania for the year 1911 and found them correct and in accordance with the Balance-sheet.

H. W. W. ECHLIN.

15th January, 1912.

E. L. PIESSE, Hon. Treasurer.

12/1C86

ANNUAL REPORT

OF THE

ROYAL SOCIETY OF TASMANIA

FOR THE YEAR

1911

TASMANIA:

Printed at "The Mercury" Office, Macquarie Street, Hobart.

PAPERS AND PROCEEDINGS

OF THE

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OF TASMANIA

FOR THE YEAR

1911

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