



The Contributions of John Thomas Jutson
to Geomorphology

By
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VOLUME I

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Summary

This study of the contributions of John Thomas Jutson to geomorphology has drawn on his published works as well as manuscripts contained in the collection bequeathed by Jutson to the Mines Department of Victoria, now the Department of Minerals and Energy, and termed by the writer the "Jutson Documents".

Volume I consists of two parts. In the first, some of Jutson's contributions to semi-arid zone and coastal geomorphology are critically examined.

In chapter 1, the topic of slope and planate surface development are considered, and it is shown that although showing the influence of Davisian thought, Jutson offers an original statement of slope retreat as a process in slope and planate surface genesis.

In chapter 2, theories of the origin of the dry lakes of Western Australia are examined in relation to Jutson's study of them. Modern studies are shown to support Jutson's theory of their origin. In addition, and related to the lakes problem, the question of the extent of the Miocene transgression into Western Australia is examined through a series of communications between Dr. Dorothy Carroll, Professor E. deCourcy Clarke, and Jutson. These communications are presented and critically appraised.

In chapter 3, Jutson's contributions to coastal geomorphology are examined in historical perspective, and in particular, his role in the development of ideas concerning shore platforms is shown to be important.

Part Two consists of original documents that pertain to the history of Australian geology and geomorphology.

Chapter 4 is an account of one of Jutson's field trips in 1915; chapter 5 consists of a transcript of a dialogue between the late Dr. W. R. Browne and the writer; and chapter 6 is the bibliography of Jutson's library.

Volume II is an edited compilation of Jutson's professional correspondence, being part of the Jutson Documents, and consists of 378 letters spanning the years 1898 to 1950, and including correspondence with geologists and geomorphologists from the United Kingdom, Europe, South Africa, North America, New Zealand, and Australia. The volume is introduced by a brief editorial statement, and closes with an author index.

Historically, Jutson is placed by the writer outside of the mainstream of geological activity during his long life. But despite this, his contributions are judged to be significant and original, and should earn for him a place in the annals of the history of Australian geological science.

Statement of originality

This thesis contains no material which has been submitted for any other degree or diploma in any university and contains no material previously published, except where acknowledged in the text.

Acknowledgments

The author is indebted to many for their willing provision of assistance, both tangible and intangible, in the course of this study.

Special thanks are due to Mr. J. L. Knight, Assistant Director, and Dr. D. Spencer-Jones, Director, of the Geological Survey Division of the Department of Minerals and Energy, Victoria, for their help and encouragement during visits to the library of the above Department where the Jutson Documents are located. Dr. Spencer-Jones also gave permission to use the documents for thesis purposes. In addition, permission was kindly granted by Dr. W. R. Fairbridge of Columbia University to include his letters to the late Dr. J. T. Jutson in Volume II of this work. And to those from whom the author, through ignorance of whereabouts, has not received personal permission to include their letters in this volume, the author extends sincere apologies.

In Perth, Western Australia, Mr. J. H. Lord, Director of the Geological Survey, kindly made available the library resources of the Department of Mines and the services of Survey personnel during the author's visit there.

Mrs. Vera Donaldson of North Caulfield, Victoria, graciously received the author at her home, on very short notice, and her reminiscences of her late uncle, Dr. Jutson, were most valued. She also assisted by providing photographs of Dr. and Mrs. Jutson, and by her written communications in response to later enquiries.

The author was especially privileged to meet and converse with the late Dr. W. R. Browne, of Edgecliff, New South Wales. His spirited

recollections of Australian geologists did much to bring to life for the author the history of Australian science. And it was through Dr. Browne that the author met with Dr. D. F. Branagan and Dr. T. G. Vallance of the Department of Geology and Geophysics at the Sydney University. These, through conversation and their several important published works on the history of Australian geology, have provided invaluable help.

The author received the cheerful assistance of Mr. P. Taylor and Miss K. Glenn in performing the arduous task of bibliographic checking, and Mr. D. Webb and Miss K. Korff painstakingly assisted in proof reading. And appreciation is here expressed for the work of Mr. M. Pitcairn who prepared the photographic plates. Special thanks are extended to typist Mrs. D. C. Fitzpatrick who patiently met the demands of the writer.

Finally, the author is grateful to the Academy of the New Church, Bryn Athyn, Pennsylvania, for allowing an extended study leave, and for financial support. And the Research Committee of the University of Adelaide, South Australia, also made funds available to help defray costs incurred in Australia without which help the author's programme would have been seriously handicapped.



Plate 1

John Thomas Jutson and Lilian Jutson, c. 1900.
Reproduced from photograph, gift of Mrs. Vera
Donaldson of North Caulfield, Victoria, to the
author.



PROLOGUE

It is contrary to the usually accepted ideas of justice and fitness that the romance of our early geological gropings in Australia should be neglected by us - heirs to all that picturesque past - for the history of geological progress in Australia is bound up with a wealth of heroic deeds; with herculean struggles with Nature when in unbending and savage mood; it is a record of triumphs won in the teeth, as it were, of grim and armed antagonisms.

So wrote E. C. Andrews in 1942 in his Clarke Memorial Lecture entitled "The Heroic Period of Geological Work in Australia" (Andrews, 1942), referring to the pioneering explorers who prepared the way for the establishment of geological surveys and universities in the Colonies. The first appointment of a Geological Surveyor in Australia was made in 1850 when Samuel Stutchbury filled the post in New South Wales on the recommendation of H. T. de la Beche, first Director-General of the Geological Survey of Great Britain.

Ralph Tate, first Professor of Natural History at Adelaide University, South Australia, provides insight into the focus of attention of early geological work when he states: "Geologists have been too busily engaged in reaping the golden harvests in the domains of palaeontology and stratigraphy to be much tempted by the allurements of chemical geology or microscopic petrology." (Tate, 1893). This focus is not surprising since geology to that time was primarily a field discipline.¹

¹

In a taped interview with the writer, the late Dr. W. R. Browne (see Part Two) remarked whimsically (Dr. Browne being a petrologist) that the appointment of R. A. Farquharson as petrologist to the Geological Survey of Western Australia in the early part of this century "was a sign that they were going to do things in detail." This serves to illustrate the subordinate role that laboratory studies had, vis à vis field studies, in geology at that time.

But in addition to the emphasis that Tate recognized, geological survey work gained a fundamental impetus from the need for the colonies to find economically exploitable deposits, particularly of coal and gold, as well as other metal ores. A. R. C. Selwyn's journey from Cape Jervis to Mount Serle in South Australia (Selwyn, 1860) is one example of such a search for the resource base so necessary to the colonies' future.

The earliest account of the development of the geological sciences in Australia and of scientific societies and universities there, was given in Tate's address "A Century of Geological Progress" (1893) cited above. This contribution was followed by those of E. W. Skeats (1934), E. C. Andrews (loc. cit.), A. R. Alderman (1967), T. G. Vallance and D. F. Branagan (1968), M. E. Hoare (1969), W. R. Browne (1970), Irene Crespin (1971), D. F. Branagan (1972), T. G. Vallance (1975), and D. F. Branagan and K. A. Townley (1976).² In contrast to this body of material on the growth of geology, sources on the history of geomorphology are meagre, and documents relating to its development in Australia are particularly sparse. What contributions there are must be sought as scattered and incidental references in the works cited above. This is not surprising, for early contributions to geomorphology, or "physiographical geology" as nineteenth and early twentieth century geologists called it, were made by geologists (see Chorley,

2

To these citations should be added Ann Mozley's "A Check List of Publications on the History of Australia Science", Aust. J. Sci., 25: 206-214, 1962; and "Supplement to a Check List of Publications on the History of Australian Science", Aust. J. Sci., 27: 8-15, 1964. These provide invaluable sources for historical studies. In addition, the Records of the Australian Academy of Science provides a growing source of material.

R. J., Dunn, A. J., and Beckinsale, R. P., 1964). The focus of attention was on rocks and not on landforms as such. In addition, the fundamental problems for geologists lay not in what W. R. Browne has good-humouredly called "superficial geology" (Browne, interview, loc. cit.) but, as we have indicated, in other areas.

In closing his paper, "The Challenger Expedition and Australian Science," Branagan (1971/1972) stated: "From this time on we can note an acceleration in the quantity and quality of Australian scientific work. The years before 1874 had seen the growth of a lusty baby. In 1874 the baby came erect and began to walk." By coincidence this was the birth year of John Thomas Jutson, who was among the first of Australian geologists to break from the traditional pattern; with him, landform study became "geomorphology" rather than "physiographical geology"; it became a scientific discipline in its own right, not the mere handmaiden of geology.

John Thomas Jutson: 1874 - 1959

...truly one of the great geomorphologists of the world....

R. W. Fairbridge

Jutson was for most of his life a week-end field worker, and it says much for his diligence and indefatigable character not only that he acquired a formal education, but that he carried out field observations that form the basis for all his many geomorphological contributions. He lived and worked in an Australia remote from the educational and research centres of North America and Europe. But this he turned to advantage, for, while reading widely and corres-

ponding with many of the leading geologists and geomorphologists of the day, he yet retained an independence and integrity of mind.

During most of his life he pursued the dual roles of man of the law and scientist. On the surface these appear unrelated. But if analysis, reasoned thought, integrity and honesty are fundamental to the legal mind in the pursuit of justice in law, are these qualities not of equal importance to serious scientific endeavour? There is ample evidence to show that Jutson's life exemplified these qualities. He was certainly single-minded in his efforts to arrive at the closest approximation to the truth concerning the evolution of landforms to which the evidence could lead him. In this weighing of available evidence, the legal and scientific minds are complementary.

Born at Richmond, Victoria, on September 18, 1874, son of English-born restaurateur Charles George Jutson and Elizabeth Jutson, formerly Elizabeth Hall of Tasmania, Jutson died at the age of eighty-five on November 14, 1959. In a tribute to him published in 1960, J. L. Knight gives a general picture of the man:

With the death in Melbourne on the 14th November 1959 of Dr. John Thomas Jutson, there passed one of the most outstanding figures in Australian geology. A man of resolute determination, Dr. Jutson fashioned for himself a career in geology in defiance of all probability. Although destined by his parents for a career in law he did not rebel indiscriminately against it. He willingly accepted his filial obligations but with a zeal which is perhaps the secret of his success, firmly decided to devote himself to his real love - geology.

Dr. Jutson's life story is indeed one of success — in the most honourable sense of the word - success — against odds, odds which continued to be against him — even when he had fought his way to the top. — Over half

a century of geology is here unrolled before us revealing distinction even in the remarkable period of Australian geology - the era which produced the Gregorys, the Jacks, and Woolnough's and others of that calibre.

Deafness overtook Dr. Jutson at the age of 30 to add to his struggle against odds but did not affect his cheerful and friendly disposition. (Knight, 1960)

This evaluation is largely confirmed by the present writer's investigations. But the portrayal of Jutson's life as a continual struggle against odds is, perhaps a little overdrawn. Certainly, his life was unusual in some respects, but, like many men who have made a worthwhile contribution to human culture, he had an interest which he developed, and throughout his life persistently made it bear fruit.

In a letter to the writer dated July 3, 1975, Jutson's niece, Mrs. Vera Donaldson, provides insight into the origins of Jutson's interest in geology and into his character when she writes:

I think he just took it up as a hobby in the first place. I do not know of any relative who encouraged him to study it [geology]. In those days [1904, when Jutson married Lillian Hall, to 1910] the shops were open on a Friday night till 10 o'clock in the evening and uncle Jack would go to the Library in Melbourne and auntie Lily would have a look round Melbourne shops then meet him afterwards My aunt and uncle were a very devoted couple and she did everything to help him in every way. Often on a Sunday in the fine weather he would take the boat Edina to Portarlinton and study there and collect various rocks etc. He would always see the funny side of anything and had a great sense of humour. He was very upright and honest in every way and seemed to enjoy life to the full He did not smoke or drink and led a very quiet life He passed away on Nov. 14, 1959 after only being ill for 3 or 4 days. His wife passed away on 13th Feby [sic] 1961. He had the misfortune to be deaf, and wore a hearing aid for years. This was a great drawback. When he had to go to lectures he had to rely on other people's notes for any information on different subjects.

Jutson attended school in the Melbourne suburb of Northcote, later entering the Melbourne High School (Mrs. Vera Donaldson, personal communication). In 1899 he left school and joined the law firm of Fink, Best and Hall as a law clerk in 1900 (Knight, loc. cit.) From this time to 1908 he attended night school at the Working Men's College (now the Royal Melbourne Institute of Technology) studying geology under G. B. Pritchard "under whom I first had my lessons in geology and mineralogy." (Jutson correspondence, Letter No. 211, Vol. II.) Jutson's period of study at the College ended in 1908, but it was followed by two years as Government Research Scholar at the University of Melbourne under Professor E. W. Skeats during 1909 and 1910.

Successfully applying for a post as Field Geologist with the Geological Survey of Western Australia, Jutson worked in the West from 1911 to 1918, this being the only full-time post he held as a professional geologist. During this time his interest in geomorphology clearly emerged with the publication of works on Victoria and Western Australia.

In 1918 his work in the West was terminated. "He retired as a senior Field Geologist on 1st July, 1918, due to reduction in staff. In today's terms 'retrenched' would be the correct term to use, as it is clear he gave excellent service during his term of appointment." (J. H. Lord, Director, Geological Survey of Western Australia, personal communication)

Though he was then 44, Jutson entered the University of Western Australia and completed the B. Sc. degree in 1920, during which year he also successfully applied for the post of Assistant Geologist Gold Coast, West Africa. However, Jutson turned down the appointment presumably because of the possibility of further deterioration of his hearing owing to the required continuous use of quinine in the tropics (see Letter No. 331, Vol. II). This signalled the end of his career as a geologist, as he then returned to Melbourne to join the law firm of Wilcox and Hall, while at the same time entering the University of Melbourne as a law student, taking the L.L.B. degree in 1925. In 1928 he became a partner of the firm, a position he held until his retirement in 1952. During this period of his life, when he was no longer a practising professional geologist, he received the Clarke Memorial Medal in 1937, and in 1942 was awarded the degree of Doctor of Science from the University of Western Australia.

In certain respects, Jutson worked in isolation from the geological community of his day. His failure to exert strong personal influence within this community was probably due to the part-time nature of his geological work, although his reserved disposition and deafness may have been additional reasons. In a letter to A. Coulson he wrote: "I suffer from deafness which in the field causes inconvenience both to anyone accompanying me and to myself." (Letter No. 218, Vol. II) This isolation, together with the fact that he was mostly self-taught in regard to geomorphology and thus free from close orthodox influences, may indeed have been the single most important factor in nurturing the creative and original

thinking that his life's work displays.

Though Jutson devoted his time fully to geology and geomorphology for only nine of the forty-three years in which he was active, at least half of his many publications appeared outside this period. They represent the products of spare-time activity, as verified by A. Gibb Maitland in the Prefatory Note to Jutson's Physiographical Geology of Western Australia (1914), by his niece's comments noted above, in letters to A. Coulson (Letters Nos. 218 & 220, Vol. II), and from his former field colleague Professor E. deC. Clarke (Letter No. 54, Vol. II).

His service on the Structural and Landforms Committee of the A.N.Z.A.A.S. during the nineteen thirties, the invitation he received to serve as one of the Vice Presidents of Section P for the 1937 Auckland Meeting of that Association (see Letter No. 118, Vol. II), and his being the recipient of the Clarke Memorial Medal and the D. Sc. degree are testimony to the respect in which he was held by his contemporaries. In addition, the comments made by men at home and abroad (see below) are further evidence of their professional esteem for his work.

The following extracts from letters to Jutson spanning the years 1910 to 1935 give ample testimony to the high regard in which his work was held by geologists in Australia and overseas. The reader is referred to Volume II of this work wherein the letters may be read in full.

E. C. Andrews (1910). "All the more would I urge this on you because your paper evidences care and true scientific insight."

(Letter No. 1)

----- (1911). "Have read your two papers on Yarra (and Warrandyte) physiography and geology.

"Allow me to congratulate you on your work....

"Long live physiography. I foresee a great career of usefulness for you in Victoria. You have there the evidence to work out the history of the Tertiary in southeastern Australia." (Letter No. 2)

T. S. Hall (1911). "I must warmly congratulate you on your work, and I admire it very much indeed. It is tip top." (Letter No. 140)

W. M. Davis (1918). "Your letter of Jan. 26 reached me a short time ago - not long after I had the pleasure of reading your valuable paper on W. Australia in the December number of the Geographical Journal...." (Letter No. 105)

T. W. E. David (1919). "I congratulate you on it [1919 paper in Geographical Journal] as well as on your other important contributions to geological knowledge. Your accounts of the evolution of topography under sub-arid conditions are most interesting and valuable." (Letter No. 102)

D. Mawson (1920). "Am most interested to get your reprints - you have produced some excellent work and I take this opportunity to compliment you upon it." (Letter No. 326)

----- (1922). "You have done some very good work indeed in geology and geography. I found you were well known amongst English Geologists." (Letter No. 326)

K. Bryan (1923). "I have been reading your papers on erosive processes in Western Australia with much interest & take the liberty of sending you separates of articles some of which refer to similar subjects.

"Close & detailed studies such as you have made seem to me to be the right line of attack on desert problems. The era of broad generalizations on inadequate observations is over." (Letter No. 21)

----- (1924). "It is interesting to find on the other side of the world a man who is attacking the same problem in much the same way. I hope to hear from you at intervals, and

would be glad to have separates of your papers." (Letter No. 22)

- W. E. Wood (1925). "You do not know it - of course - Bulletin No. 61 of the W. A. Geology series, written by you is one of my treasured possessions; carried for scores of thousands of miles; rather dog-eared, now carefully rebound & reposing in my bookcase in my Perth office." (Letter No. 376)
- O. E. Marks (1933). "I was very pleased to get your letter, having read with much interest your paper on elevation, or its absence at Port Phillip Bay, not merely interest but pleasure at the critical caution with which you attacked the investigation. I have long felt that this critical judgment and care is the one essential feature which modern physiographers seem to avoid, let alone regard as of any importance. A picturesque interpretation, something that appeals to the imagination, is what counts. A plain impartial looking for the facts, unfavourable as well as favourable to an interpretation seems to be distasteful even unacceptable."
- C. Fenner (1934). "I beg to offer you my sincere congratulations on the production of such a fine volume, so clearly written and documented, and so beautifully illustrated." [reference to 1934 edition of The Physiography of Western Australia, Bull. No. 95, Geol. Survey W. A.] (Letter No. 130)
- M. Ongley (1934). "Thanks for arranging for me to get your Physiography of Western Australia. It puts to shame anything in that line so far done in N. Z." (Letter No. 329)
- D. Johnson (1935). "With warm personal regards and with sincere appreciation for your courtesy in sending me a copy of your valuable work, of which we shall make excellent use,...." (Letter No. 161)
- E. O. Teale (1935). "It [Physiography of Western Australia, Jutson, 1934] marks a great progress in the scope of the information since your first edition and you are to be greatly congratulated on the comprehensive and careful presentation of the subject." (Letter No. 359)
- J. Walther (1935). "On my return from the Alps I found your new edition of Geomorphology of Western Australia and congratulate you heartily to (sic) the completion of your so valuable life's work....Through the revision your book did not only win volume but also depth and will always stay as the standard work of Western Australia." (Letter No. 373)

The Jutson Documents

Jutson bequeathed to the Victorian Mines Department a large body of personal documents which bear upon his work as a geologist and geomorphologist. These include notes on his reading, lists of references on various topics, drafts of both published and unpublished papers, a considerable body of correspondence with geologists and geomorphologists in Australia and overseas, and some field notebooks.

Accompanying these is a considerable number of glass-plate and celluloid negatives of scenes in Western Australia, which date back to the period 1911 - 1918, and of Victorian coastal sites.

In addition to these documents and negatives, Jutson bequeathed his library to the Department for disposal, it is presumed, as the Department saw fit. Most of his books were given to the library of the Melbourne University, while some remain at the home of his niece, Mrs. Vera Donaldson of North Caulfield, Victoria. His large number of reprints, however, are housed at the library of the Department of Minerals and Energy, Victoria. Upon receipt of these books and reprints, the library prepared a card index of them.

In the early stages of this present investigation, the writer made xerox copies of all Jutson's documents, and received a photocopy of the card index of Jutson's library from the Department. In addition, the writer had prepared selected copies of Jutson's photographic negatives.

The following remarks are designed to introduce the reader to the important components of these documents, and in so doing, to provide a basis for the discussion of the origins of Jutson's inspiration.

Correspondence

It is beyond the scope of this study to offer an annotated edition of Jutson's correspondence. However, some introductory remarks concerning some of the contributors will serve to place it in the context of the study.

We note first that although the letters span the years 1898 - 1950, about 71% of them were written during the nineteen thirties - the decade which followed Jutson's becoming a partner in the law firm of Wilcox and Hall, and during which he saw the new edition of Physiography (Geomorphology) of Western Australia through the press as Bulletin 95 of the Geological Survey of Western Australia. As well, it was the decade of active field work at coastal sites of Victoria and New South Wales which formed the basis for papers on shore platforms published during this time and later. In this period, Jutson began the habit of retaining copies of letters he wrote to others; thus of sixty-four letters written prior to 1930, only four are copies of his letters. Then for the time following 1930, the ratio of letters to and from Jutson is close to one to one.

Jutson's correspondents can usefully be considered in two groups: those who wrote to him from overseas, and Australian correspondents.

Overseas correspondents

Prominent among this group is W. M. Davis whose letters to Jutson were written during the period 1913 - 1932, beginning, therefore, in the year following his resignation from the chair of geology at Harvard University in 1912 (Chorley, Beckinsale, and Dunn, 1973, p. 439). Aside from the interesting personal commentaries Davis makes regarding his own circumstances and those surrounding him, the letters reflect interests active at the time of writing and can be related to articles and books published by him. For example, his letter of June 20, 1913 (Letter No. 103, Vol. II, of this work) includes a lengthy discussion of geographic description, both in reference to Jutson's Yarra paper (Jutson, 1911) and in general. In the letter Davis says: "Regarding methods of presentation, a subject to which I am giving special attention just now, and one on which I lectured in Paris as visiting professor at the Sorbonne, 1911-12 you will find a number of notes in my package of notes...." These comments can be related to his 1915 paper, "The Principles of Geographic Description" (Davis, 1915). Likewise, his letters written in the period 1917-1920 (Nos. 104-108) include discussion of the coral reef problem which he addressed in numerous articles and a book over the period 1913-1934 (see Appendix III of Chorley, Beckinsale, and Dunn, 1973).

Other American authors include K. Bryan, D. W. Johnson, C. K. Wentworth, E. Blackwelder, and N. M. Fenneman. Jutson's correspondence with Bryan and Blackwelder concerns their mutual interest in arid zone

studies, while that between Jutson, Johnson and Wentworth involves common interests in coastal studies. Correspondence with Fenneman has merely to do with the request for and exchange of each other's published works.

Jutson's correspondence with C. A. Cotton, G. Jobberns and J. A. Bartrum of New Zealand includes both trivial and substantive material, that with Bartrum being of most value in its relation to shore platform studies with which both men were involved.

Among the other overseas contributors, L. C. King of South Africa and J. Walther of Germany deserve special mention. King and Jutson shared a common interest in arid zone processes as will be shown below in Chapter 1 of this study; and Walther had a significant influence on Jutson which, too, will be elaborated on below.

Australian correspondents

Men with whom Jutson corresponded in Australia can be grouped according to their involvement with various facets of Jutson's life and work.

C. A. Sussmilch and E. C. Andrews were prominent in geological circles in Sydney, New South Wales, and shared with Jutson an interest in physiography, contributing to its study in published works.³

3

Memorials to these two contemporaries of Jutson reveal men of outstanding scientific character and personal qualities, that to Sussmilch (1875-1946) written by Andrews (1948), and that to Andrews (1870-1948) by A. B. Walkom (1948). Both men deserve close study in reference to their contributions to the development of geomorphology.

The letters from Andrews are of special interest, with Andrews giving critical commentary on some of Jutson's early published work, revealing Andrews' keen interest in landscape evolution.

F. Chapman, A. Coulson, F. A. Singleton, F. Stillwell, and A. H. Voisey are linked with Jutson through Jutson's work on Victorian geology and coastal geomorphology. A. Coulson and Jutson jointly published a paper, in preparation for which Jutson consulted with F. Chapman and F. A. Singleton respecting palaeontological questions. A. H. Voisey's interesting letters reflect a shared interest with Jutson in coastal geomorphology, as well as other matters (see below).

T. Blatchford, C. S. Honman, F. G. Forman, E. S. Simpson, and E. deC. Clarke form a group as a result of their being colleagues of Jutson's during his appointment to the Geological Survey of Western Australia. Aside from the correspondence with Clarke, that with the others deals with purely personal concerns.

Another group includes C. Fenner, W. Howchin, A. H. Voisey, O. E. Marks, F. A. Craft, and C. A. Sussmilch. The rationale for this grouping is this writer's speculation that Jutson had in mind to prepare a work on Australia as a whole. Two pieces of evidence suggest such an interest. The first consists of Jutson's own grouping of MSS under state titles, suggesting that he was gathering material for such a work. But more direct internal evidence is found in a letter to Voisey (Letter No. 300, Vol. II) in which we read: "Your work in the Pine Creek district should be interesting both from a geological and

a physiographical point of view, and I should like to hear from you regarding especially the physiography since I am interested particularly in the physiography of arid areas of Australia."

This interest may in part explain why Jutson communicated with men beyond the states within which he worked and lived (Western Australia and Victoria): with Marks in Queensland, Voisey in connection with his work in the Northern Territory, with Sussmilch and Craft in New South Wales, and Fenner and Howchin in South Australia.

Other correspondents

E. A. Teale, A. E. Kitson and F. C. Colliver all share with Jutson the experience of being students of G. B. Pritchard, for in a letter to Colliver, Jutson says: "I note that you were trained by Dr. G. B. Pritchard under whom I first had my lessons in geology and mineralogy. He also trained a number of distinguished geologists among them Sir Albert Kitson, Sir Edmund Teale and Dr. Summers of the University." (Letter No. 211, Vol. II)

Apart from all other correspondents stands E. deC. Clarke, colleague of Jutson in Western Australia and life-long friend. The correspondence between these two workers touches on many aspects of their mutual professional and personal interests too numerous to discuss here. But aspects of their work that touch the central themes of this study are introduced later, notably in connection with the "Miocene submergence" debate between Clarke, Dorothy Hill, and Jutson.

In conclusion, one might note that, although many of the letters do not bear directly on the topics selected for close study here, nonetheless, they are presented as a whole so that the letters that are directly relevant to the study can be seen within the context of the whole. Furthermore, this correspondence represents a valuable addition to the archives of Australian geological sciences.

A portrait of field work

Chapter 4 of this volume consists of a description by Jutson of field work near Kookynie, Western Australia; this account is accompanied by a short prefatory note.

The Jutson archive included forty-six field notebooks (see footnote 5 below), some of which have been perused to ascertain their relevance to this study. The earliest of these is dated "Xmas 1901", and, as noted below, includes more purely descriptive material - concerning both the occurrences on the trip and features of the landscape - than later ones which contain more sophisticated geological and physiographical observations.

The portrait here presented has more of the character of the earliest notebooks, although the dearth of geological observations clearly reflects the fact that it was written for his wife Lilian as a descriptive account of his experiences.

Dialogue with the late Dr. W. R. Browne

Chapter 5 of this work is a transcription, with preface,

of an interview between the late Dr. W. R. Browne and the writer, recorded on tape shortly before Dr. Browne died. Characterized - in a note to the writer - as an historic relic by Dr. T. G. Vallance of the University of Sydney, a copy of the tape has been sent to be deposited in the Archives of the University.

Although trivial or non-substantive material is commonly edited out of such interviews, it was decided to leave it in here because it establishes the atmosphere in which the interview took place.

Bibliography of Jutson's library⁴

The bibliography of Jutson's library presented in Part Two of this work is a useful complement to the letters. The books and reprints of journal articles to which he had direct access in his study at Ivanhoe, Victoria, are thereby known, and can be used to give knowledge, albeit incomplete, of the fields to which his correspondents contributed.

There are only eight foreign language works, two of these being by W. M. Davis (1914, 1918) and two by J. Walther (1915, 1924) in German. In Letter No. 306 (Vol. II of this work), there is some evidence to suggest that Jutson could read German, for he says to

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All bibliographic references given in this description of Jutson's library refer to the bibliography of his library that appears on pp. 204-298 of Volume I of this treatise.

Johannes Walther: "I wish that you would write to me in German, as I can understand what you say in that language and it would probably be easier for you." But overall, taking into consideration the Jutson library evidence and his own published writing, as well as his other MSS, there is no evidence suggesting that European workers exerted any significant influence on Jutson through their own writing. By contrast, the works of Sir Charles Lyell, H. T. de la Beche, Sir Archibald Geikie and James Geikie, as well as other English-language authors, are represented in Jutson's notes if not in his library. Influence did come, however, through discussion in English of the ideas of S. Passarge, and through personal contact with Walther as will be shown below in Part One of this study.

While contributions of Australian geologists make up a substantial part of the bibliography, contributions to New Zealand geology and geomorphology are also represented by C. A. Cotton, J. A. Bartrum, and G. Jobberns, and to African geology and geomorphology by A. L. Du Toit, F. Dixey, L. C. King, A. E. Kitson, E. O. Teale, and C. Van Riet Lowe. American workers are likewise represented, most prominently through references to books and articles by W. M. Davis; but, in addition, there are included works by W. H. Twenhofel, whose cited contributions to sedimentology reflect Jutson's interest in the interpretation of sediments, and by E. Blackwelder, whose studies of weathering bear on Jutson's arid zone and coastal studies.

Other groupings of works in the library include nineteenth-century English language literature of Sir Charles Lyell, Sir Archibald and James Geikie; works based on the voyages of H.M.S. Beagle (Darwin, 1890), the Wilkes Expedition (Dana, 1859), and the voyages of H.M.S. Fly (Jukes, 1847) and H.M.S. Challenger (Moseley, 1892); and works on Australia by J. L. Stokes (1846), J. B. Jukes (1847, 1862), R. A. C. Selwyn (1854-55; 1861), J. E. T. Woods (1862, 1880), R. A. C. Selwyn and G. H. F. Ulrich (1866), R. Etheridge (1876), W. B. Clarke (1878), R. A. F. Murray (1887), J. Stirling (1887), T. W. E. David (1890), R. L. Jack (1895), H. P. Woodward (1897), and T. S. Hall (1899).

Other manuscripts

In addition to the above described correspondence and library references, the Jutson archive includes a large quantity of manuscript notes and references grouped in categories of Jutson's choosing. These reveal a painstaking thoroughness on his part, and often include lengthy extracts from books and articles, not all of which were in his own library. Each of these MSS, numbering in excess of 2000 sheets, has been assigned a number, and for each a card has been written describing briefly the content of each MS or group of MSS. In addition, an author and subject cross reference card index has been prepared. Typescript and manuscript copies of published and unpublished articles are also included in these documents.

On the basis of these various sources, the origins of Jutson's inspiration will now be examined.

Origin of Jutson's interests and ideas

Awakened interest - the formative years prior to 1900

The emergence of Jutson's interest in landscape predates his formal education in geology under G. B. Pritchard at the Working Men's College in Melbourne, Victoria. This conclusion is based on two pieces of evidence, the first contained in a letter from E. J. Robertson (Letter No. 336, Vol. II) written in 1898: "With regard to Geikie's book I would be very pleased to read it but perhaps it will be as well not to send it at once as things are unsettled...." The letter goes on to describe some of the geological and topographical aspects of the local terrain from observations made by Robertson in his spare time.

Other evidence is contained in a field notebook of Jutson's dated "Xmas 1901" and numbered "No. 3 18."⁵ The first two pages give the reader a flavour of Jutson's style and of the nature of the early

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Jutson scratched out "3" and replaced it with "18", a confusing thing to do to say the least. From the "List of Contents of Field Books of J. T. Jutson" - received in 1977 upon the writer's request from Mr. J. L. Knight of the Department of Minerals and Energy, Victoria - no clear pattern of time, place, and notebook numbering is evident. For example, No. 18 is dated 1901 and No. 10 is dated 1914. Many of these notebooks are now lost, so that there is now no way of solving the time-place-number problem. However, this does not invalidate the use of this notebook evidence in the present discussion.

excursions that Jutson took in company with friends. "Australian Alps Trip. Xmas 1901. Thurs. 26 Dec^r 1901. Left by 6.15 am train to Bright with Charlie Fred & Hedger. Arrived at Bright at 3.45 pm."

The account opens thus:

Observed the country all the way. As far as Wallan it is all basaltic. The volcanic vents around Wallan & Mt. Fraser (a crater) can be well seen. At Wandong we reached the top of the dividing range & the country has changed to upper Silurian. At Wandong & Kilmore East the country is very steep & hilly. Just before Broadford the Sugar Loaf is seen. This is much higher than the surrounding parts & is very pyramidal in shape. Is it a volcanic vent?

After leaving the train the group of explorers continued on bicycles. Thus on page five: "At Bright we mounted our bicycles and road to Harrietville 16½ miles."

Two changes are noticeable in this notebook between its earlier 1901-1902 and later 1903-1904 entries. First, there is a change in Jutson's calligraphy, letter size undergoing a reduction, giving a more mature appearance to his penmanship. This is evident in a comparison of entries for "Thurs. Dec^r 26 1901" and "Thurs. Dec^r 30th 1902" in Field Note Book No. 18. The second change relates to the level of geological observations that Jutson makes, later ones being more significant, and together with topographic sketches, geological sections, and questions raised, indicate a greater degree of geological sophistication. This possibly reflects the influence of the formal geological education that Jutson was then receiving at the Working Men's College.

The comment in Robertson's letter to Jutson, cited above, and the evidence provided by the notebook establish the fact that Jutson was actively interested in landscape prior to the time of his formal education in geology. This early interest may also account for the presence of three introductory works in his library: The Art of Sketching from Nature (Rowbotham, 1877), Atlas of Physical Geography (Johnston, 1892), and Open-air Studies: an Introduction to Geology Out-of-doors (Cole, 1895). The works by Johnston and Rowbotham are not listed in the bibliography of Jutson's library, but were found in the remnant of his library at the home of his niece, Mrs. Vera Donaldson. These works were inscribed by Jutson, establishing his ownership of them.

But what influences came from the many works that predate 1900, referred to above as either being part of his library or which he consulted in other libraries? Aside from his field notes, Jutson's MSS are undated; but the change in his penmanship noted above, and this considered together with Jutson's personal circumstances at the time, indicate that his notes on nineteenth-century works cited above were written certainly after 1900, and in all probability after 1925 when he completed his law studies.

Thus there is no evidence to contradict the view of Mrs. Donaldson that "he just took it [geology] up as a hobby in the first place." (Personal communication) And this view is substantiated by the late Dr. W. R. Browne, when, in an interview with the writer, he said: "I think he was largely self taught." (See below, Chapter 5)

The period of formal geological education and research experience
1900-1910

As noted above, Jutson attended geology classes for the first time in night school at the Working Men's College in the period 1900 to 1908. His teacher was G. B. Pritchard, himself a student at the University of Melbourne, receiving the "B.Sc. degree in 1908 and D.Sc. in 1911." (Dickison, 1957) To judge from a remark made to F. C. Colliver in the letter cited above (No. 211, Vol. II), Jutson held Pritchard in high esteem, for he says: "I do not...think that there will be another Doctor Pritchard at the Melbourne Technical School." But other than the fact that good teachers do encourage and inspire their students to develop their own interests, there is no evidence to suggest that Pritchard exerted influence in directing Jutson's interests into physiography.

In addition to Pritchard, there is evidence of influence, or at least encouragement, from J. W. Gregory, Professor of Geology at the University of Melbourne during this period. Gregory's many interests expressed in his published works included geomorphology, as, for example, in his book on rift valleys (Gregory, 1896) and on fiords (Gregory, 1913). And in two letters, Gregory expressed his regard for Jutson, as well as his interest in Jutson's developing geological thought. In a letter of September 12, 1911, from Scotland, Gregory wrote:

Allow me to express my best thanks for your reprints
and my congratulations on your very interesting papers.

I should have written before, but was away when your letter came and was waiting till I had made some use of your papers for the new edition of the Geography of Victoria. I am very grateful to you for your kind remarks. It is always very pleasant when one thinks that one's work has been of some little use. You will see when the new edition comes out that I have incorporated your results and have sent in a fresh sketch map to illustrate them. (Letter No. 138, Vol. II)

J. L. Knight (1960) likewise attributes to Gregory an influence on Jutson, but without substantiation.

In addition to Pritchard and Gregory, we may suppose that Professor Skeats encouraged Jutson's interest in landscape, for Jutson's first two publications "A Contribution to the Physical History of the Plenty River; and of Anderson's Creek, Warrandyte, Victoria" (1910), and "A Contribution to Physiography of the Yarra River and Dandenong Creek Basins, Victoria" (1911) are physiographic in emphasis. And Skeats' "Notes on the Geology of the You Yangs, Victoria" (Skeats, 1907) includes observations and commentary on landscape features testifying to his interest in physiography.

But the clearest statement of the source of Jutson's inspiration comes from his own pen. Writing to W. M. Davis in 1932 he says:

You thank me for my interest in your work and welfare, but it would not be possible for me other than to take the keenest interest in both, for you have both directly and indirectly through my life given me some of the greatest pleasure that I have had. The direct pleasure has been derived from the perusal of your books and numerous papers, and the indirect pleasure has come to me from the stimulus given by your work to go into the field and study physiography for myself.

Before reading your works, I had made acquaintance, of course, with the great old English Masters, A. C. Ramsey and Archibald Geikie, but it was your work showing the

evolution of landforms that ultimately gave me such an interest in the subject. (From Letter No. 234, Vol. II)

And in the Introduction to his 1914 Physiographical Geology of Western Australia, he makes the following acknowledgment: "On general physiography the writer's largest debt is to the writings of W. M. Davis, of Harvard, United States of America, but much is also owed to the works of C. R. Keyes, also of the United States, J. W. Gregory of Glasgow, and E. C. Andrews, Griffith Taylor, and C. Hedley, of Australia." (Jutson, 1914, pp. 17-18)

Oddly, however, there are but two brief references to Davis' works in Jutson's MSS. In a list of references compiled for their possible bearing on the question of beach ridges, Jutson includes "W. M. Davis' Die Erklärende etc p. 473" and "W. M. D. Geog Essays pp. 708-709 (1909)" (MS F4 17.2). But when working in the arid interior of Western Australia Jutson would not find Davis' normal cycle thinking relevant for climatic reasons, and nor would he find Davis' arid cycle (Davis, 1905) of service either because of its highly deductive nature containing "whole sequences of changes, many of which had not yet been observed in the field." (Chorley, Beckinsale, Dunn, Vol. 2, 1973, p. 496), and because it "applied particularly to a region with block-faulted ranges and many enclosed basins." (Ibid., p. 655) - circumstances which did not match those encountered by Jutson in the interior of Western Australia. And in Jutson's own acknowledgment on p. 158 of his 1914 treatise, we read: "...the writer desires to acknowledge his indebtedness to the writings on arid erosion of W. M.

Davis... and C. R. Keyes, but he has endeavoured to work out the results of desert erosion in Western Australia from his own observations, with the assistance of any recorded facts pertaining to that country, rather than to formulate a scheme for an arid climate in general."

This statement is revealing of Jutson's own scientific character, namely, that as a man of science he was not a designer of grand theoretical schemes derived deductively, but rather was he primarily a field man of independent mind. Nonetheless while working of necessity with abstractions, as all researchers must, his focus was on field problems to which he sought answers in the evidence that the field would itself furnish. And when the abstractions of earlier workers failed to provide interpretive concepts, he constructed his own, thereby contributing original thought to the science of geomorphology. This will be exemplified in Chapters 1, 2 and 3.

PART ONE

Introduction

Jutson's important contributions to geomorphology concern, first, arid and semi-arid zone regions in Western Australia, and second, coastal features in and adjacent to Port Phillip Bay, Victoria, and around Sydney, New South Wales.

With respect to the former, his contributions are wide-ranging, both topically and geographically. Furthermore, with respect to scale, they range from consideration of the broad features of plateaux and plains, river basins and wide-spread lakes, to the minutiae of weathering and erosion considered both morphologically and dynamically.

Nowhere does Jutson clearly define what he meant by arid or semi-arid climate, a point raised by K. Bryan (see Letter No. 21, Vol. II). Not that Jutson failed to consider climatic factors, as reference to his 1914 (Bull. 61, Geol. Surv. W.A.) or 1934 (Bull. 95, Geol. Surv. W.A.) work will testify. But clear definition he did not give. In the works of some writers, especially of European workers (notably H. Baulig, 1952; A. Cailleux & J. Tricart, 1965; and P. Birot, 1968), the attempt is made to clarify the demarcation between morphogenetic regions in terms of process and form. But in view of the complexities of the problem, made more so by palaeoclimatic factors, Jutson can be excused for his failure. And in responding to K. Bryan's remark to Jutson in the letter cited above, M. G. Wolman remarked: "In addition his [Bryan's] observations about the importance of defining more appropriate geomorphic measures of climate were not only sharp

then [1923] but remain so today" (personal communication, 1976).

Jutson's contributions to coastal geomorphology are more restricted in scope, focussing primarily on shore platforms, with secondary attention being given to general aspects of coastal erosion and sedimentation, and the role of man as a geographic agent in coastal processes.

The writer has of necessity restricted the analysis of Jutson's contributions concerning arid and semi-arid zone geomorphology, treating slope and planate surface development and the dry lakes of Western Australia. The omission of a treatment of weathering and special erosion processes, of granite landforms, and of the broad topic of physiographic divisions of Western Australia, may be regarded as serious. However, in analyzing the selected contributions, aspects of some of the omitted topics are inevitably discussed, albeit incidentally, and a proper treatment of Jutson's regional geomorphology awaits the work of those who know Western Australia from direct and extensive field experience. The writer's study of Jutson's coastal work is more exhaustive, largely because Jutson's contributions in that field are far more circumscribed. The following remarks are offered to introduce the reader to the topics.

While the development of slopes and the origin of planate erosional surfaces can be considered independently, with Jutson, as with others, the processes of their development were interdependent, and for this reason they are dealt with together in Chapter 1. Those familiar with geomorphological literature of the nineteenth and twentieth

centuries will at once recognize the central position that the study of these topics has had in the science of landscape development.

For Jutson, prior to the publication of his 1914 treatise, there were three well defined theoretical schemes offering explanations of planate surface development: the plain of marine denudation, a surface formed by subaqueous marine erosion championed, for example, by A. C. Ramsey in his essay "The Denudation of South Wales" (Ramsey, 1846); the peneplain of W. M. Davis' normal cycle of erosion, the seminal idea for which Davis acknowledged having its source in Powell's "Exploration of the Colorado River" (Davis, 1954, p. 350); and the desert plain formed by S. Passarge's "levelling without base-levelling", a concept Jutson encountered in Davis' "The Geographical Cycle in an Arid Climate" (Davis, 1905). These ideas formed the background to Jutson's consideration of the development of the Old and New Plateau surfaces of Western Australia in his 1914 work.

Prior to the 1924 publication of W. Penck's Die Morphologische Analyse (Penck, 1924), definitive studies of slope development per se were few. According to Schumm and Mosley (1973), Davis' "only article dealing solely and specifically with hillslopes" (p. 10) was a short note entitled "The Convex Profile of Bad-land Divides" (Davis, 1892), this being a discussion of G. K. Gilbert's treatment of "Bad-lands" in his famous "Geology of the Henry Mountains" (Gilbert, 1877). It is not surprising, therefore, that Jutson's treatment of the topic should be entwined with his consideration of erosional surfaces, rather than being treated in insolation.

The history of the study of slopes that developed after Jutson's contribution to it is complex and well beyond the scope of this study to discuss in any meaningful way. However, Jutson's contribution is tied into the history through the work of K. Bryan, W. Penck, and L. C. King, in Chapter 1 of this study.⁶

The critical evaluation of Jutson's contribution to the study of the dry lakes of Western Australia is presented in Chapter 2, and includes discussion of all theories that have been proposed for their origin; the sources of the ideas that Jutson brought to their study is thereby indicated in Chapter 2.

Two recent works demonstrate the currency of Jutson's ideas about the genesis of the lakes. In Jennings and Mabbutt (1971), Mulcahy states: "...since the observations of Woolnough and Jutson, this southern part of the Salinaland [that includes the lakes and sandplains] has received but incidental attention, and that mainly in studies of soil distribution." (*Ibid.*, p. 217) Later in the same article Mulcahy says of the salt lakes: "Their floors are now wind-planed flats, with banks of sandy or gypseous windblown deposits and lunettes...on their eastern and southeastern shores. Jutson (1934) described the low-

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Three particular volumes in the Benchmark Papers in Geology Series (Rhodes W. Fairbridge, Editor) are most useful in bringing together papers that represent important contributions to the topics in question. These volumes are: Stanley A. Schumm and M. Paul Mosley (eds.), Slope Morphology, Dowden, Hutchinson & Ross, Stroudsburg, 1973, 454 p.; George F. Adams (ed.), Planation Surfaces, Peniplains, Pediplains, and Etchplains, Dowden, Hutchinson & Ross, Stroudsburg, 1975, 476 p.; and Cuchlaine A. M. King (ed.), Landforms and Geomorphology. Concepts and History, Dowden, Hutchinson & Ross, 1976, 404 p.

ering by deflation of the salt-lake floors below the original valley levels, with migration of the lakes as they are forced upwind by the accumulation of aeolian materials on the lee shores, so that the old drainage lines are not only partially blocked, but distorted and displaced westwards." (p. 219) The second recent work referred to is that of Killigrew and Gilkes (1974) which provides data substantiating Jutson's account of the lakes' origin, as is shown below in Chapter 2.

The major part of Chapter 3 is devoted to an account of the study of shore platforms in Australasia and Jutson's part in it. The background studies of these features made prior to the time when Jutson entered the scene are discussed in detail, as are the studies made by other workers whose articles appeared during the time when Jutson was actively involved in the topic during the period 1931 to 1953.

Subsequent to this time, studies of shore platforms have continued to appear from time to time. These studies have enriched the literature on platforms in four respects. (1) Platforms on the shores of the United Kingdom, Japan, North America, and the Soviet Union have received attention as exemplified by the studies of So (1965), Takahashi (1973), Trenhaile (1978), and Zenkovitch (1967). (2) Field-based morphometric studies to which statistical techniques have been applied have helped resolve previously raised but unanswered questions, as for example in Trenhaile (1972). (3) Flume experiments have been employed in the attempt to answer questions regarding levels of wave attack. And (4) Davies (1973) has raised questions regarding the influence of climate on coastal processes, and thence on form, appropriate now because of the wider geographic distribution of studied platforms.

These studies do not invalidate Jutson's contributions to the study of shore platforms, nor do they make them any the less important. They do, however, represent advances toward the answering of questions raised by Jutson and other earlier students of these coastal features. It is not, however, within the scope of this study to elaborate on this.



Plate 2

Little Table Top, Cue, Western Australia. Laterite capping of the Old Plateau surface; surrounding pediment (not so named by Jutson; but see Jutson 1950, p. 97, Figs. 47, 48, 49 & 50.) is the New Plateau surface. Photograph by J. T. Jutson in the period 1911 - 1918.

Chapter 1

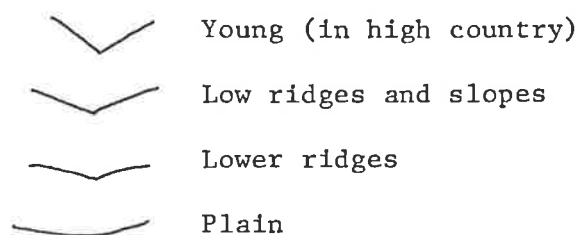
Slope and planate surface development

As slopes are the fundamental unit of landform assemblages and no facile generalisations may be made concerning their origin, it follows that none can be formulated to explain the diverse landscapes of the world.

C. R. Twidale

Jutson did not publish any papers that dealt expressly with the topic of slope development. Nevertheless a number of his works discuss various aspects of the question, and it is clear from his field notebooks that he recognised the question as an important one. He mulled over the problem, not only while in the field, but in his reflections on fundamental questions as expressed in his published work. As evidence, the following extract was taken from Field Note Book No. 10, page 13. "The high belt of Di presents some instructive lessons in physiography. It appears to have been an old plateau and it is in a youthful stage of dissection - the gullies (and there are many of them) being quite V-shaped (with no width on the bottom) altho' somewhat open thus  In places the solid rock outcrops at the bottom & the sides are generally covered with a sheet of creeping rock waste. They diverge (or radiate) on the surrounding plains (on all sides) & there they pbly [probably] become largely lost, the water pbly simply spreading out into broad sheets and evaporating before reaching any main drainage line such as Lake Raeside....As the gullies are descended the ridges become lower the sides flatter until there are practically no sides. Thus this is a normal phase. except that the base level here is the high plain & not the country

usually not much above sea-level. Forms of these valleys



...."(dated 14/8/14).

This extract is placed in a series of notes of a strictly geological nature, and serve to demonstrate the clear distinction Jutson made in his own mind between geological (bedrock) and geomorphological thought. It also demonstrates Jutson's sense of the dynamics of process ("...the sides are covered with a sheet of creeping rock waste"), a feature characteristic of most of his work.

Other features of Jutson's thinking expressed here are noteworthy, particularly the Davisian allusions. His use of the term "youthful" carries only descriptive connotations, despite the phrase "youthful stage of dissection", for he is merely emphasising the V shaped cross-section of the gullies. But then the later outline (with sketches) of the change in cross-valley morphology do represent an attempt, it seems, to view the situation within the conventional Davisian concept of valley morphology in progressing from a valley head to its base level within the normal cycle of erosion. But quite evidently the attempt is misplaced, since the site he is describing lies in a semi-arid zone to which Davisian normal cycle concepts do not apply, although Jutson's attempt to apply them here is confirmed by his statement "Thus this is a normal phase....", which may be rephrased for the sake of clarifying the intended meaning

"Thus the down-gully change of cross-gully morphology exemplifies [Davisian] normal cycle development..." Yet the inappropriate application of normal cycle concepts is further substantiated by his reference to "broad sheets" of water (McGee's sheetfloods) "evaporating before reaching any main drainage line...."

The above comments on what is a single, isolated, passage from an early field note book require qualification. In none of Jutson's published works is valley morphology described within so strictly a Davisian model. But as will be shown later, Jutson owed a deep and sincere debt of gratitude to Davis, and held him and his work in high esteem. However, as his (Jutson's) thinking progressed, it became free of its early dependence on Davisian concepts, and takes on an independence of the kind that is characteristic of original thinkers. None of these comments should detract, however, from our appreciation of Jutson's typical close attention to detail, both of form and process, that is exemplified in the quotation.

Jutson's ideas about slope and planate surface development are most clearly expressed in his discussions of the nature and origin of the plateau surfaces and the "dry lakes" of Western Australia. We must, therefore, outline the morphology of these features as a basis for the discussion that follows.

Plateau surfaces of Western Australia

In Bulletin 61 of the Geological Survey of Western Australia titled An Outline of the Physiographical Geology of Western Australia (Jutson, 1914) we read:

Its most striking characteristic is that of a vast truncated upland (which is known as the Great Plateau of Western Australia) elevated to a height varying almost from sea-level to perhaps close upon 4000 feet. Its general height is below 2,000 feet, and its average height would probably be from 1,000 to 1,500 feet. It occupies practically the whole of the country, except for the narrow coastal plains by which in places it is bounded. Inland, the plateau may be regarded as a vast, gently undulating area....The interior of the Great Plateau is arid and has no permanent rivers. The drainage runs into shallow basins (with no outlet except at times of great flows of water), called "salt" or "dry" lakes, as they are for most of the year dry, and salt is deposited from the evaporating water....It must be understood that when reference is made to the Great Plateau, it includes the whole plateau of Western Australia formed prior to the commencement of the present cycle. The term "old plateau" is used synonymously with "Great Plateau", but in places the former more particularly refers to those portions of the latter which are situated in the Salt Lake and Eastern Divisions. The context will make this clear. The "new plateau" is the plateau now in course of formation by arid erosion in the interior. (pp. 19 & 20)

In discussing the origin of the old plateau in the Salt Lake and Eastern Divisions, Jutson employs the method of multiple working hypotheses (Chamberlin, 1890), postulating four modes, asking:

"Is it a plain of marine abrasion, an uplifted peneplain or a low-lying plain of desert erosion since elevated to its present position, or does it represent an old still-standing plateau of originally much greater height than at present, but the surface of which has been reduced to its present level by arid erosion?" (p. 78)

These four hypotheses represent all the then current modes of thought regarding the origin of planate surfaces in arid lands, the "plain of desert erosion" entering the field of possibilities for Jutson through the works of Davis (1905), Passarge via Davis (1919), and Keyes (1912).

After discussing the various possibilities, and eliminating marine abrasion for want of evidence, and admitting the possibility of genesis under arid conditions, Jutson (1914) cites evidence to support the view that in the past the climate was moister than at present and concludes that "it seems on the whole more reasonable to consider than perhaps the total surface suffered peneplanation and formed a mighty peneplain, rising however (at least immediately after the formation of the coastal peneplain) to a considerable height in the interior, although much less in height than the present surface. The peneplain might be named the 'Great Peneplain of Western Australia'" (ibid. p. 93).

To this point in his treatment of the plateau surfaces Jutson displays no particularly original thought. This is understandable, for in dealing with the remnant surface of the old plateau there is very little evidence to work with, other than the existence of the remnants themselves. There is no alternative to discussing possible modes of origin within the framework of the then current theoretical concepts. However, in dealing with the new plateau, an extensive surface still expanding at the expense of the old plateau, Jutson displays an originality of thought that demands close analysis.

The initial surface from which the new plateau is formed is that of the old plateau. In the physiographic division under consideration, the bedrock consists of an "old probably pre-Cambrian complex of sedimentary, metamorphic and igneous (both acid and basic) rocks...." Continuing the description, Jutson writes:

Usually the granites and sediments have siliceous caps, and often the basic rocks possess ferruginous ones. These are formed by the underground water being drawn to the surface by capillary attraction, and there depositing the substances carried in solution, chiefly iron and silica, or both. The caps vary from a few inches to perhaps six feet or more in thickness. Below the caps, the rocks are usually highly decomposed to various depths (in places down to hundreds of feet), the softest portions being adjacent to the caps.

If a surface be assumed on the great uplift somewhat deformed from a dead level and, perhaps, dissected to some extent by the old deep leads, when rain falls (generally in considerable quantities when it does do so), it will naturally seek the lowest portions of the country, forming for a few hours a stream entering one of the many inland drainage basins. If there be sufficient fall in the ground, this occasional stream will corrade a channel for itself, or use a pre-existing one. The depth of corrasion that can be accomplished will vary according to rainfall, degree of slope, depth of the receiving basins, and nature of the rocks, but aggradation of the basins and the lower ends of the channels will immediately commence....The stream will then have cut a narrow, rather steep sided gulch, especially where the rocks are protected at the surface by a hard cap..., and are comparatively soft below.... This can actually be seen going on in Western Australia. The elevated block is represented by a portion of the old plateau, and the plain (the base level) by the new plateau. On the cessation of vertical stream corrasion, atmospheric erosion then operates, wearing out the sides and undermining the surface where hard, with the result that the latter caves in, and so the gulch becomes widened. The debris from the sides and surfaces must be removed, and this may tax the transporting agencies to their fullest extent, so that vertical erosion by this means may be restricted. (Ibid. pp. 142-144)

This last sentence is significant. As we shall see shortly, Jutson held the wind responsible for the evacuation of debris from slopes and plain surfaces alike. However, he here clearly attributes to running water not only the initiation of a new and lower surface, but also the extent of vertical lowering of the old surface in the production of the new. Continuing, then, with the quotation.

In the meantime lateral erosion steadily proceeds, and the gulch becomes wider until it may be called a shallow valley, generally with cliffs ("breakaways") or steep slopes.

As the valley widens, the stream formed by the rain similarly spreads out, with practically no erosive power. Moreover, it is comparatively seldom that rain falls, so that this fact, with the rapid evaporation, prevents anything like a permanent stream....The ever widening bottom of the valley is on the other hand constantly exposed to the wind, which, as the valley expands, gradually becomes the chief and almost the sole denuding agent....From the single valley formed in this way, there appears to be nothing to prevent the whole surface of the arid belt of country from being denuded by this process of lateral erosion into a vast high plain or plateau. But many valleys are formed and these widened in opposite directions until they in time meet others approaching them. Under these conditions the belt of cliffs becomes narrower and narrower, until on the junction of two adjacent valleys, these cliffs are wholly consumed and one wide plain results (Figs. 67-72). This process is going on steadily at the present time, and in many areas it has reached a stage that the valleys have become a vast plateau (the "new plateau") with only a few residual ridges and isolated hills (Figs. 25, 73, and 74). In parts however, the new plateau appears to merge in the old plateau by long, gentle slopes, this, no doubt, being largely due to the absence of hard protective caps (Fig. 75). (*Ibid.*, pp. 144-147)

In describing the formation of the new plateau in the foregoing statement, Jutson presented what is believed to be the first detailed account of the formation of pediments and their coalescence, to produce a pediplain by the process of parallel retreat of slopes aided by deflation. Before him, McGee (1897) described the vast rock planed surfaces of the Sonoran district of California, and attributed them to the corrasive action of sheetfloods; in addition he described their growth in terms of the retrogressive erosion of flanking mountain masses (*ibid.*, p. 110). But, as we shall attempt to show, Jutson's treatment of the origin of these vast desert plains is more comprehensive.

That the two workers were dealing with the same type of topography is evident from McGee's description of the Sonoran district.

...but so far as the observations go they indicate that the vast plains diversified by scattered sierras and buttes do not represent an alluvium-buried mountain land so much as a planation level with a few monadnocks still surviving. (Ibid., p. 92)

In one important respect, however, the regions differ in that, whilst the Sonoran district "is a great plain sloping from the high Sierra to the sea" and therefore having exorheic drainage, the drainage of the new plateau of Western Australia, at least in its interior parts, is endorheic. This has a bearing on the mode of evacuation of the eroded material treated of below.

McGee is unequivocal in his treatment of the mode in which the plains are formed; sheetfloods, to which he attributes great erosive (corrasive) capacity, are the principal agents.

The efficiency of the sheetflood in corrasion is made manifest by consideration of its mechanism: The velocity of flow must be considerable, else the flood is absorbed and evaporated; the water must be laden to its full capacity with abrasive material, else it runs clear and gathers into streams. This material, whether fine or coarse, is exceptionally hard and sharp rather than softened and rounded by decomposition, and the internal currents in the shallow sheet are such as to constantly batter and scour the subjacent surface as the mass half rolls and half slides across it. The inference from the character of the sheetflood is consonant with the necessary inference from the character of the baselevel surface. Over dozens or scores of square miles in carefully examined localities, hard rocks like those of the mountains, and with no sign of decomposition, are planed almost as smooth as the subsoil by the plowshare, with nothing either in configuration or in covering to indicate that streams have flowed over them, and extended consideration has yielded no other suggestions as to the eroding agent than that found also in analogy with the observed sheetflood. (Ibid., pp. 108-109)

In addition to the above quotation, one summary statement will serve to make McGee's position clear.

In brief, it may be affirmed, with so much confidence as the conditions of observation during two expeditions warrant, that the general effect of sheetflooding... is to carve baselevel plains, lightly veneered by the carving material, about the medial altitudes; that these plains tend ever to retrogress into the mountains, and thereby steepen their slopes and render them exceptionally rugged; and that the anomalous topography of the region is not susceptible of explanation by other agencies. (Ibid., p. 110)

Point for point, McGee and Jutson differ in important respects. Re-capitulating, the sequence as envisaged by Jutson includes an initial stage of valley incision by stream flow in the undulating surface of the uplifted old plateau, the valley-side slopes retreat under the action of the rain and the wind and thereby the valley is widened, until at length adjacent valleys merge to produce extensive plains. In this scheme, Jutson attributes to stream action the primary role of surface lowering (whereas, once the valley widens, "the stream formed by the rain...spreads out, with practically no erosive power"); in contrast, McGee attributes to sheetflooding the process of surface lowering, which process has the power "to carve baselevel plains". As to the mode of evacuation of detritus, Jutson is clear. Substantiating his own observations with those of his colleagues working in Western Australia, he concludes:

There is, however, no doubt that an enormous amount of detrital material...in a very fine state is being "exported" (i.e., removed from the division) by the wind, some to another area, but the final resting place of much of it must be the sea. This "exportation" may be carried out in stages by means of temporary deposits by the wind in various parts of the country. Thus the land surface of the State is being slowly but surely reduced by the action of this powerful agent of destruction, the wind. (Jutson, 1914, pp. 150-151)

McGee, on the other hand, is equivocal on the point, the reader

being left to surmise that evacuation is brought about by running water, a process that seems scarcely feasible within the context of an erosional system depending on periodic sheetfloods to accomplish the task of surface lowering by abrasion.

One final point of contrast between the two systems of thought outlined above is this. Whereas Jutson is clear in defining the initiation and perpetuation of the piedmont angle, initiation by stream incision and perpetuation in the process of slope retreat, McGee is again undefinitive in this regard. It is true that he suggests the means whereby the angle is maintained; "these plains tend ever to retrogress into the mountains,...", but the question of the initiation of the angle is not raised.

The role of wind as an agent of erosion

That Jutson ascribed a dominant role to wind as an eroding agent, howbeit as an agent of exportation rather than as one of abrasion, but still as an agent whereby the land surface is lowered, is clear from the foregoing discussion. In this regard his thinking follows that of J. W. Gregory and of C. R. Keyes to whose work he refers. In addition, in the general climate of thought at the time, to the wind was attributed as great power as an eroding agent as was attributed to water in humid regions, as illustrated by the following.

The wind is the great agent of denudation in arid regions. It sweeps along hard grains of sand, which wear away the rocks exposed to them, like a natural sand-blast. Bare rock surfaces in deserts are often polished, as well as characteristically sculptured, by wind erosion. The wind may pile up the loose material in dunes or sandhills, or

sweep it into the hollows, which are thus gradually filled up. (Gregory, 1908, p. 47)

Notwithstanding the fact that during the past decade the wind in the capacity of a potent agent of general erosion has come to be recognised more and more universally, there still lingers a certain reluctance to admit its effectiveness in specific cases or its high rank among the degradational processes....

Recent investigations in arid and semi-arid countries appear to demonstrate beyond all shadow of doubt that as a denuding, transportative, and depositional power the wind is not only fully competent to perform such work, but that it is comparable in every way to water action in a moist climate. As lately noted, it is significant that most of the broad intermont plains of the Southwestern Desert, for instance, should be areas of rapid degradation instead of aggradation, as is shown by their remarkable rock-floors, that the little normal water action therein should be confined to the loftier mountains, and that general plains surface should be so little affected by stream corrasion. General desert leveling and lowering must find for their chief sculpturing agency something other than stream action. All things considered, deflation, or wind scour, in arid lands not only appears to be the principal erosional process, but water action surprisingly subordinate. (Keyes, 1912, pp. 540-541)

Subsequent to the publication of the 1914 edition of the Physiographical Geology (Physiography) of Western Australia, Jutson published several papers that focussed on a number of processes operating in arid and semi-arid lands: on sand ridges, sand plains and sand glaciers (1918), sheet-flows (1919), the "clawing" action of rain (1919), dust whirls (1920), and gravitational drift of rock debris (1921). Clearly he was seeking answers to questions that in his mind required more rigorous study. In this regard, correspondence Jutson had with Kirk Bryan is interesting, not only in showing that Jutson's work was in the forefront of arid geomorphology, but also because it expresses the high regard that his contemporaries had for his work. The following are letters written to Jutson in March 8, 1923 and March 4, 1924.

(Letters Nos. 21 & 22, Vol. II)

I have been reading your papers on erosive processes in Western Australia with much interest & take the liberty of sending you separates of articles some of which refer to similar subjects.

Close and detailed studies such as you have made seem to me to be the right line of attack on desert problems. The era of broad generalizations on inadequate observations is over.

I am searching for valid criteria to show wind scour and it seems to me that you have in your parallel arrangement of debris at right angles to the slope of the rock surface (pediment) a useful & valid criterion.

I have a paper ready for the press on wind scoured hollows (rock tanks) in which I have used them as criteria. Pedestal rocks (mushrooms) are not necessarily due to wind action being found in all climates, but as I am showing in another paper even in a climate with about 5 inches they may be safely attributed to rain and weathering.

When these papers come out I will send you separates, and you will note that I am attaching a great deal of importance to the variety of arid climate, which is producing the results observed. Your papers, doubtless because the facts are so well known in Australia, merely say that your field of study is sub-arid with less than 10 inches rain-fall. In Arizona & New Mexico we have little wind work with 10 inches & in most localities streams are dominant in transporting debris down to 3 inches. Perhaps you have very high temperatures or the rains come in small amounts or very gently?

This letter was returned to Bryan and remailed Nov. 15, 1923. And in pencil Jutson noted on it "Recd 5/2/24. Answered 18/2/24"; these notes are an interesting commentary on the time factor in international communication at the time.

On March 4, 1924, Bryan again wrote to Jutson at a time when he (Bryan) was employed as a geologist in the Water Resources Branch of the U.S. Geological Survey. Quoting in part we read:

...You will note that I am still unconvinced of the general efficacy of wind as an erosive agent, but that I have no doubt of its transporting power. [Here, Bryan agrees with Jutson, who, as noted, attributed to wind the role of transportation of debris.] Generalizations, however, are really premature and the advocates of wind erosion have sinned in assigning the phenomena of one type of arid climate to all types. They have, also, made sweeping statements on insufficient and uncritical observation. Your papers seem to me to be a refreshing contrast, because your observations are close and critical. You have not, however, defined the climatic characteristics of Western Australia precisely, and this omission leaves me wondering why there is laterite in the region, and whether there are not periods of heavy rainfall at intervals of a year or two. I mention these matters because I feel that with close and critical analysis we will one of these days be in a position to generalize on the value of wind work.

In regard to sheet flood erosion you will note that the Papago country is the type area in which McGee made his observations. The rock plains that he saw are the pediments I have described and most of them are no longer in process of formation. The sheet flood that he describes so graphically took place in an area where the streams are depositing, not eroding. McGee's theory, therefore, rests on faulty observation. I did not feel justified in saying that sheet flood erosion is impossible, -- it may easily be, and, therefore, did not see the use of calling attention to McGee's defects as a field observer. However, it occurs to me that you might be glad to have this inside information on a favorite American doctrine.

It is interesting to find on the other side of the world a man who is attacking the same problem in much the same way. I hope to hear from you at intervals, and would be glad to have separates of your papers.

During the period of this series of letters Jutson was no longer working as a geologist, and had, as we have recounted, returned to the legal profession, allusion to which is made in the following two letters from Bryan, the first of which is of interest primarily from a biographical point of view, whilst the second contains commentary on Bryan's work at the time and his appraisal of the state of knowledge concerning arid geomorphology. The first, dated Sept. 2, 1924 was

written at Cle Elum, Washington (Letter No. 23, Vol. II).

Your letter of Feb. 18, resent July 29 was received today in this little mining town among the pines, where I am trying to determine whether if we build a dam 100 ft. high to store irrigation water, we will flood the mines with seepage. It is a far cry from the Arizona desert, but only 30 miles from a desert just as real tho not as notorious.

However the problem of the dam or the damn problem is so difficult & engrossing that I will wait until winter when I am back in Washington, D.C. to write you a real letter.

Let us hope that we will have this correspondence on a business basis. Please send me U.S. Geological Survey, Washington, D.C. I travel much & have no permanent address in the city of Washington.

I regret that you are away from a desert at present, but so am I. One must eat: it is a vulgar but necessary habit.

The following extracts are from Letter No. 24 (Vol. II), in November, 1924.

The past season has not been very productive of new material on desert processes, but after January 1 I hope to have my time free to work on a long delayed report on the San Pedro Valley in southern Arizona, an area that has a fine display of the erosion surfaces that I have been calling pediments. Similar surfaces have been found in Nevada and very extensive geological reconnaissance is being carried on by the U.S. Geological Survey. In unraveling the post-Miocene history, the geologists engaged have found that the study of these surfaces is of prime importance. When this work is published I will see that you get copies, but that time may be several years from now, as our publication schedule is much congested.

It seems unfortunate that your service in western Australia is at an end, and that you are living in a relatively humid climate, because I feel that you were just well started on the problems of arid regions. However, I believe that similar studies of humid areas are just as necessary. We know very little about the angles of slope typical of erosion in various humid climates or the effect of periodicity of rainfall. This ignorance

leads to much confusion of thought in the study of the so-called normal cycle and in the interpretation of sediments. Perhaps you will be able to give us something on the vicinity of Melbourne.

The above letter is the last of the correspondence between the two arid zone workers to deal with matters relating to arid regions. Subsequent correspondence in the 1930's was limited, Bryan writing to Jutson briefly in response to his receipt of Jutson's paper on erosion and sedimentation in Port Phillip Bay (with a title reminiscent of the title of the famous paper of Bryan's on erosion and sedimentation in the Papago Country, Arizona), and of Jutson's second edition of his Physiography (Geomorphology) of Western Australia. Bryan's letter of November 9, 1929 (Letter No. 25, Vol. II) is worthy of inclusion in the present sequence in that, despite the lapse of seven years, it is linked with his letter of November 14, 1924.

I have your letter of Sept 29 & have enjoyed your paper on Port Phillips Bay. I am glad to know that you are back in the game I will send you under separate cover what I have that is new.

Physiography or as it is coming to be called geomorphology is apparently taking a burst all over the world. The Germans are excited over the philosophy of W. Penck. Just what is doing the work in the U.S. I do not know. For one thing the arid regions are coming into their own. Obviously the transition types have the greatest importance & I hope we will hear from you.

In Bryan's letter of March 4, 1924, he raises two points which it is now our intention to discuss in some detail with reference to Jutson's views. These are the role of wind as an erosive agent, and the work accomplished by sheet floods.

In his 1914 treatise Jutson attributes to the wind great erosive power, as has been shown. But by assigning to the wind such a role in the formation of the extensive new plateau, Jutson is faced with a

problem as were Passarge and Keyes before him. We read:

It is a remarkable fact how even, generally speaking, the surface of the present plateau is....Assuming that it was formed in the way described, first, mainly by narrow vertical water erosion and then by wind erosion (mainly lateral), it is difficult to understand why new valleys and new cliffs are not being extensively formed by the wind (as the rain without some substantial inequalities of surface to concentrate the water in a channel, is insignificant in its erosive power), below the levels of the present broad valleys. (Jutson, 1914, p. 153)

After citing the development of the plain as an example of Passarge's "levelling without base-levelling" because the present plain is so elevated above sea level, he resolves the problem as Passarge did by assuming that "the rainfall supplies detritus to the lower portions, and this keeps the wind too busy for further erosion" (*ibid.*, p. 153). This solution, however, raises an additional problem for Jutson (which need not be presented here) to which with some hesitation he offers a solution, concluding, however, that "This again requires an equilibrium that demands strong evidence before it can be definitely accepted" (*ibid.*, p. 153). These ideas are brought forward to substantiate the opinion that at this time Jutson was not altogether satisfied with the position he had taken with regard to the efficacy of wind erosion, and the papers cited on page 28 bear further witness to this.

In the second edition of his Physiography of Western Australia (1934), in which he incorporates the ideas developed in the papers referred to above, Jutson makes a clear shift away from his earlier emphasis on the efficacy of wind erosion. To what extent Bryan influenced this shift cannot be said; except to note that his papers on arid land processes appeared before Bryan's paper on pediments in

Arizona, and prior to his correspondence with Bryan, and both of which, we may surmise, could have helped to push him in the direction he subsequently moved in, as expressed by the following:

Which of the two agents, water or wind, is the more powerful in moulding the whole surface of the semi-arid country is a difficult question to decide. It has been indicated above where in particular areas or under a particular set of conditions one or the other predominates, but viewing the country as a whole the writer inclines to the position that for the present stage of erosion the rain is the predominating, but not necessarily the overshadowing partner....

It will be noted that the conclusion as to the relative efficiency of wind and water in producing the surface features of Salinaland, modifies considerably the ideas expressed by the writer in the first edition of this work, as to the predominance of the wind. Those ideas were formed largely in consequence of the recognition of the general absence of stream corrasion outside the higher lands, and of the non-recognition of the great importance of the slow, but steady transportation of fine detritus on the gentle slopes over wide areas by the action of the rain. Subsequent detailed studies in the field in a wet season vividly brought home to the mind of the writer the power of gentle flowing rain, and the conviction consequently that the wind had not the importance previously assigned to it. (Jutson, 1950, pp. 270-271)⁴

Despite this shift of emphasis away from wind action, Jutson nonetheless maintains his belief in the generalizations of Passarge and Davis employed in his 1914 work, as the following statements will suffice to show:

...The mode of formation of this new plateau has been indicated in above descriptions of the various types of valleys. Briefly stated, the plateau has resulted from the rapid attainment of a temporary high level base-level of

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The 1950 edition of the work is a reprint of the 1934 publication, with no change being made to the text, although geological maps have been updated.

erosion, by stream action and from the widening and practically the union of the valleys at that level, combined probably with the filling of deformation basins and valleys existing prior to the change to aridity.

...The new plateau apparently forms, therefore, a striking example of Passarge's great generalization as to the production of plains of erosion much above what may be termed normal base-level..., or, in other words, of levelling without base-levelling. (*Ibid.*, p. 233)

In this connection it may be mentioned that Davis... maintains that the operation of the wind in arid areas relatively increases the more level the surface becomes. (*Ibid.*, p. 266)

We note further, that it is in the section of the book from which the last statement was taken that Jutson first applies the term "pediment" to one part of the newly forming new plateau surface.

At the foot, however, of some "breakaways" and hills... the ground is practically level over wide areas, so that rain transportation is almost negligible.

The conditions mentioned are favourable to deflation, and the writer inclines to the opinion that in the narrow areas referred to, the wind is a more powerful factor than the rain in the removal of the fine detritus, although, as already stated, the actual wearing away of the cliff or hill face as a whole is due to rain action, following disintegration.

At the foot of the cliff or hill, however, the wind may corrade and so help in the destruction of such cliff or hill. No definite evidence on this point is known to the writer, and the matter, therefore, awaits further investigation.

The narrow strip of ground above referred to with the veneer of detritus forms the pediment in accordance with the meaning given to that term by Kirk Bryan.... (*Ibid.*, p. 267)

Discussion

In the foregoing sections on the formation of plateau surfaces and the role of wind we have shown that Jutson outlined a complex of processes leading to the formation of the new plateau which included the notion of parallel retreat of slopes; and that following an initial emphasis on wind as an agent of erosion, he later modified his position.

With regard to Jutson's views on slope and plain development, the following qualifications and amplifications should be made. Parallel retreat was not a universal process in Jutson's thinking, for in speaking with reference to the Salt Lake Division in which he first outlined his theory of parallel retreat, he states:

...The cliffs below the hard surface cap [laterite on the old plateau surface] are undermined by rain and insolation, with the ultimate collapse of the projecting cap. (Foot-note. When there is no hard cap, the same agents reduce the cliff face, without however, the undermining. When not protected by a hard surface cap, the slope of the hill may, according to various conditions, become more gentle, but steep faces on these uncapped hills are frequent.) (Jutson, 1914, p. 147)

And in a most significant passage, which for additional reasons given below, we present at length, we read in reference to the valleys of Salinaland whose development contributes to the formation of the new plateau:

The next type of valley is one which in a normal climate would be classed as very old, but which in the area under review (to use W. M. Davis's expression) has been "born mature", that is to say, that it has apparently from its inception never had the features of immaturity. Such a valley is very wide; has long very gently sloping sides, which from some points of view are hardly appreciable; is flat-bottomed at the middle, with irregular channels usually not more than four to six

feet deep, and often "braided"; possess no spurs; has, as a rule, hardly any channels on the valley sides; and has no meander system developed within it. The rocks forming the valley sides have no hard caps. If such caps ever existed, they have been removed long since.

The mode of development is evidently due to the fact that vertical stream corrasion has been so slow that the sides of the valley, under the influence of ordinary weathering and disintegration, with the removal of waste by rain and wind, have been cut down almost as fast. [Emphasis added] As temporary base-level is reached, the valley sides become of greater width and of decreasing slope, until ultimately a gently undulating plain is formed traversed by a small drainage channel. A number of valleys so created and reaching approximately the same stage at a considerable height above sea-level, as in parts of Salinaland, would thus form a plateau with faint and, in places, almost indistinguishable drainage lines, in which stream corrasion is of very slight character or practically negligible. Corrasion may ultimately cease and deposition take its place, which will result in raising the height of the valley floor and in a further levelling of the country, by means of which the valleys may lose their identity. (Jutson, 1950, p. 232)

In addition to illustrating that in Jutson's mind slope recession was not the only process whereby gently undulating plains could develop, this passage focusses on the relative rates of valley-side slope downwasting and stream incision and the way in which landscape morphology is thereby affected. This idea clearly foreshadows the basic idea contained in the formulation of a non-cyclic model of landscape evolution by Kennedy (1962) wherein cross-valley morphology is expressed as a function of the relative rates of valley-side slope downwasting, stream incision, and of uplift. Other workers, it should be noted, have given emphasis to the need to consider the budget of the various elements of landscape, as, for example, has Twidale (1960) in an important consideration of slope development.

Jutson (1914) applies the idea in another context. In discussing

the concurrent lowering of the old and new plateau, we read:

....It is evident, however, that in an area of arid erosion, the whole surface of the country must suffer deflation, so that while the new plateau is being widened by the destruction of cliffs, the surfaces of both the new and the old plateaux must also be in course of reduction....

There is evidently, therefore, a distinct connection between the height of the cliffs and the rate of deflation of the old and new plateaux' surfaces respectively. If the rate be about even, the difference in height between the two, will remain about the same, but if the new plateau be faster (as one would expect for similar rocks), then such difference should increase. Vice versa, it should decrease. (Jutson, 1914, p. 154)

In the 1950 edition of the work, this commentary is deleted, presumably because of Jutson's shift away from emphasis on the role of wind. But the idea to which we now draw attention is the recognition of the need to take account of the relative rates of processes operating on the various elements that constitute a given landscape assemblage.

We have, to this point, drawn attention to Jutson's ideas concerning planate surface development, slope development, the work of wind, and the budget concept as it affects landscape morphology. The following has a particular bearing on the first of these.

During the late nineteen forties and the decade of the fifties, L. C. King published a number of papers in the U.K., Africa, the U.S.A. and Australia, which led to the publication of his The Morphology of Earth in 1962. During the early part of this period Jutson corresponded with King, in 1948, and in 1950 presented King's paper "The cyclic land-surfaces of Australia" (King, 1950) to the Royal Society of Victoria.

Jutson's interest in King's work is expressed in two letters (Letters Nos. 271 & 272, Vol. II) which are here quoted in full. The first is dated June 28, 1948.

Dear Professor King,

I wish to thank you very much for sending me a copy of your presidential address entitled "Landscape Study in Southern Africa." I have enjoyed reading it. It is so comprehensive and stimulating. Of course, an outsider like myself cannot pretend to follow all the arguments and conclusions, since they are based not only on field observations but upon extensive literature, some of which I have read, particularly several of Dixey's papers.

What has interested me most is your doctrine of pedimentation as the main mode of development of landscape in Southern Africa. It recalls to my mind many of the conditions in semi-arid Western Australia. Strangely enough, before the receipt of your paper, I had been thinking of pediments in that country, but only in a vague kind of way. Your paper came as a flash of light on the subject, and I hope now to go into the matter.

Accompanying this letter in the Jutson Documents is a longhand quarto page of notes, largely quotations, made by Jutson in his reading of the paper referred to above.

The second paragraph of the letter is of particular interest. As has been pointed out above, Jutson, in 1914, described the formation of the new plateau of Western Australia by a process of parallel retreat of slopes (in addition to other processes discussed below in Chapter 2), and it was asserted that this was the first explicit statement of pedimentation and the formation of the pediplain, terms that were first used in this sense by Bryan (1922). It is odd, therefore, that Jutson should say to King: "Strangely enough, before the receipt of your paper, I had been thinking of pediments in that country....Your paper came as a flash of light....", partic-

ularly because in his 1934 edition, Jutson applied the term pediment, following Bryan, to the rock planed surfaces at the base of scarps.

One explanation is that when working and writing in 1914 and thereabouts, Jutson was thinking in terms of Passarge's theory of desert levelling to the exclusion of all other concepts, and, of course, quite oblivious of the fact that he was foreshadowing later theoretical developments. Alternatively it may be argued that for him at that time, general theoretical schemes were few in number, and with normal cycle thinking excluded for climatic reasons, he was left with the contributions of Davis, Passarge, and Keyes to provide the basis for his observations and thought. Indeed, to two of these workers he acknowledges his indebtedness in a statement on page 158 of his 1914 treatise, where we read:

...,the writer desires to acknowledge his indebtedness to the writings on arid erosion of W. M. Davis..., and C. R. Keyes, but he has endeavoured to independently work out the results of desert erosion in Western Australia from his own observations, with the assistance of any recorded facts pertaining to that country, rather than to formulate a scheme for an arid climate in general.

The second letter, written on December 13, 1948, reads as follows:

Dear Professor King,

Many thanks for your interesting letter of the 5th of August last. I must apologise for not acknowledging its receipt earlier but I have been hoping to let you have some of the references you desire. Unfortunately however, I have not come across any. You seem to have ascertained the few that are apparently available.

I shall await the development and publication of your ideas with interest. They certainly appear to be revolu-

tionary, but that is all to the good provided, of course, they have some backing of facts and sound reasoning, which I am sure will be the case with you.

P.S. I have just received a letter from Sir Edmund Teale. He tells me that he had the privilege of reading a big paper by you before the Geological Society of London. I trust that when published you will be able to forward me a copy.

The paper read by Sir Edmund Teale was King's "The study of the world's plainlands: a new approach to geomorphology" (King, 1950b)⁵, a work which King explicitly states was the outline to his then envisioned comprehensive work ultimately published in 1962.

The "revolutionary" ideas to which Jutson referred involved the proposal of pediplanation as a universal mode of plainlands genesis, and the world-wide correlation of surfaces so formed. This process involves "Penckian scarp retreat" (King, 1950b, p. 102) offered by Penck in 1924. However, C. R. Twidale first drew the writer's attention to Jutson's explicit description of slope retreat as an important process in landscape evolution in Western Australia (personal communication), and subsequent searching has confirmed in the writer's mind that Jutson, not W. Penck, deserves recognition as the first clear expositor of this important process.⁶

5

By coincidence, both Jutson and Teale (formerly Thiele, prior to World War I) were from Victoria, Teale being a graduate of the Melbourne University (Browne, personal communication).

6

Jutson's failure to recognise the significance of his own proposals has parallels in the history of other sciences, as for example, in the case of Robert Boyle. In 1661 Boyle defined elemental and compound matter in the sense propounded by Lavoisier and Dalton, but failed (as did his contemporaries) to recognise the significance of his ideas (see A. J. Ihde, The Development of Modern Chemistry, Harper & Row, New York, 1964, pp. 27-28.) Original thinkers are thus "born before their time."



Plate 3

"Dry" lake (playa lake), Western Australia.
Locality unknown. Photograph by J. T. Jutson
in the period 1911 - 1918.

Chapter 2

The dry lakes of Western Australia

The interpretation of field-work is often a strictly personal affair, depending on the observer's character, training and experience.

H. H. Read

The so-called "dry lakes" of Western Australia are widespread there, and are an important aspect of the geomorphology of the State, although particularly confined to Salinaland. They have captured the attention of geologists over a long period, beginning with H. P. Woodward's account of their form and origin in 1897, and receiving attention as late as 1974 in a letter to Nature to which we shall refer later. In these accounts, made largely by men working in the Geological Survey of W. A., several of whom were Jutson's colleagues, all possible theories for their origin have been proposed, with theories embodying aeolian, glacial, marine, and fluviatile processes all having their protagonists. Jutson (1950) summarises these. Jutson's account of their morphology and origin is important for two reasons; firstly, because it has a bearing on the development of the new plateau and of planate surfaces in general, and secondly, for the focus it gives to weathering processes.

The following general description of the lakes provides a basis for the discussion which follows:

The lakes in Salinaland cover many square miles of country, and in size vary in both length and breadth from many miles to a few hundred yards. Some of them are

isolated, but many are more or less connected in long strings; yet the individual lakes so loosely connected have very different outlines and areas. After heavy rains the lakes may be covered wholly or in part with sheets of water, which are usually not more than an inch or two to 12 inches deep....the evaporation is so great that the lakes rapidly dry up, and remain so for most of the year....On evaporation of the surface water, some salt and gypsum are usually precipitated, but as scarcely any thick deposits of these, as precipitated, are found, they must be blown away by the wind year by year; small dunes of seed gypsum and of the powdery impure form known as "kopai" occur on the borders of some lakes.

The dry floors of the lakes are abnormally smooth and level. The lakes are in some cases actually rock-floored in part at least, especially along their western borders, and these rock-floors form the "billiard table" surfaces. Other portions are covered with fine detritus, brought mainly in suspension by the surface water, the thickness of which detritus in some cases is only a few inches...

As the lakes lie chiefly along the main drainage lines of the country, the writer...had suggested that the term "stream-lake system" may be applied to the phenomena as a whole. (Jutson 1950, pp. 213-214)

...many of the lakes are bordered by cliffs...of various rocks on one or more sides, but it has been found that the cliffs occur mainly on the western, northwestern, and southwestern sides (which for the sake of brevity are subsequently included in the general term "western sides"). Sand, in the form of sand dunes and sand plains, occur on the eastern, northeastern and south-eastern side (which for the sake of brevity are included in the general term "eastern sides")....

By erosion the cliffs are in process of removal and their place is taken by smooth rock-floors ("billiard-table" floors). The cliffs, therefore, are receding to the west and the rock-floors are also growing in that direction.

On the eastern sides, the sands and silts appear to be steadily encroaching on to the lake surface owing to transportation by wind and water, so that on each side of the lake the boundaries may be in course of displacement....

If the processes of erosion and deposition described above be continued for a long period, which is likely where there is a dominant wind, a lake may be slowly entirely transferred from its original site to another position, and by the growth of vegetation the original site might be quite obscured.

The migration now appears to be westwards, but it is possible that in times past, a change in the opposite direction of migration may have occurred, and by that means, a lake, as some of the present ones are, could be in the middle of a wide flat with saline vegetation. (Ibid., 224-226)

Jutson's treatment of the lakes is outstanding for a number of reasons, paramount among which is that his description of their morphology and relationship to other elements of the landscape such as the sand plains and dunes of the region, and the buried bedrock depressions ("deep leads"), is the most comprehensive ever given. And in exploring the question of their origin he skillfully employs the method of multiple working hypotheses. Furthermore, his own conclusions are presented equivocally, not through any lack of sound logic or unwillingness to put his ideas on the line, but because in his stated view the evidence available to him demanded such a position. The following critique of the various theories proposed is presented to substantiate this.

The glacial theory proposed by W. D. Campbell in 1906 is more plausible than may at first appear, certainly more so than J. W. Gregory (1914) would have us believe. That moving ice is capable of producing rock basins is well established, and Campbell's admittedly limited experience with the lakes led him to believe that they occupied sediment filled rock basins. In addition, at this time it

was known that Western Australia had participated in the Permo-Carboniferous glaciation of Australia as demonstrated by the glacial beds in the Carboniferous rocks of the Gascoyne area. But the reasons Campbell gives to support the theory are scarcely sufficient grounds for such support in any other than a speculative, but nonetheless useful, way (useful in that, without speculation, science cannot exist). The reasons he gives are as follows.

In regard to the origin of these rock basins, there is but one agent that could scoop out hollows such as these, and that is ice; in either the form of an ice cap over the whole country or by more local glaciers....

The prevailing rounded features of all this part of Western Australia would be quite such a configuration as would be produced by glacial action.

The distribution of the obsidianites over the face of the country appear to be very irregular, and appear to point to other sources of distribution than sub-aerial. That they are volcanic ejecta is self-evident, but their source is still a mystery. The author would suggest that their distribution has been largely by means of ice drifting from some antarctic region. The greater portion of the southern seas may have been at one time covered by ice floes, which would be coated with snow; this would afford a soft bed on which obsidianites would fall and cool, and the floes might then drift to and strand upon Australian shores; the ice would in time melt and deposit them there. (Campbell, 1906, p. 23).

The case for glacial action given above does not stand up to close scrutiny. The author's failure to consider alternative possibilities for the origin of the rock basins, the rounded features, and the distribution of the volcanic debris weakens his case substantially. But in addition, to call for support in the existence of glacial evidence from the Gascoyne area (1000 km away) and from South Australia (1600 km distant) without at the same time

adducing evidence from the study area, does not represent particularly sound logic. Nor can much weight be given the statement regarding the "rounded features of all this part of Western Australia", which, apart from other considerations, as an observational statement it is itself questionable. The ice-rafting argument is nothing less than pure speculation in that there is no evidence in the area adduced in its support. It is reminiscent of Sir Charles Lyell's theory of marine intervention coupled with transportation of debris by icebergs to account for "drift" in England (Chorley *et al.*, 1964, chap. 11). Finally, the obsidianites which occur "over the face of the country" are described briefly on the previous page. We read:

The writer was shown an obsidianite,...Their occurrence is rare in this district. (*Ibid.* p. 22)

The statement speaks for itself.

In 1916, A. Montgomery published what was, and still is, the longest and completely unillustrated treatment of the lakes and their origin. Stating that there is evidence of marine action to 2000 feet ASL in Western Australia, Montgomery asserts at the outset that the lakes are the consequence of the slow emergence from the sea of a submerged peneplain, and then presents his case to substantiate the proposition. His case demands careful analysis for two main reasons. Firstly, though presented in an unnecessarily verbose style, it is nonetheless thoroughly and at times convincingly argued. And secondly, if the theory is correct, much of Jutson's work on a variety of topics dealing with the geomorphology of Western Australia is seriously undermined. We know that Jutson realised this (Jutson, 1950, p. 200), and as recently as the late 1930's and early 1940's, he engaged

in lengthy correspondence with his former colleague E. deC. Clarke on the question of the extent of the Miocene submergence of Western Australia to which we shall turn later.

Montgomery pursues two lines of evidence, the first physiographic and the second geological and geochemical. For the former, he includes the following features. The granite hills of the state rising from the plains bear striking resemblance to the granite islands off the Albany coast, and suggest an identity of sculpturing agent, and from the Fraser Range district, he describes an association of landforms that include bare rock surfaces (pediments).

In one or two places noted, the bare rock surface was seen to be cut to a quite even plane, forming a smooth floor to which joints gave the appearance of a mosaic pavement....

It is difficult to believe that wind would have planed such a hard rock to an even surface, though the occurrence is quite comprehensible if regarded as formed by beach erosion. At this place also the resemblance of the edge of the plain to a beach is most marked; it is quite level and runs in and out among the rocky spurs meeting it in the most regular manner at one level, forming inlets, bays, and every feature seen on a beach. It must be said, however, that no beach gravels were seen at this point. (Montgomery, 1916, p. 64)

As with the case of the granite hills, the resemblance of the features described to coastal form is the basis of the argument; in fact, this argument is the basis for all cases of evidence of a physiographic kind, save that of the "deep leads". We note, however, that from a logical standpoint the argument of "origin by resemblance" as it may be called, is equally valid in the converse, for it may just as well be argued that the resemblance of a coastal bare rock surface to a sub-aerial piedmont bench support the view that the former is of sub-

aerial origin. In a similar vein, the "breakaways" capped by laterite with outlying "stacks" (erosional outliers) and other associated forms "continually recalls that [scenery] seen along any sea or lake shore which is fringed with water-worn cliffs." (*Ibid.*, p. 73); likewise, water gaps are interpreted as breaches in old coastal cliffs, whilst in regard to sand hills and sand plains we read:

...it is the position and behaviour of the sandy areas that the writer has found what appear to him to be powerful arguments in favour of the theory of lacustrine or marine planation as against that of wind erosion. Sand-hills and sand-patches are found very commonly all over the country at all levels, but it is very noticeable that the biggest and best formed sand-hills, except those close to the coast, are usually,...on the high land surrounding the lake plains. (*Ibid.*, p. 81)

To this point, Montgomery's case is based entirely on morphological evidence, and although the observations are sound, the weakness of the logic and his failure to attempt to explain form in other than marine terms, renders the case unconvincing. However, his account of the plateau surface, the "old plateau" of Jutson, and of the deep leads is more convincing because he supports the case with geological evidence. With regard to the plateau surface we read:

...It may represent an older marine plain, possibly dating back to the subsidence which led to the formation of the Eucla limestone. (*Ibid.*, p. 74)

But then, he argues, it may be part of a peneplain surface formed before the subsidence which allowed the shallow sea to "carve the present features", the breakaways and associated forms. Although not explicitly stated, Montgomery is therefore postulating a marine origin for Jutson's "new plateau", as well as for the "old plateau".

He goes on to suggest that the old plateau surface may be one of immense antiquity.

It is even rather likely that this surface was to some extent the reappearance of an immensely older one, on which the beds of the Nullagine series (Proterozoic) had been deposited, for traces of these are found not very far north of this region, and the big mass of them in Mt. Yagahong shows that they must have been a thick and extensive series. (Ibid., p. 74)

It is in the southern part of the State in the Norseman area that direct evidence of marine submergence is found; and to this evidence Montgomery gives considerable weight in support of the marine hypothesis.

The most positive indication of marine conditions in the interior of our State are those already mentioned as having been found in the Norseman district near Lake Cowan. Here there are two occurrences of remains of undoubtedly marine organisms, one a thick bed composed almost entirely of sponge spicules, described by Dr. Hinde, in the Princess Royal Deep Lead, and the other a bed of marine shells in limestone, found by Mr. Campbell near the Norseman Causeway. (Ibid., p. 95)

He accepted Campbell's and Hinde's conclusions that the deposits were of post-Cretaceous age and late Tertiary or Recent age respectively, and that the former was a deposit formed in the open ocean at some distance from a coastline and probably in considerable depth of water, concluding that the Norseman site was once in deep water well out from the coast.

This might perhaps have been during the time of the Cretaceous-Tertiary subsidence, which resulted in the laying down of the Eucla limestone....The presumption would then be that the deep leads generally are most likely of an age between the times of the Cretaceous-Tertiary subsidence and elevation which gave us the

Eucla limestone and a subsequent similar Post-Tertiary down and up movement which gave the more recent Norseman beds. The leads would represent stream channels in the land surface existing between the two periods of subsidence. (Ibid., p. 95)

The geochemical evidence Montgomery uses in support of the marine theory is that derived from analyses of the groundwater found in the goldfields, which point, in his opinion, "to derivation of their saline contents from a marine source rather than directly from integral constituents of the bedrock" (ibid., p. 86). But a later passage tends to undermine his whole case based on this evidence, for we read:

Continuing northwards, freshwater wells and soaks become much more frequent and more permanent, and the salinity of other wells is generally much less than in the southern districts. North of Kookynie, fresh water wells become quite common, though the lowest lying ground is apt to be very salt, and salt lakes are still common. (Ibid., p. 88)

Plainly, the evidence cited as much supports a theory which includes undeniable marine transgression to a limited extent in the southern districts coupled with transport inland of salts as cyclic salt as suggested by others (see below). But as with other evidence cited above, Montgomery fails to argue alternative possibilities and weight them against the case for a general and widespread marine transgression of the State.

From the outline above of Montgomery's case for a widespread marine transgression it is clear that the interpretation of the fossiliferous beds of the southern part of the State are crucial to it, both with respect to the time of deposition and nature of the environment of deposition. With regard to the latter, there is little doubt that the Eucla Limestone and the Norseman shelly beds

represent shallow sea and littoral facies respectively. The occurrence of these therefore, cannot support a theory of wide-spread marine transgression deep into the State. The sponge spicule beds, however, pose something of a problem, although on their interpretation as deep sea facies Montgomery relies heavily to support his case. On the question of their age J. W. Gregory asserted in 1914 that:

The sponge spicules from Princess Royal, on the eastern shore of Lake Cowan, described by Dr. Hinde... yield no definite evidence as to age: they contain Cretaceous, Miocene, and living deep-sea species, and are as consistent with the Miocene as with the Pleistocene age of deposit. (Gregory, 1914, p. 659, footnote)

And with regard to environment, the following extract from a series of correspondence Jutson had with Professor E. deC. Clarke and Dr. Dorothy Carroll casts serious doubt on the interpretation of the sponge beds as an indication of deep sea conditions; and the doubt expressed here, in 1939, existed from the time of their first discovery.

One of us (E. deC. C.) made the suggestion of the Miocene submergence because he understood from Chapman's writing that the sponges of the Plantagenet Series are fairly deep water forms and he could not understand how, if Norseman had been 600 feet below sea-level, Southern Cross could escape submergence. Then working on a very different problem, evidence that seemed to favour this idea was got by the other (D.C.) however, we feel some doubt now about the fundamental "fact" - the deep water character of the sponges - because of the occurrence of dicotyledonous leaves associated with them. The sponges were to be re-examined at the British Museum - but no results can be expected for years now. (Jutson Documents).

J. W. Gregory's theory for the origin of the lakes presented in two papers published in 1914 and 1916 represents a more serious challenge to Jutson's views on the matter, and the essence of his case is given in what follows.

In his 1914 paper he initially outlines rival theories and quickly disposes of them. Woodward's suggestion that the thinly veneered rock surfaces of the lakes are "salt wind-planed flats" is inconsistent, argues Gregory, with the fact that the bed rock in some lakes is several hundred feet below the surface, being covered with thick alluvial material. Faced, then, with the problem of explaining the bare rock lake beds, Gregory argues that:

...The plains of bare rock described by Mr. Woodward may represent shallow lake beds from which the old alluvial deposits have been removed by the wind; or they may be areas which belong to a different category from the true lake-basins. (Gregory, 1914, p. 658)

The weakness of this counter-argument is manifest; Gregory speculates that "old alluvial deposits" once existed, and then must invoke the wind to account for their removal. Then to suggest that the lakes in question may belong to a different category without offering possible alternatives, does not help his own case. However, he goes on by outlining what he called "a modification of the wind erosion theory" in which the wind eddies which "must have a boring action on the ground beneath" are held responsible for the lakes.

...But this theory fails to explain the distribution and variations in level of the lake-basins, and it assumed the shallowness of the alluvium on the lake floors. (Ibid., p. 659)

The glacial theory of the lakes' origin as proposed by W. D. Campbell which was outlined above, is discredited by a logic which is of dubious merit, as the present author's parenthetical remarks in the following quotation hopefully demonstrate.

The South Australian glaciation was Cambrian; [this is false; even with reference to the evidence and general opinion of the day] The most widespread glaciation in Australia was Carboniferous; and the only evidence for Pleistocene glaciation on the mainland of Australia is upon the summits of the highest peaks of the Southern Alps. There is no evidence of any Kainozoic glaciation in Westralia, and from the nature of the sediments in the lake-basins it would appear most improbable that these basins can date from any Palaeozoic glaciation. [Gregory neither describes the nature of the sediments, nor gives reason why they could not be of Palaeozoic age.] The Southern basins are certainly not pre-Kainozoic, [no evidence is given to support this view] and until some evidence of Kainozoic glacial action in this part of Australia be forthcoming the glacial theory must be dismissed. (Ibid., p. 659)

The marine hypothesis of Montgomery discussed earlier is believed plausible by Gregory because of the occurrence of the Norseman limestone; but is discredited because, "...as will be seen later, the varying level of the lakes and the absence of marine fossils north of Norseman render the marine theory improbable." (Ibid., p. 659)

Having disposed of the opposition, Gregory goes on to present his own theory, and devotes the main body of the paper to its substantiation. The lakes represent dismembered river systems of probable Miocene age, for, in his own words,

...these rivers were broken up by their inability to keep their channels clear from encroaching sand-dunes during the post-Miocene desiccation, which may be explained by a reduction of internal rainfall coincident with the uplift of the country for about 1000 feet. (Ibid., pp. 663-664)

The evidence that he selects as crucial to his case is of threefold character, namely altitudinal, morphological, and depositional, as outlined in what follows.

The clue to the origin of these lake basins seems given by their altitudes. Neighbouring lakes occur at about the same level, but they have a gradual fall in one direction....

That the lakes are remnants of dismembered river systems is indicated both by their distribution and shapes. They occur in connected groups, and are usually very narrow in proportion to their length....Lake Way is represented as crescentic, and its form resembles a great river meander. The grouping of the lakes suggests that they occupy the expansions of old river valleys, most of which have been buried by alluvial deposits. The deeper parts of the basins have been probably filled by lake silts, and during a period of desiccation wind-transported drifts formed banks across the narrower parts of the valleys and thus divided a once continuous river-basin into a series of basins of internal drainage....

Various mining operations have proved in the alluvial deposits the existence of gravels which appear to have the character of river gravels; but it is of course possible that these beds might have been formed by rivers near their entrance to a sea. Such evidence as is available as to the nature of the deposits in and beside these lakes appear to me, however, in favour of their lacustrine and fluviatile rather than of their marine origin.

The most weighty arguments for the fluviatile origin of these lakes is their distribution and range in altitude....(Ibid., p. 660)

Gregory goes on to reconstruct the drainage systems that pre-dated the lakes, using cartographic and altitudinal data then available to him.

As to the age of these systems, the Norseman limestone is crucial to his case. This, for well argued reasons, is regarded by him to be of Miocene age, and

...the gradual nature of the slope, especially about the level 1000 feet above sea level, indicates that the valleys were excavated when the base level of the country was approximately at the level of the Norseman limestone. (Ibid., p. 663)

In a subsequent paper published in 1916, Gregory added nothing substantial to his case. He does, however, modify some of the details of suggested Miocene drainage lines on the basis of more reliable data provided by the Contour Map of Western Australia. South Sheet (scale 1 inch = 30.6 miles) issued by the Department of Lands and Surveys, and by handbooks supplied to members of the British Association on the occasion of the Association's visit to Australia in 1914. But the paper is noteworthy for two other reasons. First, because in it he acknowledges that C. G. Gibson (1909) had "adopted for the Ponton River the conclusion as to the age and nature of the Westralian lake basins that I reached independently the same year" (Gregory, 1916, p. 329); and second, the latter part of the paper is largely devoted to a discussion of the term "peneplain" in reference to the plateau of Western Australia and to Jutson's ideas concerning it, and to this we shall turn later.

The selections from Jutson (see pp. 51-52) embody the essence of his ideas concerning processes now operating in the formation and migration of the lakes, based on close field studies. These ideas were presented first in his 1914 treatise on Western Australia, were developed in his papers of 1917 and 1918, and presented again with some additional commentary in his 1934 edition of The Physiography (Geomorphology) of Western Australia. Distinguishing between "deformational" lakes and "erosional" lakes, the former occupying the surface of down-warped or down-faulted basins subsequently infilled by detritus of one form or another, the latter being purely of sub-aerial origin, Jutson invokes the fundamental trilogy of weathering,

erosion, and deposition to account for the observed features of the erosional lakes, notably the cliffs that commonly bound the western margins, the "billiard-table" rock-floors of the western margins of the lakes, and the salt and silt accumulations on the eastern flanks. Weathering, notably exsudation (flaking of rock by salt crystallization), operates on the face of the cliffs, at the cliff-base, and on the rock floors, the products being removed mainly by deflation during dry periods and accumulate on the eastern margins; the cliffs thereby retreat and the rock floors expand. And with encroachment of detritus from the east onto the lake bed, the whole system migrates westward.

The development of Jutson's thinking about the lakes is not difficult to trace, and makes for an interesting tale. We have already seen in reference to the origin of the new plateau surfaces that Jutson attributed an important role to wind. So too, in his original theory of lake migration the wind plays the dominant role.

Migration of the salt lakes of Western Australia could arise in the following way:- A lake may be bounded on one or more sides by cliffs ("breakaways"). Owing to some prevailing wind, the surface at the foot of one of these cliffs, say on the eastern side, is subjected to strong wind erosion, and as the cliff recedes, this surface is planed down to the level of the lake floor. (Jutson, 1914, p. 155)

In this statement the idea of cliff recession is an original one, but the idea that the rock surface is then planed down by the wind is an idea that goes back to Woodward, as we have seen.

In his 1917 and 1918 papers, the ideas presented in rudimentary form become expanded, and far greater attention is given to details



Plate 4

Western margin of a "dry" lake, Western Australia, locality unknown. Undercutting of cliff by exsudation (salt weathering), and retreat of cliff to form the "billiard-table" rock floor of the lake. Photograph by J. T. Jutson in the period 1911 - 1918.

based on field studies. Perhaps the most important new idea to appear is that concerning weathering; exsudation is assigned an all-important role in the formation and retreat of the cliffs and the modification of the rock floor of the lake. Jutson's attention to mechanical weathering by salt crystallization was drawn by J. Walther in 1914 at the time of the visit of the British Association (see Letter No. 371, Vol. II), as he declares in the opening remarks of his 1918 (a) paper on rock weathering.

Jutson has his final published say on the subject in his book (1950). In this treatment no new ideas or field data of his own are added. However, he does incorporate new data from other sources in this his most thorough analysis of the theories that rival his own and presented on pp. 214-224 of his book. In order to do justice to Jutson's cases against the marine and ancient river theories which pose the most serious challenges, the following extracts are presented.

The absence, however, of marine deposits over most of Salinaland, although negative evidence only, was a serious bar to the acceptance of the marine theory; but that objection was answered by the assumption that erosion had removed the marine deposits over wide areas. That assumption, therefore, implied a considerable lapse of time since the great uplift, but there was the contrary assumption that the breakaways and rock-floors were practically the same as when they were moulded by the sea, implying, of course, a very short period of time since the great uplift.

Those who opposed the marine theory, from a study of the evidence of the amount of subaerial erosion which has taken place since the great uplift, contended that a considerable time had elapsed since that event and that the "breakaways" and rock-floors were the result of that erosion. The recent determination of the Norseman limestone as of Miocene age is not opposed to that contention. Thus the first named assumption in support of the marine theory may be true, but there seems little doubt that the second is false.

The study of the chemical composition of the underground waters appeared to strengthen the marine theory; but doubt has been thrown on the validity of the inference that such composition was the direct result of an invasion of the sea in Miocene times, owing to the possibility, as suggested by Talbot..., that the salt may have been derived from Palaeozoic rocks since removed by erosion, and to the fact that large quantities of salt are now being carried inland from the ocean by the wind and precipitated with the rain....How much is carried into Salinaland is not known, but it may be assumed that all the salt carried by the wind is not deposited in the coastal districts, and therefore, that a portion is being carried to and deposited in Salinaland.... Further investigation of this important matter is desired, and the results are awaited with interest.

The occurrence of the gasteropod Coxiella and the claim made that it proves the existence of a Miocene sea far inland, have been referred to...If such claim be valid, then there has been a widespread invasion of a Miocene sea. It appeared, however, to the writer that the evidence was too slender to justify such a generalization....He therefore referred the matter to Mr. F. Chapman, A.L.S., F.G.S., the Commonwealth Palaeontologist, and that gentleman has kindly submitted the following statement:-

"The argument in favour of Coxiella being a thalassoid type, as held by the late Chas. Hedley, is its supposed external resemblance to the marine shell Truncatella. Proof of relationship should be based on the structure of the radula, which although known in the case of Truncatella, seems not to be known in Coxiella.

"The arguments in favour of Coxiella being a typical freshwater genus with adaptive powers of living under brackish conditions are -

- (1) The association of undoubted typical freshwater genera of other groups, as species of the bivalved ostracod Cypris.
- (2) The association of the gasteropod Bythinella, typically a freshwater genus, of streams and ponds.
- (3) The occurrence of Coxiella far inland, where no later invasion of the sea had occurred probably since Cretaceous times, as at Lake Eyre.

"The conclusion from the above points in evidence is that Coxiella appears to be a freshwater mollusc that has adapted itself as much, or more than any other aquatic species, to brackish water conditions; the marine transgression theory, therefore, is not supported by the facts."

Mr. Chapman's weighty remarks need no comment. (Jutson, 1950, pp. 221-222).

So rests Jutson's case against the marine theory. His case against the ancient river theory is of a different nature and consists of two main elements. Firstly, the ancient river theory does not uniquely account for what Gregory regards as the most weighty evidence in its favour, ("the general southerly fall of the country"), and secondly, the principle of uniformitarianism is implicitly invoked with considerable power. We read:

J. W. Gregory...had pointed out that the lakes conform in altitude to the general southerly fall of the country, which fact, he contends, makes the marine theory improbable. It is, however, equally confirmatory of the Miocene river theory, and of the theory advanced by the author, that the forces at present operating have been mainly responsible for the formation of the erosion lakes.

The ancient river theory fails to recognise that wide long valleys are being formed at the present time, that they are growing headwards, that capture or union of one valley and of one lake with another is taking place, and, in short, that drainage systems, although interior, often indistinct and often disconnected, are in process of formation and development, despite the accumulations of sand, which have in the past broken up, and are now breaking up other systems or portions of the same systems, of drainage. Hence the fallacy of assuming that because one lake may be connected with another by an ill-defined channel, that channel is necessarily evidence of the existence of an old river valley. It may be, but it may equally be evidence of a channel that has been cut by headward or other erosion from the one lake to the other since, and not prior to, the formation of the two lakes. Perhaps, however, as investigation proceeds, these ideas will be more widely accepted, just as the formation of the rock-cliffs and of rock-floors, and the migration of lakes, under present conditions, have been recognized. (Ibid., pp. 223-224)

As indicated above, the selections given were drawn from what was to be Jutson's last published statement on the lakes. However, over the period 1939 to 1942 he involved himself once more with the question of the extent of the Miocene submergence of Western Australia in a series of communications with his former colleague E. deC. Clarke, then Professor of Geology at the University of Western Australia, and Dr. Dorothy Carroll of the same university, who, in the course of research on the sand-plain soils of Western Australia, adduced evidence that had a bearing on the submergence question. Although the debate took place during 1939 and 1940, a post script on a letter to Jutson from Clarke which reads:

Did I tell you I got several Aturias [?] lying on the surface east of Kennedy Range - and others got a few Kainozoic looking pelecypods? So here's for the next debate on the "Miocene Submergence". (Letter No. 76, Jan. 12, 1942, Vol. II).

Jutson did not take up the challenge judging from available evidence, and we can suppose that he remained unconvinced of the theory of widespread marine transgression. The sequence of communications now follows.

The Miocene submergence debate

The first item in the sequence is from a letter to Jutson, followed by a preliminary draft of a statement for the Structural and Landform Committee of ANZAAS. These introduce the topic.

I am sending you under separate cover [a] retype of the paper on Sandplain soils - on which I would greatly appreciate your criticism. [see Letter No. 72, dated Feb. 21, 1939, Vol. II]

A.N.Z.A.A.S.

Structural and Landforms Committee.

Extent of the Miocene Submergence.

"On the 'Great Plateau of Western Australia' there are large areas covered with sandy soil which are known as 'Sand Plains'. They have been supposed by some writers to be the remains of sand dunes now levelled by the wind; by others to be soils formed by the weathering, in situ, of granite; by others to be the remains of soils of podsol type, formed in some previous epoch. The mineralogy of sand-plain soils has recently been investigated by Dr. Dorothy Carroll as one of the 'projects' of the Commonwealth Research Grant to the University of Western Australia. Those aspects of her work which bear directly on the mining geology of the area examined will, it is hoped soon be published as part of a bulletin of the Geological Survey of Western Australia. Some of her results however, which may be of interest to this Research Committee, are not of direct value from the mining point of view and are not dwelt on in her contribution to the bulletin. Dr. Carroll has very kindly consented that these should be communicated to this Committee.

"The area examined is near Southern Cross (long. 119 20 E, lat. 31 15 S) and covers about 1000 sq. mi. No part of it is less than 150 mi. from the Southern or from the Indian Ocean.

"Dr. Carroll notes that thin beds of grit occur in many parts of the sand plains of this area. They contain small, waterworn pebbles of quartz, and, occasionally of felspar. Laterite pebbles

are also found in some places. These grits lie on the worn surface of the Pre-Cambrian rocks. Ferruginous ('lateritic') gravel occurs in many places forming a horizontal layer, 6 inches to 2 ft. below the surface of the sand-plain.

"After detailed study of the mineralogy of the sand-plains Dr. Carroll concludes that (for fuller explanation see Appendix), firstly, with trivial exceptions, they are not formed from wind-blown material, which disposes of the suggestion that they are the remains of sand dunes; secondly, the rocks from which they have been formed in situ are in part metasediments, which disposes of the suggestion that the sand-plains are underlain entirely by granite; thirdly they have been formed by some process not now in operation.

[Emphasis by Jutson]

"She considers that their origin may be explained as follows:- In some previous cycle a peneplaned surface had been formed on the Pre-Cambrian rocks; this was then gradually submerged. Such submergence would lead to the formation of a thin sandy series, which, if the rate of submergence exceeded the rate of supply of material, would not have been subject to much transport or sorting (Grabau, Principles of Stratig. 2nd. edition, p. 731). In this way the series of bedded grits was formed. Soils of the podsol type were formed from these grits - podsoles form most freely in sandy, open-textured, badly-drained material, in a humid climate. Such conditions would, she considers, obtain during the early stages of a period of emergence. It is interesting to note that J. A. Dunn has suggested a similar origin for grits and laterite on a peneplaned surface

in India. Jour. & Proc. Asiatic Soc. Bengal (N.S.) vol. XXIX
no. 4, p. 285)

"It will be seen then that the last epoch in the history of this area, as deduced from a study of its soils, is not incompatible with the extensive Miocene submergence and subsequent events already suggested to this Research Committee (A.N.Z.A.A. Sc. vol. XXII, p. 466 et seq., 1935). [MSS C 128(1) - 128(3)]

Appendix

Notes on Mineralogy of Yilgarn Sand-plain Soils.

by Dorothy Carroll, Ph. D.

"When a rock weathers to a soil some of its minerals are so resistant to chemical change that they are passed on to the soil unaltered. There are two groups of such resistant minerals:-

1. Quartz and some of the feldspars.
2. The 'heavy minerals' - so-called because their specific gravity is greater than that of bromoform (about 2.9) in which they sink. Here belong such minerals as amphiboles, garnet, rutile tourmaline, zircon, etc., etc.

"Now the soils of any district may be either 'sedentary' i.e. have been formed by the weathering in situ of the basement rocks of the district, or 'transported' i.e. formed by the weathering of superficial deposits laid by wind or water on the basement rocks. If they are sedentary the vast majority of their heavy minerals will have been derived from the underlying rocks. Therefore in a study such

as this it is necessary to know the heavy minerals characteristic of the principal basement rocks of the district, and, to this end, several representative types of these rocks were crushed and the heavy minerals separated out with bromoform.

"The particulars obtained of the heavy mineral assemblages of the Yilgarn sand-plain soils show that more or less the same minerals are found in all the soils. A closer inspection however, shows that the mineralogy of individual soils varies in two important respects: first the relative number of grains of each of the minerals varies from soil to soil (to ascertain this the grains in the heavy fractions have to be counted); and secondly, the minerals themselves often exhibit characteristic forms and colours. Examples of this 'poly-varietalism', as it is called, are found most noticeably in zircon, tourmaline, garnet, amphibole and rutile.

"It was found that both the relative amount and the varieties of the heavy minerals give important evidence as to the origin of these soils, and in addition the actual proportion of the whole soil which is made up of heavy minerals is often a criterion of origin.

"In this district there are three main types of rock concerned in soil formation: the gneisses and granites; the greenstones; and the meta-sediments. Soils formed from these show the following heavy mineral characteristics:-

"Soils derived from gneisses and granites have zircon prominent, with rutile, garnet and amphibole.

"Soils derived from greenstones show abundance of amphibole with or without an increased amount of opaque grains, including an

increase in magnetite, and subordinate or no tourmaline and andalusite.

"Soils derived from the meta-sediments show abundance of andalusite and staurolite, with subordinate tourmaline and zircon, and with small though constant amounts of spinel.

"From a consideration of the mineralogy of these soils from the sand-plains it is found that the soils have been formed by weathering in situ of granites and/or gneisses, and meta-sediments.

"It has been suggested that the sand-plains may be dune-sands spread out by the wind. Dunes have a definite grade of sand grains i.e. contain those grades which are most easily moved by the wind. The sand-fraction of the sand-plain soils was shown by mechanical analysis to be poorly graded; the graphs obtained were not similar to the curves which would be obtained for soils formed from eolian deposits. Moreover most of the grains in the sand-plain soils are angular to sub-angular rather than rounded (i.e. there are fewer rounded grains than there are in typical sand dunes).

"In this part of Western Australia there are two distinct periods of soil formation: present-day soils [formation, which produces (JTJ)] normal brown to reddish brown loams and clays [and sandy clays or depending on the character of the parent rock (E. deC. Clarke)] in the more low-lying parts (unquestionably formed by weathering in situ of the country rocks); and [soil formation of

a former physiographic cycle, which produced (JTJ)] the yellow to pale brown sands on the plains which contain a lateritic layer absent from the present day soils. Field occurrence indicates that the sand-plains [under discussion are covered with (JTJ)] residual soils which remain to-day because of the low relief and scanty rainfall of this part of the country. Occasionally in some of the sand-plain areas we find granite outcrops surrounded by newly formed pinkish brown soils [which (JTJ)] are not at all similar to the yellow soils of the sand-plains themselves. This indicates that the normal weathering of a uniform expanse of granite could not, under the present climatic conditions, give rise to a typical sand-plain soil."

[MSS C 128(1) - 128(7)]

This MSS is followed by MSS C 128(8) to 128(11) dated March 15, 1939, being a note by Jutson on the sand plain soils question.

Note re the Sand Plain soils of Western Australia

"Dr. Carroll's paper is most interesting, since it approaches the problem of Tertiary submergence or non-submergence of most of Western Australia from a new angle. There are, however, various points to be considered before in my opinion it can gain full acceptance.

"I have not seen the reference to Grabau or that to Dunn, so that these notes may fail to appreciate the full significance of Dr. Carroll's work.

"Dr. Carroll deals with the three predominant rock classes of

the goldfields, granites, greenstones and meta sediments and she seems to show clearly that the soils of the sand plains have been formed by weathering in situ of granites (or gneisses) and meta sediments.

"If these soils are due to weathering in situ then they must have been formed prior to the assumed submergence and Dr. Carroll thinks that the conditions may have been such that there would have been little transporting or sorting during submergence. (Note (12) the submergence was gradual and yet the rate of submergence should exceed the rate of supply of material). Surely, during submergence and/or emergence there would be some sorting out and the different classes of sands would tend to lose their identity. It seems almost impossible to believe that sands formed by weathering in situ would retain their positions over the same rocks during both submergence and emergence.

"You point out in your 1935 A.N.Z.A.A.S. Notes that Norseman in Miocene times was at least 600 feet below the sea. The sea bed can be eroded at that depth (vide 'Johnson's Shore Processes and Shoreline Development' pp. 80-83)

"Presumably these sandy beds are intended to be correlated with the Norseman shell beds and the sponge spicule beds found elsewhere. Is it not strange therefore that so far no fossils have been found in them?

"On p. 6 of your notes, it is stated that there are two distinct periods of soil formation in the part referred to in Western Australia. Have you found one soil lying on the other, or do you

find the two soils in separate places?

"You say also that the sand plains are covered with residual soils which remain to-day because of the low relief and scanty rainfall. But suppose these soils were formed without submergence during a wetter climate. They would no doubt be constantly in course of removal, but they would also be in course of formation, just as is happening in any normal climate to-day, provided of course the rate of removal is not extremely rapid, as when man takes a hand by mutilating nature. Then when the change to aridity took place the type of soil would be there and the conditions postulated by you might prevent its removal. The later pinkish brown soils would be formed under present conditions, but they would not be so leached as the earlier ones; hence perhaps the difference.

"Your notes state that present-day soil formation produces normal brown to reddish brown loams and clays and sandy clays or depending on the parent rock in the more low-lying parts. You would apparently therefore exclude the present formation of all sandy soils (except perhaps in the limited areas around the granite to which you refer) but surely sandy soils are being produced at the present day.

"We know that if there has been Tertiary submergence then since re-emergence there has been great erosion. How is it therefore that these old sandy soils have been so widely preserved?

"It has been suggested (you say) that the sand plains may be dune sands spread out by the wind. I do not know of any published statement to that effect and I should be glad to have the reference.

You and Talbot have however suggested (Bull 75, p. 41) that the heavier sand from a granite area of sand plains may be due to the wind acting as a leveller-up rather than a dune builder and I have shown at Comet Vale that the small inclined sand plain there must be due to wind action (Paper in the Roy. Soc. of Vic. about 1917 or 1918; if you have not a copy I can send you one) Hence we can have transport by the wind without the formation of dunes.

"I drew your attention to some of the sand phenomena at the Fishermans Bend area on our recent visit there, but unfortunately we did not have time to go into the matter. I am satisfied from my observations there that the wind can not only be a great sorter out of the fine material from the coarse but that it can heap up the coarse sand into ridges an inch or two high, the finer material being in part blown away. Hence if we could have a series of layers formed in this way and then exposed in section, there would be a series of coarse and fine sands apparently formed under water and very current bedded. If there were no dominant wind for a long period then the sands might be drifted to and fro without any great change in their position. They might thus be true residual sands.

"In the dry areas of Western Australia if the soils referred to are residual then the removal of the finer lighter particles could be accomplished by the joint action of the wind and of the rain. The gentle action of the latter on ground of little inclination, is in the long run extremely effective, as I have endeavoured to show in the second edition of the Western Australia Physiography.

"Perhaps what I have said does not present a definite answer to Dr. Carroll's hypothesis and perhaps I do not appreciate fully all the points you and she make but I cannot help feeling that there is a screw loose somewhere. Perhaps also I am prejudiced against submergence, although I have tried to weigh fairly all the arguments in its favour contained in your notes. However, my ideas are submitted to you for what they are worth." [MSS C 128(8) - 128(11)]

The following letter does not bear directly on the soils question, but does relate to the submergence debate and for this reason is included here.

My dear Jutson,

Thanks for your appreciative letter regarding "Middle and West Australia" which I got when in camp at the Irwin River a fortnight ago. It is good to hear of one person who confesses to have read it through.

Regarding the Beach Deposits on the Nullarbor Plains you will find that in the annual report of the G.S.W.A. for 1919, p. 5, Talbot speaks of the "uplifted estuary into which Goddard Creek flowed". I wrote to Talbot about it in 1934 and he replied as follows:-

Dec. 6th, 1934.

"It is such a long time, over 18 years, since I was at Goddard Creek that my recollection of what I saw is rather hazy, but the estuarine appearance of the country below where the creek ended is still impressed on my memory. The creek emptied on to a flat on which were banks of seed gypsum. There was a low limestone cliff on the east side of the flat, and I think a somewhat lower cliff or rise on the west side. On the flat shells were thickly strewn between the gypsum banks. I brought back a collection of the shells, and also some sample bags of the seed gypsum, but what was done with them I do not remember. There ought to be some record of them in the G.S.W.A.

"Goddard Creek has a well defined channel and a sandy bottom until it runs out on the flat mentioned above.

There is a good fresh water sand soak in the bed of the creek a few miles below the railway. I remember I did not see as much of the country as I wished as I had a very severe attack of lumbago caused by putting on pyjamas the string of which was not dry after being washed at the soak where we had a day's spell.

"Goddard Creek is the outlet from Lake Carey."

For some forgotten reason I did not refer to this apparently in my report to the Structural and Land Forms Committee in 1935, although I actually told Talbot when I wrote to him that it was for this that I wanted the information.

Andrews wrote to me recently that his MS. and maps on Eastern Australia have been sent in, so the paper should be in print before very long. [Letter No. 73, Vol. II]

The following letter, dated May 8, 1939, and accompanying reply by Carroll to Jutson on the sand plain soils issue are self explanatory.

My dear Jutson,

Yours of May 1 is just to hand. I am truly sorry & ashamed that I have overlooked acknowledging yours of March 15 & all the interesting matter connected therewith. It arrived when I was arranging a senior geology excursion to the Irwin River. Dr. Carroll wrote a reply to it - to which I was supposed to add something: I did not before leaving for Irwin R. When I got back I would, if asked, have said that I had posted it off - but here it all is!

There is a great deal in your objections but I think you will see that there is something for the submergence too - naturally we are all of us looking for the truth & I hope I shant mind if you end up by showing us to be all wrong! There are just one or two points in addition to Dr. Carroll's notes:- bottom of your p. 2 "conditions postulated by you" - does this refer to the idea of submergence? or climatic change?

first para. on p. 3 - "and sandy clays etc. depending on the character of the parent rock" should be added after "reddish brown loams & clays." In this place Dr.

Carroll & myself were considering rather the climatic conditions (wh. of course influence the character of the soil quite independently of the influence of the lithology of underlying rock) but we should have put in something about sandy soils as you point out.

I have been interrupted several times while writing this, & think it's better to post than to wait for a "more convenient season."

Many thanks for all your interest & the notes about S. Melbourne are most valuable. [Letter No. 74, Vol. II]

Sand Plain Soils of Western Australia

A reply by Dr. Dorothy Carroll

"The investigation of the grits in the Yilgarn goldfield is by no means complete, and the suggestions put forward can only be regarded as tentative. Nevertheless it seemed worthwhile placing the occurrence of these grits on record. The relationship between all the sand-plains and grits has not yet been worked out. The idea that the grits were of marine or lacustrine origin was suggested to me by a passage in Grabau: 'Where transgression takes place over an old peneplain surface, on which residual soil has accumulated during the long period of exposure this ancient soil may be incorporated, without much change, as a basal bed'. And again on p. 733, 'The falling sea-level or rising land block is accompanied by a continuous regression of the seashore and a consequent sea-ward migration of the shore zone with its attendant deposits. If the emergence is not too fast the waves will be able to remove much of the formerly deposited shore detritus and carry it seaward into the shoaling off-shore districts'. On p. 276 an example is given in which transgression takes place with the rate of supply equal to the rate of subsidence.

'Where atmospheric agencies have been sufficiently active to disintegrate a granite surface without, however, reducing the felspar to clay, the basal sandstone will be a feldspathic arenite or arkose. Again examples are known where decomposition has affected the underlying crystalline old land to a considerable extent, but where little sorting of the material was accomplished by the transgressing sea, so that the basal bed is a highly argillaceous arenite. The contact of this with the underlying crystalline basement rock will in consequence not be a sharp one, the crystalline rock grading through a decomposed zone into the overlying sandstone. An example of this indefinite type of contact is seen on Presque Isle near Marquette, Michigan where the Lake Superior sandstone passes downward into a rock produced by the consolidation of the undisturbed disintegrated surface of the basal peridotite'.

"The connexion between the Norseman spicular beds and these grits needs to be clarified. Coming west from the Nullarbor Plain there is a series of sediments which was laid down in a shallowing sea. The rock types vary from the limestone of the Plain to fine sandstone and occasional gritty and conglomeratic beds in the vicinity of Salmon Gums. Spicular beds also occur here, and some of the material has been sent to the British Museum so that the approximate depth of the sea in which it was deposited may be known. Outcrops are scarce, but further detailed field work will probably reveal many more. The grits of the Yilgarn are not considered to be contemporaneous with the Norseman but younger, as this part (Yilgarn)

of the continent was then still slightly above sea level and may have been swampy or marshy land. Recent evidence on the sponge-spicules at Norseman shows that these may indicate a shallower depth than 600 ft., although recent dredging off Esperance showed that sponges were most abundant at 90 to 100 fathoms. The lack of fossils in the Yilgarn area is no conclusive criterion against marine conditions, for grits, especially thin beds of grit, are poor preservers of fossils, and with few exposures remains are liable to be missed. If a lacustrine environment be postulated then the life to be preserved would be scanty. It is felt that when more work has been done on the sediments of the Salmon Gums region to the east and the Plantagenet series due south, it will be shown that there is a thin skin of Tertiary rocks which thickens to the east and south. The small thickness of these grits indicates that there was little material to be distributed, and from this it is seen that the land must have been brought nearly to the base-level of erosion, for otherwise there would have been beds of clays, conglomerates, alluvium, and thicker beds of sand in the piedmont zone. The evidence so far, though this is not complete, shows that this is not so.

"Support is given this idea by Prescott's work on the soils. When discussing the occurrence of lateritic soil profiles in W.A. he mentioned the fact that laterites usually occur at the time when the land is more or less level and at the base-level of erosion. He states: 'The sand-plains of W. A. do, however, strongly suggest complete fossil soils. When once this surface horizon (i.e. the sand)

has been removed, the persistent ironstone would weather very slowly and only under heavy rainfall conditions would it either break down to a new soil or be entirely denuded away'. 'Jensen recognises definitely that the lateritic cappings of the tableland sandstones of Northern Australia are early Tertiary in age preceding an uplift'. 'In the case of the sand plains occurring extensively to the east of the river valleys of the Darling Range, it is probable that the surface denudation has been at a minimum, and that with minor redistributions of material the whole fossil soil profile is preserved'. The mineralogy of these soils supports Prescott's idea. And these are the two ages of soil formation: the sand-plain lateritic soils formed prior to uplift; and the normal reddish brown soils forming at the present time. These types of soil are found in separate places, not overlying one another. The fossil soils now sand-plains could have been formed without submergence in a wetter climate, and this would have had to have been prior to uplift, otherwise the remains of drainage channels would be more marked than they are, and there would be more pebble and gravel beds around these channels. There are undoubtedly some beds of drift gravel and grit, and these are in the positions where they are expected, e.g. Lake Koorkoordine. But very often in the lateritic layers of the sand-plains a coarse grit was encountered, and the soil had apparently weathered from this. Such grits could be readily accounted for as recemented weathered bed-rock if the grains were angular, but in every specimen seen some of the small pebbles are well-rounded, and such rounding implies water action. It may be argued that such rounding means considerable

transport, but the mineralogy of the soils reflects in a general way that of the underlying rock. A glance at the geological map of this area shows that there are large areas of granitic rocks. Slight sorting in any of these areas would not affect the general 'granitic mineralogy' of the samples examined. In 100 square miles of granite minor redistributions of material would be negligible.

"You say, 'We know that if there has been Tertiary submergence then since re-emergence there has been great erosion'. But do we know this? The very fact of the preservation of these old sand-plain soils, irrespective of whether they were formed from the grits or the grits + crystalline basement rocks indicates a slowing down of the forces of erosion. It is considered that the peneplain was formed at a very much earlier time than since the Miocene uplift. It is a resurrected peneplain, and as such would not offer a good surface for erosion.

"Montgomery is responsible in part for the idea that the sand-plains may be dune sands spread out by the wind. He gives no references but the idea seemed to be prevalent at the time he wrote (Royal Soc. W.A. vol. 11, p. 81, 1915-1916). When speaking of wind deposition he states 'It may seem the most unkindest cut of all to the supporters of the wind-erosion theory to claim their sandhills in support of that of water planation.....' and again he states 'and just as we often find marginal sand dunes round the existing salt lakes, so the main belts of sandy country correspond with the position in which we might expect sand dunes to have been formed round the older and larger lake beds, which now form the brown soil plains. They are still in

position in which they were formed when the lakes were filled with water and are therefore on the high country separating the lake basins'. The mechanical analyses of the sands and the grain sizes and degree of rounding are against such a dune origin, although in isolated instances wind action is evident." [MSS C 128(12) - 128(15)]

Jutson now replies to Clarke and Carroll, and they in turn present their final statement on the issue.

My dear Clarke,

I have your letter of the 8th inst. with the reply by Dr. Dorothy Carroll, which I shall peruse and send a reply if anything further is to be said.

You need not worry over not acknowledging my letter of the 15th March last for I knew that you would intend to acknowledge it immediately and that some oversight had caused its want of acknowledgment if received by you. I was anxious, however, to know that you had received my letter and the accompanying papers. [Letter No. 208, dated May 16, 1939, Vol. II]

My dear Clarke,

On the 16th of May last I acknowledged the receipt of your letter of the 8th of the same month enclosing Dr. Dorothy Carroll's reply to my queries and stating that I would peruse same and send a reply if anything further was to be said.

I have intended writing to you in regard to the matter for some time past but one urgent thing after another has turned up and delayed me.

Now firstly in regard to your own points:

(1) Bottom of my p. 2 - "conditions postulated by you." These are the present conditions of low relief and scanty rainfall by reason of which you say that the residual soils remain to-day.

(2) The insertion of "and sandy clays &c. depending on the character of the parent rock" after "brown loams and clays" clears up my query.

Dr. Carroll's reply

(3) Since I have not seen Grabau, I am very glad to have the quotations so kindly supplied by Dr. Carroll. Grabau sets out what might take place on submergence or emergence but it seems to me that these statements only apply when the fact of submergence or emergence is independently proved. Take the various quotations from Grabau.

(a) "Where transgression takes place over an old peneplain surface, on which residual soil has accumulated during the long period of exposure, this ancient soil may be incorporated without much change, as a basal bed"

If we admit that this may be so, could this old soil not be a basal bed without transgression by change of conditions, such as climate, uplift, a sudden increase in the drainage area by river capture, with consequent increased sediment brought into the original area, or deformation and formation of a lake? It seems to me to be unsafe to deduce submergence from the facts stated.

(b) p. 773 - "The falling sea-level or rising land blocks....off-shore districts".

This seems to be a perfectly true statement, but the difficulty is to demonstrate it in any particular area. It does not appear to me to be demonstrated in regard to the sand plains.

(c) p. 726 - Example of transgression where the rate of supply equals the rate of subsidence. This I presume Grabau demonstrates.

(d) "Where atmospheric agencies have been sufficiently active....the basal sandstone will be a felspathic arenite or arkose".

Precisely, but this does not prove submergence.

(e) Decomposition affecting crystalline old land &c. Little sorting by the sea. Contact not sharp, the crystalline rock grading through a decomposed zone into the overlying sandstone. Could not this occur in a lake or on a flood plain? In the absence of other indications of a marine transgression, the freshwater fluvial or terrestrial origin seems more likely.

(4) "Coming west from the Nullarbor Plain there is a series of sediments which was laid down in a shallowing sea".

That is in effect what I have said myself (see Bull. 95 p. 202), a sinking area forming the old Eucla Gulf and then a final wider shallow submergence giving the Norseman and Albany Marine beds as comparatively shallow water deposits, but this does not mean a submergence over more than half Western Australia. I quite agree that if the Yilgarn grits turn out to be marine, they are probably younger than the Norseman beds. That would agree with my ideas just outlined, and if they are shallow water then the land to the north may never have been covered. Dr. Carroll thinks the grits may be lacustrine; hence our ideas are perhaps approximating.

(5) Prescott's work -

Prescott agrees with Woolnough that the laterites were formed at base-level. Before either of them I showed in 1914 (Bull. 61) that the laterite capped the peneplain, although I did not state that the laterite was formed at base-level. I see no objection to the contention, although I have not sufficient personal knowledge to say yea or nay, that there are two ages of soil formation, one formed prior to uplift and the other subsequent thereto, but this does not of course indicate submergence. Dr. Carroll recognizes this point when she states that the fossil soils could have been formed in a wetter climate without submergence, prior to uplift.

(6) Well-rounded pebbles in the grits. This Dr. Carroll truly states implies water action and that it may be argued that such rounding means considerable transport. It is certainly difficult to imagine well-rounded pebbles without much transport. Could not these grits be fluvatile, laid down during the meanderings of rivers in their old age on the peneplain and could not the rounded pebbles be derived from earlier beds? I suggested this latter possibility in connection with some abnormally rounded quartz pebbles in the Darlot deep lead (Bull. 95, p. 209). It seems to me that if we admit that the laterite was formed on the peneplain at base-level, we must have very strong evidence before assuming wholesale submergence since.

(7) My statement that if there has been Tertiary submergence then since re-emergence there has been great erosion. Is this not shown by the deep valleys of Swanland and by the interior valleys along which the salt lakes lie. Even if it is contended, with Montgomery (now supported to some extent by you) that these lake valleys were formed during the Miocene submergence, every one acquainted with these lakes must admit that there has been extensive erosion, resulting in the migration of the lakes as well as

the deepening and lengthening of the valleys.

(8) Age of the peneplains. I have expressed the opinion in my physiography of Western Australia (Bull. 95, p. 204) that the peneplain was completed during or prior to Miocene times. As for being a resurrected peneplain it may be in part, but I have not sufficient knowledge of the later known facts - which may support this idea.

(9) The idea that sand plains may be dune sands spread by the wind. It appears to me that Montgomery's somewhat obscure statement is susceptible of a different interpretation from that given by Dr. Carroll. He claims the sand-hills in support of water planation and his subsequent remarks (quoted by Dr. Carroll) indicate that there are marginal sand dunes around the existing salt lakes and also marginal sands around what he regarded as the sites of older and larger lakes. He compares the two groups of phenomena and, since he is arguing against wind erosion, he maintains that the brown soil plains are not the result of wind erosion but were laid down in the big old lakes and hence are of water origin. He does not, nor does anyone so far as I know, dispute that the sandhills around the present lakes, are dunes. What then does he mean when he states that the sand hills are against the wind erosion theory? To my mind he refers to the ones around the old big lakes and his reference to belts of sandy country is a very loose way of indicating the "sand hills". Whether they are dunes or not, I cannot find any indication that he regarded the sand plains as dunes spread out by the wind.

In conclusion I might refer you to some statements by Henri Baulig as to what happens to a decomposition cover on a peneplain on transgression. See Journal of Geomorphology, March 1939, pp. 126-27, footnote 6.

Dr. Carroll's work is very instructive and suggestive and has given me much food for thought. My criticisms are made in no carping spirit and she may later be able to convince me, but at present I must remain an appreciative sceptic. She admits that the position at present is tentative. That being so, I hope that she will pursue her studies from the valuable new angle of approach that she has discovered.

re Goddard Creek shells

In the absence of an examination of the shells collected by Talbot, do you think it wise to assume that they are recent and that consequently in very recent times, the plat-



eau has been uplifted? This is something entirely new and would want strong evidence in support.

I would be glad to receive a copy of Dr. Carroll's Geol. Mag. paper. [Letter No. 209, dated Sept. 7, 1939, Vol. II]

My dear Jutson,

...Enclosed is the long delayed continuation of our discussion on the Miocene submergence (?). The delay is entirely my fault - Dr. Carroll wrote her part - which is the majority of the combined effort - months ago. I need add nothing more except to say how much we both appreciate the chance to discuss things with you, to express the hope that you will overlook any crudities of expression that may have crept in and that you will feel inclined in your own good time to continue the debate. We hope also that we are really trying to debate the merits of the case and not indulging in a debating society stunt - of course by "we" I mean Dr. C. and myself. You will see that I am feeling doubtful about my thesis for one or two reasons. It would be a great thing to have the ecology of the sponge fauna thoroughly established.

Well I expect you have had enough of this for this time anyway. [Letter No. 75, dated May 3, 1940, Vol. II]

Sand-plain soils of the Yilgarn Goldfield,
and the "Miocene submergence".

Reply to J. T. Jutson's letter of Sept. 7, 1939
and accompanying letter of E. deC. Clarke of May
3, 1940. [By Dr. Dorothy Carroll]

- "3. a. Yes, the basal bed could be incorporated without submergence.
b. This has not been demonstrated, it is just a suggestion.
c & d. Also suggestions.

"These suggestions were made principally to try to get interest aroused in this whole question. The quotations from Grabau show that

the submergence idea is worthy of consideration. The grits themselves and their distribution now require more careful investigation.

e. Yes, such a deposit could occur in a lake, but not, I (D.C.) think on a flood plain, though this statement will have to receive more attention.

4. Southern Cross is about 230 miles from the south coast where there are extensive Miocene sediments. Between the sea and Southern Cross is one of the largest sand-plain areas in the State. Sponge spicules have been found in soils from Lake King (Jour. Roy. Soc. W.A. XVIII, p. 132), approximately 60 miles from the sea and 130 miles from Southern Cross, and from Lake Brown, 50 miles north west of Southern Cross. There are very few rock outcrops on this extensive plain, and in many places, towards the western edge, grits have been found. At St. Ives, about 200 miles from the present coast, spicular rock has also been found. This material, together with that from Salmon Gums is at the British Museum awaiting examination (now, of course it will have to wait until the war is over). If Norseman were submerged by an epeirogenic movement to a depth of 100 fathoms, then, presuming that the profile of the land was much as it is now, Southern Cross would have been submerged about 60 fathoms. But if this were so, where are the sandy sediments which were deposited? They cannot have been completely removed by erosion, for if so, some evidence would have remained in the form of sandy beds elsewhere. Also a flat land surface is not so easy to erode as a high mountainous one. It must be remembered, however,

that the fossiliferous beds are only very thin. I (D.C.) enclose a photograph of an exposure of spicular sandstone underlying fine-grained white sandstone; here the fossiliferous layer is only about a foot thick. In this district (Salmon Gums - south of Norseman) there are many places where there are well-rounded pebbles strewn on the surface of the ground, and yet there is no indication of the strata from which they have been derived. The rounding has been caused by water, not wind. It occurred to me (D.C.) that the small pebbles in the Southern Cross grits may have originated in the same way, a thin veneer of pebbles and worn coarse sand incorporated with present day weathered rock material, and the whole more or less cemented with clay or with secondary silica. The absence of fine-grained sandstone beds (or at any rate these have not yet been discovered) at Southern Cross would seem to indicate that it was not submerged to the same depth as Norseman.

"If, in Miocene times the sea did not cover Southern Cross then, since on evidence accepted at present, Norseman was 600 feet below the Miocene sea level the difference in level of Southern Cross and Norseman must have been at least 350 feet more than it is now (present levels, Southern Cross 1157 ft. Norseman, 927 ft.) This extra difference may have been removed by (a) warping - a flattening out; (b) erosion having removed so much extra material from the Southern Cross district. I (E. deC. C) think (a) has to be further considered. So far as (b) is concerned - if it is the true cause, then Norseman may have been in a deep gulf with a high western edge. The

gulf may have been headed in a river, for there is much alluvial material at Coolgardie. Southern Cross would then have been on a flat plateau which would have been an excellent place for podsol, now evidenced by laterite (Prescott), to develop if the climate were moist. But if this were so, there should have been a good deal of erosion, and then we should have had evidence of it in alluvial deposits near the margins.

"The Plantagenet Series as far as we know was probably formed in a similar manner to the sediments now being deposited in the Bight (see report by Serventy, Roy. Soc. W.A. XXIII, p. 65) where abundant sponges live. (I (D.C.) was unfortunately unable to obtain specimens of these sponges as the material trawled south of Esperance was not kept).

"At Norseman, too, there is a beach deposit which indicates a retreat of the sea to the south and east. Scrutiny of present day beaches seems to show that shelly deposits are much rarer than one would expect, so that the chances of finding fossiliferous deposits in a flat, undissected land, such as so much of the southern interior is, are extremely small.

5. Agree with your remarks.

6. The rounded pebbles are generally small, about 1/4 inch diameter is the usual size. Lithologically the grits could be fluvial, and in Lake Koorkoordine they occur just where on our theory such river beds should be. Our theory implies a very small amount of erosion since the emergence, i.e. very little if any change in topography. But grits with rounded pebbles which might be fluvial occur in the

highest parts of the country, so that if by some means their fluvial origin can be established our ideas on the wide Miocene submergence will have to be modified or scrapped.

"Also there is a possible source for the larger rounded pebbles in a Pre-Cambrian conglomerate at Burbidge. The weathering of this conglomerate might readily cause confusion, for the sandplain near Burbidge is of well-rounded pebbles up to the size of a hen egg. The pebbles in the grits are all smaller and generally I (D.C.) do not think that the Pre-Cambrian conglomerate is responsible for them.

"I (D.C.) did not mean to imply that the laterite was formed prior to submergence. I meant that it developed in the sandy soils which were formed after emergence but before the land was raised to its present level. The climate must have been moister than it is now.

7. Deep dissection has only occurred on the west and south west sides of the uplifted block (Bull. 95, p. 83) and farther inland the rivers become senile. We cannot at present agree that the features of the salt lakes show that extensive erosion has taken place there. (This might make the subject for future discussion and investigation). So that it seems doubtful to us whether the erosion of the present physiographic cycle has really been 'great'.

8. On reading through the Pre-Cambrian succession one is struck by the number of unconformities with peneplanation between the different series. In the Pilbara region, for instance, the peneplain is to a large extent the early eroded surface of the Mosquito Creek series from which the Nullagines have been removed; and so in other

parts there is good reason to believe that where a peneplain once existed it is, when uncovered by erosion, almost the same as when it was first completed. The idea of a resurrected peneplain has been suggested by a consideration of the Pre-Cambrian as mentioned above. We do not consider that there was sufficient time during the Miocene to produce such a perfect peneplain. Quoting from 'Middle and West Australia':- 'Thus the present uplifted "peneplain" of Western Australia, is, where occupied by Miocene or later deposits an uplifted plain of marine deposition, and elsewhere the direct descendant of the pre-Miocene peneplain'.

"I (D.C.) have just seen an interesting description of peneplains in Fennoscandia which gives support to our ideas. 'These peneplains were developed at widely different times during the enormously long Pre-Cambrian period, which is estimated to have lasted about one thousand million years, but during resurrection they have been partly merged into one another in the present day topography, of which they are a very prominent feature' (Jour. Geomorp. II, 1, p. 64-66).

9. Apparently there is not much foundation for our statement that a wind origin of the sand-plains by flattening of dunes had been suggested by anyone, but it seemed to us when the article was written that this idea was a common one among geologists here. Principally we think, the late Mr. Blatchford was in favour of it. There has in the past been a tendency to explain all erosion in the interior as being due to wind, and it was to counteract this idea that we wished

to disprove the wind theory. Of course 'erosion' has often been used when 'deflation' was what was meant.

10. The quotation you gave me from the Journal of Geomorphology is substantially the same as that from Grabau, and is actually what I (D.C.) meant to convey in the article to the Austr. & New Zeal. Adv. Sc. All is of course tentative and will have to be proved.

"Mr. H. A. Ellis has written in Bulletin 97 (issued in 1949) as follows:- 'Thin layers of arkosic grits sometimes containing occasional small water-worn quartz-pebbles and exposed in some of the railway and pipe-line cuttings between Burracoppin and Koorarawalyee. They do not show distinct bedding and wherever they occur they overlie granite or gneiss. The surrounding country is always of the sand-plain type, and from the macroscopic appearance of the grits and the sandy nature of the sand-plain soil it would seem a natural conclusion to consider that the sand-plains were formed from the breaking down of these grits. No fossils were found in them although they have not been examined microscopically for such organisms as sponge spicules, foraminifera, etc.

"'These grits may represent remnants of beds deposited during a marine invasion in Miocene times, proof of which has been found in the country to the south of the Area. It seems more likely, in the writer's opinion, that they are simply arkosic grits formed either insitu or very close to where they are now found during a cycle of arid erosion in geologically recent times'. This statement makes no attempt to explain the origin of the rounded quartz pebbles.

"Just lately I (D.C.) received a book of standard mechanical

analyses from America and compared the analyses of sand-plain soil and an ordinary soil from the Yilgarn with some analyses in the book. At the time I was making an analysis of the Lower Greensand at Gingin, and on plotting the results in the standard histograms was surprised to see that the sand-plain soil and the Lower Greensand were very similar, and that neither was like the analysis of detritus immediately derived from a weathered rock. I hope to continue these investigations.

"Another interesting point about old soils is that made by Browne when writing about the preservation of soils from one cycle of erosion to another: 'Of course, too, the soils dealt with are in the main sandy, and as such would be less liable to surface erosion than the more compact types: in other words their porosity has been a chief factor in their survival. As a matter of fact the fine-grained clayey soils have in many cases almost completely disappeared through sheet-wash erosion from the flat or nearly flat plateau surfaces' (Jour. Roy. Soc. N.S. Wales, 1928, p. 251. Vol. LXII).

"One of us (E. deC.C.) made the suggestion of the Miocene submergence because he understood from Chapman's writing that the sponges of the Plantagenet Series are fairly deep water forms and he could not understand how, if Norseman had been 600 feet below sea-level, Southern Cross could escape submergence. Then working on a very different problem, evidence that seemed to favour this idea was got by the other (D.C.). However we feel some doubt now about the fundamental 'fact' - the deep water character of the sponges - because of the occurrence of dicotyledonous leaves associated with them. The

sponges were to be re-examined at the British Museum - but no results can be expected for years now." [MSS C 128(16) - 128(21)]

There is nothing to be gained by a point for point analysis of the foregoing, for it speaks for itself. But some general comments are worth making.

Discussion.

All parties display exemplary thoroughness in presenting their respective cases, with Carroll and Clarke having the task of constructing a case which Jutson must refute. The principal weakness of the Carroll-Clarke case lies in its dearth of direct evidence to support marine transgression beyond those parts of the State where such evidence exists. As Jutson points out and Carroll concedes, the soil data and the grit occurrence with associated rounded pebbles can as well be explained without invoking marine intervention. But to counteract one theory by another that is equally implausible is of dubious merit; and this Carroll and Clarke have attempted to do, stating:

...There has in the past been a tendency to explain all erosion in the interior as being due to wind, and it was to counteract this idea that we wished to disprove the wind theory. (see above, text p. 88)

Carroll and Clarke do, however, cite one occurrence of sponge spicules which lends strong support to the marine theory, evidence which Jutson does not discuss. It is cited in Carroll and Clarke's second reply, wherein we read:

Southern Cross is about 230 miles from the south coast where there are extensive Miocene sediments. Between

the sea and Southern Cross is one of the largest sand-plain areas in the State. Sponge spicules have been found in soils from Lake King..., approximately 60 miles from the sea and 130 miles from Southern Cross, and from Lake Brown, 50 miles north west of Southern Cross. There are very few rock outcrops on this extensive plain, and in many places, towards the western edge, grits have been found. (see above, p. 84, emphasis added)

Now whilst there was at the time doubt as to the nature of the environment of deposition indicated by the sponges, there was no doubt that they are marine organisms. Their occurrence "50 miles north west of Southern Cross" and north of extensive sand-plains is highly suggestive; and extrapolation to more northern interior parts of the State where there are similar expanses of sand-plains and associated lake forms, is not unreasonable; and dearth of direct evidence of marine transgression may simply reflect the fact that it has not been found, or that it need not be found as Carroll suggests.

The strength of Jutson's case in refutation lies not only in the general lack of direct evidence to support the rival theory, but also, and more importantly, in this: that he (elsewhere) shows that many aspects of the landscape can be explained by processes now operating, whilst others can be explained by invoking the operation of subaerial processes under different climatic conditions; and all parties to the debate concede that such climatic changes have taken place.

Referring to the dry lakes, and mindful of the fact that proof of a widespread marine transgression would undermine Jutson's views of their origin, vindication of his position is found in recent studies, notably those of Killigrew and Gilkes (1974). Although these

workers make no pretence to discuss in detail the mode and time of origin of the lakes, nonetheless their work suggests that wind is an important agent in moulding the shape, at least, of the lakes through the agency of wave generated currents following the conclusions of other cited authors. In addition, they conclude that

...playa lake features...display an equilibrium adjustment of shape, size, and orientation to contemporary fluvioaeolian processes (Ibid., p. 455),

a statement that is reminiscent of Jutson's view

...that the existing agents are competent to produce the existing phenomena, including valleys, plains, and lakes, and that many factors are responsible for the results obtained, amongst which the wind was regarded as playing a prominent part (Jutson, 1950, p. 217).

Not only does the above cited conclusion of Killigrew and Gilkes favour Jutson's theory vis à vis the marine one, but also vis à vis Gregory's dismembered Miocene river hypothesis. The latter, however, has strong appeal particularly because of its simplicity, a characteristic of great virtue to men of science. And its acceptance by modern geomorphologists (Hills, Ollier, & Twidale in Hills, 1966, p. 65) lends further weight to it. Jutson's arguments against it have been presented above and need not be repeated now. But that he (Jutson) did not brush it aside lightly is evident from a number of statements, as, for example:

...there may be some distant connection between the old Miocene rivers and some of the existing lakes, but he was unable to accept the view that such lakes were, in the main, merely the dead remains of such river systems (Jutson, 1950, p. 217).

and cognisant of the fact that many lakes are linearly disposed along

drainage lines, Jutson admits that some lakes have been formed "by the encroachment of drifting sands on drainage lines" and draws attention to the consequent need to examine "The nature and origin of such lines, the time they were formed, and the changes (if any) that have taken place since their formation." (Ibid., p. 219)

In one other respect the findings of Killigrew and Gilkes lend weight to Jutson's ideas, and this with respect to the lakes' orientation in a general north-south direction. Recalling that to Jutson the westward migration of the lakes is a result of both cliff retreat on the west and encroachment of detritus from the east, Killigrew and Gilkes show that "winter wind resultants remain approximately normal to long axis orientation and opposite each other." (Loc. cit., p. 455)

It is in the context of his discussion of the lakes' origin that Jutson enunciates a philosophic position regarding landform studies, and which accounts, it is suggested, for his seeming equivocation in dealing with various topics. In his 1918⁷ paper on the lakes published in the Geological Magazine we read:

....Like most other land forms the lakes will probably, by further research, be shown to be due to no single cause, but to a combination of agents of different relative importance. (1918c, p. 305)

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On November 30, 1935, an article by Jutson entitled "The 'Dry Lakes' of Western Australia" appeared in the Melbourne Argus, following "the discovery by the Mackay exploratory expedition of several large salt lakes in the north-east of Western Australia." The first half of the article consists of an account of the physical geography of the lakes, while in the second half he describes their strange beauty, closing with comment: "...the West Australian 'dry lakes', with their wealth of unique form and colour, will provide eventually a rich harvest for those painters who are prepared to forsake the beaten paths." This article was Jutson's last published statement on the lakes.

This view is reminiscent of the position stated by Twidale in regard to slope development, who, at the conclusion of his paper presents a synthesis that has wide application in geomorphology.

Twidale states that:

...The problem is complex, involving the interaction of many factors: structure, lithology, vegetation, present climate and morphogenetic system, and palaeoclimates, stage in geomorphic cycle and recent geomorphic history. It is so complex, and there is as yet so relatively little data on which to base conclusions, that universal laws cannot be formulated....(Twidale, 1960, pp. 144).



Plate 5

Clovelly, N.S.W., 13/11/36. Shark Point. Two platforms, the higher in the foreground (with the sinuous weathering) and in the background where the waves are breaking. The second platform is 3 feet lower than the first & is in the middle distance & nearly all awash. Note also that the waves are breaking at the outer edge of the higher platform & passing over the latter. The cliffs are being undermined at the edge of the higher platform, so both platforms are being extended landwards. As seen today the length of the two platforms along the coast was about 70 yards & the width about 40 yards. [Photograph taken and description written, by J. T. Jutson.]

Chapter 3

Coastal geomorphology

Hence, speculations as to the history of the elevation of the land,..., to be of real service, demand careful scrutiny of the actual coast-lines, and an amount of geological investigation which would require long and patient toil for its accomplishment.

A. Geikie, 1893

Introduction

Jutson's work on coastal geomorphology belongs mainly to the latter part of his life; for although his early work touched on coastal questions, it was not until after he had completed his studies in law that he concentrated his attention in this direction.

Following his attainment of the LLB degree from the University of Melbourne in 1925, his life took on a pattern that was maintained until the closing years of his life. Monday to Friday solicitor and law partner, Saturday at home working in the garden, and Sunday out in the field at coastal sites in Port Phillip Bay (Mrs. Vera Donaldson, personal communication).

The stimulus for his interest in coastal work possibly came from his acquaintance with D. W. Johnson who was well known for his work on shorelines, and whom Jutson had the pleasure of guiding on coastal field trips during Johnson's stay in Australia in 1929 (see Letter No. 159, Vol. II). The friendship and mutual regard that developed between these workers is expressed in their correspondence (see Vol. II). An additional source of interest, though less direct, may have come from W. M. Davis through the latter's interest in the coral reef problem about which he corresponded at length

with Jutson at the time when he (Davis) was very much involved in it (see Letters Nos. 105-108, & 111). In addition to these intangible influences, there was a practical one. Jutson at no time owned a motor vehicle, and was therefore dependent on public transport available for coastal sites, either boat from Port Melbourne, or train from Melbourne. And perhaps too, for a man in excess of fifty years of age, field work at coastal sites on single day trips was probably more feasible than inland work. At all events, his Sunday efforts provided the basis for useful and important contributions to coastal geology and geomorphology. There were two principal aspects to his coastal interests, the first concerning erosion and sedimentation, and the second concerning shore platforms.

Coastal erosion and sedimentation.

Jutson's early work on coastal features was of a general nature, appearing in papers published in 1927 on Port Campbell, Victoria, and 1931 on erosion and sedimentation in Port Phillip Bay, the latter supported by the Council for Scientific and Industrial Research. In both he describes coastal forms and processes in a standard, but nonetheless useful way, drawing on a background of reading that included, amongst others, works of D. W. Johnson, W. H. Wheeler, and V. Cornish as testified to by his notes in the Jutson Documents (see also Part Two, Chapter 6 of this work). These contributions do not call for special evaluation.

Jutson's interest in coastal and general geomorphology and geology was not directed only to theoretical questions, for in one

published and three unpublished articles he expressed concern for the hastening of erosional processes initiated by human activity drawn to his attention on field trips. For example, on the 27th January, 1935, in Field Book No. 45 when in the Frankston to Brighton area of Port Phillip Bay he records the following:

The result of my observations is that for about the whole of this distance the tea tree & associated vegetation are being rapidly destroyed with the production of much bare sandy ground, the sand of which is cut into & loosened by the heavy traffic on it.

There are literally hundreds of cars parked in the tea tree....Wind & water erosion are immensely facilitated by what is going on....Man and his motor car want controlling as much as the sea (pp. 181-183).

The published article referred to above and entitled "Fore-shore erosion in Port Phillip Bay" appeared in the Melbourne Argus on the 31st August, 1935. After outlining the destructive and constructive role of marine processes in the littoral zone, he outlines some of the features of the Bay coast, then offers six conclusions, including suggestions for the control of the destructive marine processes and human activities. [MS 74(1)]

Of the three unpublished articles, prepared also for Argus publication, one is entitled "The menace of water - and its conversion into a blessing", being a short statement based on an article in Science (82: 461-462, 1935) by M. E. Musgrave on flood water control, with Jutson making application to problems in the catchment area of Hume Reservoir, Victoria [MSS 90(12-15)].

The other two articles are of a general nature, forming a pair. One, entitled "The making of a landscape: nature's age-long process"

[MSS 92(5-8)] is prefaced with this statement:

Most people nowadays can appreciate striking landscapes and many enjoy the quiet beauty of much of the ordinary countryside. The aesthetic satisfaction usually suffices, but to know something of the processes that have brought a landscape into existence, and to some extent, why one landscape differs fundamentally from another, need not detract from the enjoyment; but, on the contrary should increase our wonder and delight in the myriad manifestations of the great forces of nature at work upon the surface of the globe.

The other, entitled "The marring of a landscape: man's short-cut process" [MSS 92(104)] is introduced as follows:

In the preceding article a description has been given of the processes by which, during long periods, a landscape is formed. We now propose to show how by man's interference with the face of the earth, a landscape can be materially altered in a short space of time, and that usually for the worse.

The Jutson Documents include one other unpublished article on coastal features, and is entitled "Non-longshore [the absence of] drift of beach shingle in Port Phillip Bay, Victoria, and the adjacent coasts" [MSS F2 (181-211)]. The article is written in longhand, and is directed to a scientific audience rather than a public one, and consists of documentation of observations of beach shingle in the Bay. Jutson's conclusion that "There is no substantial drift of shingle along any of the coasts mentioned between tide marks" [MS F2 (206)] is followed by several pages of discussion in an attempt to explain it. The present writer is unable to attach any particular significance to this conclusion other than that sediment of the size implied by "shingle" (Jutson does not define his term) cannot be drifted in the usual way, even by storm waves.

Whatever may be the value of the above contributions, of greater significance to geomorphology is Jutson's work on shore platforms to which we now turn.

Shore Platforms

It is generally agreed that the history of the study of shore platforms in Australasia dates back to the observations of J. D. Dana during the Wilkes Expedition (1838-42) on shore platforms of New Zealand near Auckland (Dana, 1849). Despite this early attention to this coastal form and its explanation as a feature developing at and above the level of permanent saturation (Dana, *ibid.*; 1880. Bartrum, 1916), subsequent treatments of it became incidental in considerations of the general form of coastal profiles (see A. Geikie, 1903), or to deductive treatments of the "profile of equilibrium" championed by Fenneman (1902) and developed by Johnson (1919). One might suppose that during the controversy surrounding the effectiveness of marine action in bringing about extensive planation of land masses for which A. Ramsay was the principal protagonist in the nineteenth century (Chorley, *et al.*, 1964), that close attention would have been paid to coastal features of the British Isles in an attempt to resolve the issue. Such was the case.⁸ However, by the

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For example, Jutson (MS 78) quotes H. T. de la Beche (1834, p. 66) on coastal erosion of the London Clay and the growth thereby of a platform below sea level "so that the land must have once been far more extensive than at present.... Historical documents show this to have been matter of fact." And implicit in Hutton's statement is the notion of marine planation at coastal margins. "This decay [of land masses] is the gradual ablation of our soil, by the floods of rain; and the attrition of the shores, by the agitation of the waves." (Hutton, 1785, in Albritton, 1975, p. 26).

publication in 1916 of his paper "high-water rock platforms", Bartrum of New Zealand signalled the beginning of a new phase in their study which has persisted to the present time and is marked by close observations and detailed consideration of the processes operating in their genesis.

During this phase, Australasian workers have assumed leading roles with J. A. Bartrum and C. A. Cotton in New Zealand, J. T. Jutson, A. B. Edwards, and E. S. Hills in Victoria, and to a lesser extent R. W. Fairbridge when in Western Australia, being the principal contributors, with C. K. Wentworth, working in Hawaii, taking on a pivotal role, and D. W. Johnson serving as a sideline observer injecting interested and worthwhile commentary. The most significant and critical summary statements on shore platforms are those of C. A. Cotton (1963) and E. S. Hills (1971). In what follows, therefore, no attempt is made to survey the contributions of the many who have added to our fund of knowledge and insight on the matter, but rather to appraise the contributions of Jutson to it.

First to publish work on shore platforms in Australasia were J. M. Bell and E. deC. Clarke (1909), the latter to become Jutson's colleague as a field geologist in Western Australia and subsequent life-long friend and correspondent. Their work, however, after receiving acknowledgement in J. A. Bartrum's 1926 paper, has been completely neglected. But in order to see Jutson's work in its historical context, attention is here focussed on Bartrum's ideas

which entered the mainstream of thought whereas those of Bell and Clarke did not.⁹ Jutson entered the field in 1931 in a somewhat incidental way in his paper "Erosion and Sedimentation in Pt. Phillip Bay....", though without reference to Bartrum's work. Both these workers pursued independent courses, and the fabric of thought remains essentially simple with Bartrum being the principal contributor with papers published in 1935, 1936, and 1938. From this time on, others enter the field, and the interplay of ideas becomes increasingly complex. Thus, to see Jutson's work in its historical context we need to survey the essence of Bartrum's ideas prior to Jutson's appearance on the scene, and then attempt to unravel the strands of thought by considering a selected sequence of papers published between 1931 and 1953, the year of Jutson's final paper on shore platforms and the last to come from his pen. Papers appearing after 1953 allow for the final appraisal of Jutson's contributions to the topic.

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The phrase "mainstream of thought" is used with caution. Certainly the history of thought on any topic has mainstreams, but their definition is, of course, a matter of judgment, which may be good or bad depending on the quality of the scholarship of the definer. In addition, whether or not thoughts enter this mainstream is dependent in part, and perhaps large part, on the extent to which a worker can project his thought successfully into the consciousness of his particular audience. The classic example of this in geomorphology is that of W. M. Davis who was eminently successful in imposing his pattern of thinking on at least two generations of workers (Chorley, *et al.*, 1964, 1973), and who, in the words of the late W. R. Browne who met Davis, regarded himself as the apostle of the new creed (see Part Two, Chap. 5). Jutson, on the other hand, due in large measure to his retiring disposition and physical deafness, failed to exert the influence he may have had he been otherwise disposed.

Shore platforms in historic perspective.

Bartrum's 1916 and 1926 papers contain the essence of his thought regarding the morphology and genesis of platforms, subsequent ones (1935, 1936, 1938) adding refinements, but no essential shift of ideas.

Two types of platforms are recognised on New Zealand coasts; the "Old Hat" type also called "high-water" and "high-tide" platforms; and storm-wave platforms. Common to both are the backing cliff, the sub-horizontal platform, and the steep outer marginal slope descending to the adjacent sea floor. But morphological differences exist between the two forms; the former are commonly wider and occur on sheltered "submerged" coasts at the level of high-tide, and are "barely covered by mean high tides" (1916, p. 133); the latter occur as a narrow ribbon-like fringe on exposed coasts subjected to vigorous wave action, particularly of storm waves, and commonly occur "slightly above the level of normal high water." (1926, p. 799)

The mode of origin is distinctly different. The high-water platform is initiated by the submergence of a land mass in the mature stage of development (in the Davisian sense) mantled by a zone of weathered rock. Platform development ensues and "the platforms are due not so much to wave-attack upon a definite zone of weathered rock as to the destruction of the cliff faces by subaerial agencies and the removal of the resulting waste by weak wave action." (1916, p. 134); and essential to this theory is the "well-known fact that

sheets of water protect rocks beneath them from active chemical decomposition." (Ibid.) This not only allows for the retrogression of the cliff-line by subaerial agencies as the water table is lowered, but also accounts for the preservation of the platform itself, as Dana (1849), cited by Bartrum, originally proposed. This set of processes in platform development was independently and initially set forth by Bell and Clarke (1909), which, as noted above, Bartrum acknowledged in his 1926 paper.

The storm-wave platform, on the other hand, is the direct result of wave attack and occurs "at the base of steep sea cliffs at a height which approximates normal high-water level but may be a foot or two above it." (1926, p. 806) Furthermore, the platform is kept narrow by erosion of the outer marginal descent.

The original elements in Bartrum's early work are the hypothesis that subaerial erosion of the cliff face (for the Old Hat type) and not wave action is the main process operating in platform development, a theory that is not taken up by any subsequent workers; and his drawing a distinction between two types of platforms.

One other aspect of Bartrum's thought deserves mention, not so much because it has a central explicit part in it, but because it influenced his view of the context in which he saw the platforms he described; and this is the concept of the "normal shore profile" or the "profile of equilibrium" as envisioned by Fenneman (loc. cit.) and Johnson (loc. cit.). As he makes clear in the opening paragraphs of his 1926 paper, his application of the term "abnormal" to the platforms discussed above is not meant to imply that he regarded them

as unusual features, but that their existence represents a disturbance of the idealized profile offered by Fenneman. Johnson, on the other hand, clearly incorporated the "marine bench" in his profiles of equilibrium at the various evolutionary stages of his marine cycle as reference to Chapter 5 of his famous book (1919) attests.

Jutson's first published account of shore platforms appeared in his 1927 paper entitled "Notes on the Coastal Physiography of Port Campbell, Victoria." Here, shore platforms are given only passing reference, with no detailed description of form or discussion of genesis. One occurrence a short distance west of Pt. Campbell receives attention where a portion of a platform of unknown extent underlies the eastern end of a sand ridge, at an elevation six to eight feet "above the beach" (Ibid., p. 52). Its origin is attributed to "marine abrasion", and the present elevation is taken as evidence of uplift.

In his 1931 paper "Erosion and Sedimentation in Pt. Phillip Bay...." Jutson devotes a section to what, in these early papers, he consistently refers to as "wave-cut platforms". Detailed attention is now devoted to this paper because in it is found the germ of thought that permeates all his subsequent papers.

At the outset it is noted that to this time, and indeed until at least 1937 when he corresponded with Bartrum, Jutson pursued his coastal work without reference to him. Neither his published papers nor his preserved notes on reading (Jutson Documents) give any hint

that he did; and were he to have been familiar with Bartrum's work it is certain that he would have made reference to it, such was the nature of his scholarship. He approached his coastal work with a background of reading from A. Ramsay (1846: MS 79), J. Prestwich (1886: MS 81), A. Geikie (1903; MS F2-77); J. Le Conte (1902; MS 82); D. W. Johnson (1919), and many others.¹⁰ Whilst Jutson's failure to take account of Bartrum's work to this time may be regarded as a deficiency, there is, nonetheless, the advantage that he approached his work on platforms with a view clear of pre-conceived notions, for as Hills (1971; p. 139) rightly points out, "If genetic concepts are developed early, virgin sectors of the region may be used as testing grounds for any hypothesis already constructed; but in observing and reporting it is essential to note the realities of nature, in addition to any generalized or idealized type features which may be conceived, and which, as such must depart from reality. There is indeed a risk, from which the present author is certainly not free, of observing those things which the mind has

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The Jutson Documents contain extensive references to literature on coastal geology that spans over a century in time; Jutson drew not only on what he refers to as "text-books" in his 1931 paper (the works cited above would fall into this category), but also on a wide variety of works of British, American, South African, and Australian origin. Many references and extracts deal with topics to which Jutson did not address himself after 1931. But to judge from a remark in this paper, he did plan to make use of these studies, for he says: "The paper is a summary of the observations made, and of the provisional interpretation of the evidence. It is hoped that details, with illustrations, together with references to any earlier work, will be given in the future." (1931, p. 130) The hope here expressed was never realized.

already accepted, and of rejecting others. A conscious attempt to make selective observations and records may assist to overcome this natural disability."

In order to appraise Jutson's work justly, it is deemed necessary to quote at length from his 1931 paper. In the following extracts from pages 147 to 149 the italics added are the writer's, not Jutson's.

A wave-cut platform standing beyond the reach of the waves is excellent evidence of a relative uplift, but neither within the bay or outside in the immediate vicinity of Port Phillip Heads is there any such evidence.

Fine examples of wave-cut platforms in the dune limestone occur at Barwon Heads, Point Lonsdale, and on the ocean side of the Sorrento Peninsula. A wave-cut platform in the Older Basalt is also found cut just to the west of Cape Schanck. In all these cases the platforms are backed by rock cliffs which are receding owing to marine abrasion, and in this way the platforms are being extended with sea-level as at present. The platforms are exposed at low tide, and are bare of detritus. The slope of the platforms seaward is very gentle; in fact their surface must be almost horizontal. This raises the question whether the platforms have been cut as they now stand at present sea-level, or whether since their cutting there has been an uplift of the land. If the latter has taken place, then it is a striking coincidence that the platforms should be uplifted just to low water mark....Of course this could occur, but the coincidence emphasises the necessity of caution in dealing with the matter, especially in view of the occurrence inside the bay of marine deposits above sea-level. One would expect, on the theory of uplift, that the platforms would be in similar positions.

If there has been uplift then the coastal edge of the platforms before uplift would be at present sea-level, which means that the slope of the platforms would then be much greater than now, and also that the uplift must have been along a hinge line which would follow the contour of the coast, a phenomenon so remarkable as to call for the strongest evidence of its occurrence. Such evidence is not available so far.

If the uplift extended landward beyond the present coastline, then the landward edge of the platform must have been lifted above sea-level; and as no remains of the platform above sea-level have been found, it must have been removed by marine erosion. It follows, therefore, that the landward edge of the present platform has been cut at present sea-level. [This paragraph is open to criticism, not so much for lack of clarity as in the one above it, but for its questionable logic. In particular, from the argument given it does not follow that "the landward edge of the present platform has been cut at present sea-level." Nonetheless, the case is plausible, so we let it rest for the time being.]

It is clear that the platforms are now being extended landwards by marine abrasion at present sea-level. It may be asked, therefore, why could not the whole width of the platforms have been cut at that level. A difficulty in this connection is to understand how such a high-level platform could be cut for a considerable width. One would think that the waves would practically cease to abrade. That they do not, however, is shown by the fact that...the cliffs are still receding, and any one who has watched the waves at high tide, especially with a strong wind behind them, will realise the great height and power that they attain on the platforms. The facts stated discount the uplift theory.

At one of the stacks on the platform at Port Lonsdale, a few yards from the shore, marine abrasion is now working to a height of five feet above the platform.

On their seaward side the platforms are now being destroyed, so that they are relatively narrow, probably on the average not more than 100 to 200 yards wide. They have been in places breached right through to the sea shore, where the bottom of the sea, although sand-covered, is two to four feet below the surface of the platforms. At their seaward edge the depth of the sea is much greater. There is thus another lower platform being cut by the waves, the surface of which would accord more with the diagrams of the text-books. This suggests that perhaps the present high-level platforms were formed at lower levels, that they have been uplifted, and that the sea, in establishing the old level again, is destroying the original platforms. [The suggestion made in the last sentence has not been taken seriously from the time it was penned to the present. We note here that Jutson rejected it on the basis of what the present writer regards as unsound, though plausible, logic. Should this

idea have been taken up and championed, how different would have been the history of the study of shore platforms! This matter is discussed in full later on.] It has been shown above, however, that such uplift is improbable, but it is possible that in some way not yet fully understood, the sea can cut two platforms at different levels at the same time under certain conditions, such as the general abrasive power of the sea in a particular locality, which power is influenced by winds and currents, and such as the character of the rocks and the height of the land above the sea.

If the land were comparatively low and the rocks "soft", the formation of the high-level platform might be faster than that of the low-level one, and hence the two platforms, at least for a time, would be found. Increasing the height of the cliffs or a change to "hard" rocks, or the occurrence of both these factors, would slow down the formation of the high-level platform, and permit the lower one to gain upon it. Ultimately the higher one might be completely overtaken and disappear.

If the land were high and the rock "hard", the high-level platform might be destroyed as soon as a few feet in width of it were formed. Hence only one platform - the low-level one - would be regarded as forming, although, in reality, both would be formed, but the high-level one would be almost immediately destroyed.

The destruction of the high-level platform at its seaward edge aids the waves to maintain their abrasive power at its landward edge.

Examples on a small scale of wave-cut rock platforms may be seen within Port Phillip Bay at Portarlington (decomposed Older Basalt), Sorrento and Point Lonsdale (dune limestone), Geelong (Tertiary sediments), Beaumaris (ironstone), and elsewhere. They all occur similarly to the ocean platforms, and the remarks made in regard to the latter apply to the bay platforms.

From this statement the following points are abstracted:

1. Essential morphological features of the high-level platforms include the backing cliff, receding owing to marine abrasion; the subhorizontal platform, exposed at low tide and bare of detritus; and the seaward marginal slope which is being actively attacked, thereby destroying the platform.

2. This platform is termed a "high-level platform" and storm waves must play an important role in its formation.
3. There exists a "low-level platform" being cut by the waves as the high-level one is attacked at its seaward margin; and thus it is possible that the sea can cut two platforms at different levels at the same time under certain conditions.
4. Dependent on the height of the cliffs and the relative hardness of the rock, the high-level platform may or may not develop depending on the relative rates (not so stated, but implied) of cliff recession at the landward edge of the high-level platform and its destruction at its seaward edge.
5. The platforms form alike on coasts open to the ocean and in the (more sheltered) bay coasts.
6. Platforms occur alike on decomposed basalt, dune limestone, Tertiary sediments, and ironstone.
7. The lack of a detailed consideration of processes operating, other than "marine abrasion" is noted.

Between 1931 and 1939, the year of the publication of Jutson's first paper devoted specifically to shore platforms near Sydney, N.S.W., Jutson spent a lot of time in the field at Port Phillip Bay and nearby ocean sites. His field notes from this period touch on many aspects of coastal geology and geomorphology, including attention to platforms. A perusal of these notes makes it clear that this period was the crucial one in establishing his ideas on platforms, on their morphology and the processes operating thereon as

the following discussion, with extracts, makes clear.

Field Observations 1933 to 1935

The MSS here drawn on are entitled "Field Notes 15/10/33 & 6/5/34. Quiet Corner. Black Rock." [MS 83 (1-12)], "Field Notes Black Rock, Quiet Corner, Ricketts Point & Table Top Corner or Pts. 9/9/34" [MS 84 (1-10)], and "Field Book No. 45" for the period 28/1/33 to 1/4/35 at a number of sites. The first two of these MSS are written in ink on lined quarto pad sheets, and the latter in ink (in places over pencil) in a 7 x 4-1/2 inch bound notebook. Two fragments of evidence suggest that these notes were not written in the field, or if they were mostly written there (and the detailed nature of the observations in the main support the notion that they were), then additions were made later. On MS 83 (2) the comment "(from memory)" appears with reference to the height of a notch shown in a section diagram at the foot of the page. And on p. 223 of Field Book No. 45 the parenthetic remark is made that "(contents, except that there were some recent marine shells not remembered definitely enough to state but pbly mostly dune limestone fragments)" (italics added), which strongly suggests that the notes were not written in the field. In addition, on p. 227 of the same Field Book appears the following fragment: "At the Black Rocks (see Field Note Bk. 46 p. 199)". It is odd that in Book No. 45 there should be reference made to Note Book No. 46, unless Jutson wrote in error. Indeed, Book No. 45 opens: "Continued easterly along the sandhills from where finished on 31/12/33 (see Note Bk

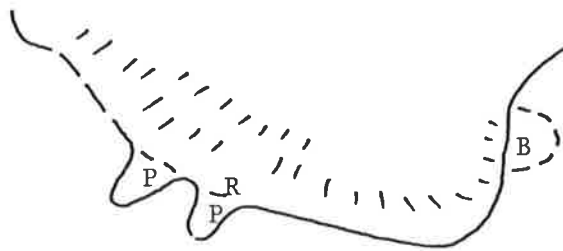
No. 44 p. 217.)" and with the date 28/1/34, suggesting that he may have meant No. 44, not No. 46, in the above quotation. But a consideration of times and places (for which evidence need not be adduced) makes this unlikely. Regrettably most of Jutson's field note books are no longer available and the point of confusion cannot be clarified.

Despite this, it is most probable that the MSS referred to were written in the 1930's, and certain that the field work to which they attest was done then; and if the MSS are not verbatim notes actually written in the field, then they were certainly drafted on the basis of documented field observations. Thus they are considered valid sources for the discussion which follows the extracts now placed before the reader.

Extract 1 [MS 84 (3)]

From C to Table Top Point or Corner, the cliffs are usually low & covered with vegetation. Moreover, they recede from the beach until at Rickett's Point they are pbly 100 yds from the sea. This is the widest area of progradation. The prograded area narrows until it disappears at Table Top Point where bare rocky cliffs abut the sea. There seems to be no doubt that all this flat area is due to progradation at the present sealevel & not to uplift. Par'lars given below.

Similar rocks & platforms to those earlier described under D continue to the S.E. for some distance. They then practically cease until we meet the 2 platforms of Rickett's Point.

Rickett's Point

- R Ricketts' Point
- B Boatshed with concrete wall around it
- P Large flat platforms of ironstone, which are covered at ordinary high tide.

Note that Jutson interprets the occurrence of the cliffs recessed from the beach as evidence of local progradation rather than of uplift; this conclusion is reached rather hastily as there are other possibilities worth exploring. However, this interest in the question of uplift (or downthrow)¹¹ is plainly crucial to a correct interpretation of the shore platforms, as of other coastal features, and Jutson's notes are interspersed with observations and remarks that bear on the question.

In this and following extracts, platforms on "ironstone" are treated of, but the exact nature of this ironstone is not made clear. The present writer supposed that Jutson is here referring to a sedimentary rock in which secondary precipitation of and cementation by iron oxides has occurred. But other remarks of Jutson suggest the possibility that it is some kind of iron rich unit intercalated between argillaceous layers, a notion supported by E. S. Hills' reference to "resistant Red Beds" (Tertiary) at Rickett's Point (Hills, 1940).

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The writer is using the terms "uplift" and "downthrow" when tectonics are implied, and the neutral terms "emergence" and "submergence" when circumstances dictate.

Extract 2 [MS 84 (4,5)]

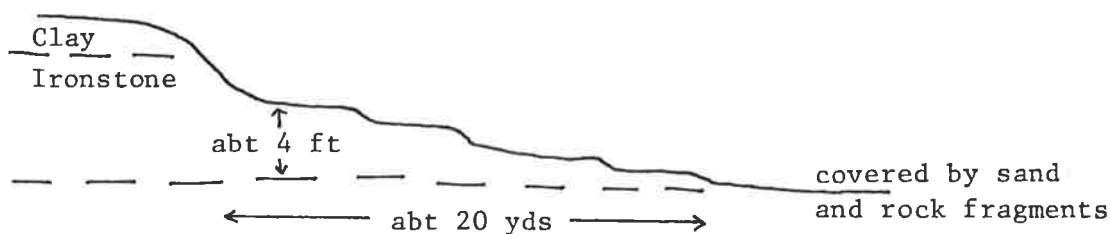
The more south-easterly platform flanks the 'cliffs' of the prograded area. These cliffs are abt 18" high & show a thin band of broken shells & small ironstone fragments (a similar band occurs towards the S. En end of the D outcrops).

Both platforms are extensive & well planed down. They show however, the usual irregular surface of the [foundation] of lower ones (9" to 12" below each other). Nothing in the way of stacks.

Between Rickett's Point & Table Top Pt small outcrops of the same ironstone on the sea floor. Some of these show well the grooving by the waves of the platform by parallel furrows at rt angles to the coast line. (Good views of these especially from the tops of the cliffs.)

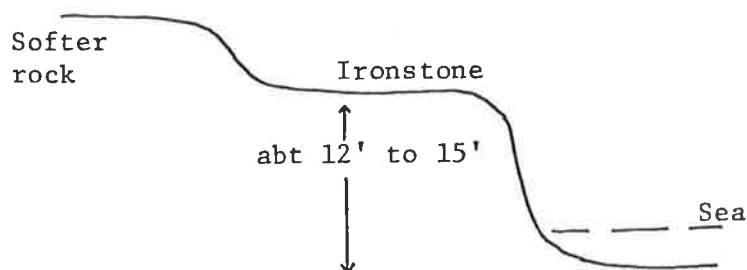
At Table Top Point

Ironstone increases in height in the cliff sections & the cliffs become the typical steep ironstone ones characteristic of Beaumaris. Just before one turn to the N.E. there are 5 ironstone platforms in a total height of abt 4 ft.



The 4 uppermost ones are now being eroded by the sea, as well as the cliffs. From the N.W. the ironstone appears to rise gradually. The sea follows the surface of one hard bed, altho' it may be inclined. Subsequently other beds are eroded & so we have the 4 upper platforms all being worn away at the same time, as also the cliff itself.

Just around the N.E. turn. The surface of the hard ironstone is probably 12 to 15 ft above the sea bottom at the base. It is exposed as a platform a few yds long and abt 10 ft wide. The rocks are softer above.



This platform is above the reach of the sea & its occurrence is no doubt due to the rapid removal of the softer rocks by ordinary atmospheric erosion.

Extract 2 has been selected for several reasons. Firstly, in the third paragraph we read of outcrops of the ironstone "on the sea floor" which "show well the grooving by the waves of the platform (the sea floor) by parallel furrows at rt angles to the coast line. (Good views of these especially from the top of the cliffs.)". Jutson has been accused of paying too little attention to specific processes operating on platforms, at least in his early published papers, relying on phrases like "marine abrasion" and "wave attack" to account for morphological aspects of the shore features he describes. And here "the grooving by the waves" is a like case in point. What he saw were parallel furrows on the sea floor in ironstone; he did not see "grooving by the waves", or if he did, then his comments do not make it at all clear just how this grooving was accomplished. But supposing he did see evidence of erosion (grooving) of the sea floor then this is an interesting comment to which we shall turn presently.

Jutson goes on in the extract to describe a succession of five

platforms up to about four feet above the adjacent sea floor. The sea is the formative agent of the four upper platforms, and is responsible for the recession of the uppermost cliff. Though not clearly so stated, it is implied that the successive platforms owe their existence to a structural factor, interbedded hard and soft layers.

Then follows a description of a lone platform on ironstone about twelve to fifteen feet above the adjacent sea floor, which owes its genesis to the retreat of its backing cliff on softer rocks "by ordinary atmospheric erosion."; and it is notable that to judge from the section there given, no platform other than the sea floor occurs at the shore line.

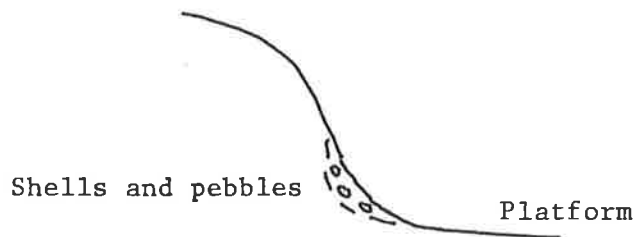
That Jutson should so uncritically conclude that the four successive platforms as well as the cliff are "all being worn away (eroded so as to form them, not destroy?) at the same time," is somewhat surprising. Perhaps the influence of structural control was so evident to the observer as to override the need to explore other possibilities. One wonders if the "grooving by the waves" (see above) at a nearby site may not be due to structural control, with waves of translation or backwash carrying abrading tools to remove soft rock intercalated with the harder ironstone.

At all events, the significant features in this extract are (1) the recognition of multiple platforms above sea level (implied) forming simultaneously: (2) the recognition of the role of structural factors in influencing the height, distribution, and surface

features of platforms; and (3) the supposition that cliff recession is at times due to atmospheric agencies, particularly where the cliff is believed to be beyond the reach of waves, and that upper platforms may thereby be formed.

Extract 3 [MS 84 (6)]

Just farther N.E. is a fine flat ironstone platform on part of which the boathouse (with a concrete wall on some sides) is built. The usual cutting into lower platforms is going on here. At the S.W. end of the platform the cliffs show a thin deposit of broken shells & small ironstone pebbles the top of which is abt 2 ft above the surface of the platform.



Like those at Rickett's Point & at D these deposits could easily be thrown up by a not very high tide.

Between the turn to the N.E. & the platform just mentd (probably not more than 70 or 80 yds) the cliffs are steep & appear in some instances to descend straight into the sea to a depth of 3 to 4 ft (tide fairly low today when inspected). In other cases small remnants of the platform (formed between tide marks) seen. Latter has therefore been entirely removed in places, thus illustrating my suggestion in the Port Phillip Bay paper that sometimes the upper platform is destroyed almost as fast as formed so that the cliff descends straight on to the lower platform.

The third extract is interesting because in it Jutson draws attention to segments of the coast where there are no platforms, but rather cliffs which descend "straight into the sea". And he

uses this absence of platforms as well as the occurrence of small ones to confirm his 1931 suggestion "that sometimes the upper platform is destroyed almost as fast as formed, so that the cliff descends straight onto the lower platform." This focus on parts of the coast where platforms do not occur relates also to a statement by E. S. Hills (1971, p. 148).

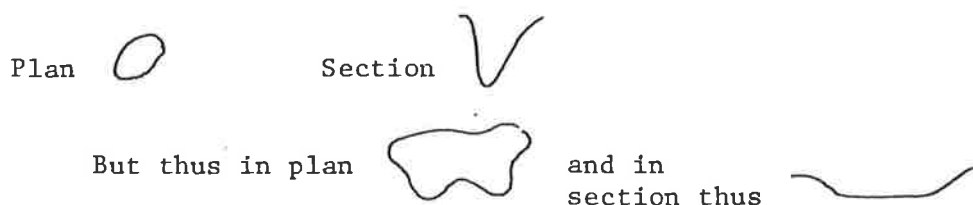
In studying the Victorian coast, the author, rather to his surprise, realized that he was himself concentrating his work on those places where prominent shore platforms exist, and neglecting the many long stretches of coast where the platforms are being destroyed, where sandy beaches front sand dunes, and even more strikingly, where great cliffs are washed by the sea, no platform feature whatever being developed. He then realized that, in fact, all previous descriptions, including his own, had been concerned only with parts where shore platforms exist, omitting references to the long stretches where there are none....

Hills seems to have overlooked Jutson's 1931 paper wherein, as has been shown above, attention was drawn to the very question of the absence of platforms; and in addition, most of that paper deals with coastal and near coastal features where there are no platforms. Furthermore, the bulk of the notes in MSS 83, 84 and Field Note Book No 45 from which extracts have been drawn concerns those parts of the Victorian coast where there are no platforms.

Extract 4 [MS 84 (8-10)]

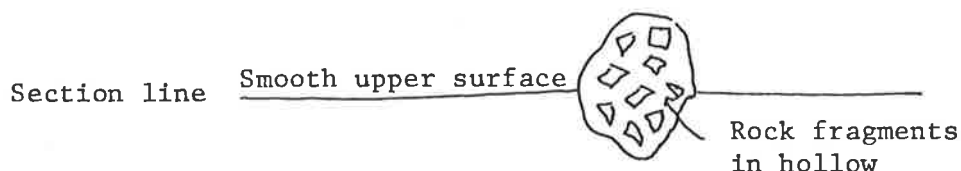
Noticed particularly today that when the surface of the ironstone platforms are exposed to the action of the sea, there appears to be a darker coloured harder patch of rock for a few inches from the surface down. Has this been formed since the cutting of the platform? At any rate it makes the rock harder for the waves to remove with the result that wherever possible there is an undermining of the lower softer ironstone.

A feature noted today were the number of narrow channels, cut by the waves, at rt angles to the shore in the ironstone platforms (i.e. the lower ones more particularly). These channels where the ironstone is brown & not too hard are sometimes very regular & very smoothly cut. Such occur on the N.W. side of Table Top Point or Corner. But in the black ironstone, they tend to be more irregular, probably due to the fact that the black ironstone is not evenly hardened & consequently the water, whilst making the channels roughly parallel as a whole, makes them somewhat curved irregularly. These channels may be from 3 to 4 inches in width & depth to 15" to 18" in width & depth. They must materially assist in the erosion of the platforms. In the same black ironstone there are irregular potholes, not the typical circular ones with smooth sides - thus in plan & section



being generally shallow compared with their horizontal area.

Another interesting feature of the black ironstone platforms is the general absence of rock fragments on the upper portion of the platform surface. Where the ironstone (black) is broken into fragments, those fragments remain in practically the same position & fill the hollow formed by their breaking up. They cover a hollow a few yards in different diameters; we may have in plan



In section thus:



Nearly all the fragments of ironstone both black & brown are angular or subangular. Very few are moderately water worn, except small flat pieces up to abt 1-1/2 inches in size. Much of the brown ironstone is comparatively soft & part of it & the whitish clays must be broken up & carried out to sea. I noticed a few well rounded shale or fine sandstone pebbles but they could be crushed under foot....

In places the ironstone platforms are traversed by irregular joints, widely spaced as a rule. The sea will cut channels in these from the surface down, widening as they go & so help to bring about the general destruction of the rocks. Where one platform is above another the water works from the lower platform upwards along the joints, with the result that the upper platform is undermined and falls away in large blocks along the joint planes, where it is further attacked by the sea.

As the platforms are so often exposed to the atmosphere, atmospheric weathering must assist in their destruction.

If earlier extracts gave support to the view that Jutson's observations took too little account of the details of coastal processes, then extract 4 vindicates him. Herein are described several processes, which, we note, all lead to the destruction of the platforms. Initially, Jutson describes surface hardening of the ironstone (by unspecified means) which promotes undermining of the softer underlying material. In addition, the presence or absence of the harder surface seems to exert influence in the character of the "narrow channels, cut by the waves, at rt angles to the shore in the ironstone platforms (i.e. the lower ones more particularly)". Then, in some detail he describes "irregular" potholes, contrasting them with the "typical cylindrical ones". It is clear that these potholes result from the fragmentation (by unspecified means) of the surface of the

platform, since the residual fragments are angular or subangular. The observed absence of rock fragments on the "upper portion of a platform surface," and the failure of the sea to remove rock fragments from the fragmentation potholes (as they may be termed), is a significant feature.

Jutson goes on to describe in a lucid way the role played by joints in the destruction of the platforms. And the final remark in the extract "As these platforms are so often exposed to the atmosphere, atmospheric weathering must assist in their destruction." is noteworthy for this reason; that Jutson suggests, (inductively and without following through), that atmospheric weathering is a significant agent in platform destruction, not platform development. And it is through giving close attention to the role of atmospheric agents working on shore platforms, that C. K. Wentworth later became prominent in the history of the study of shore platforms. (See below).

Extract 5 [MSS 84 (11); 83 (2-4) & 83 (9)]

At the seaward edge of a platform (I think opposite either E or A. See plan with notes of 15/10/33) the bottom was about 6 ft below the surface of the platform....

In the softer rocks of the cliffs, "chimneys" formed by the action of the rain. A very slight undermining (say 1 foot horizontally) by the sea at the foot of these soft vertical cliffs, will bring down a mass of rock & destroy the "chimneys". The latter therefore indicate a short period when there has been no actual abrasion of the cliffs....[84 (11)]

Quiet Corner, Black Rock 15/10/33

References to A. B. C. & D. attached

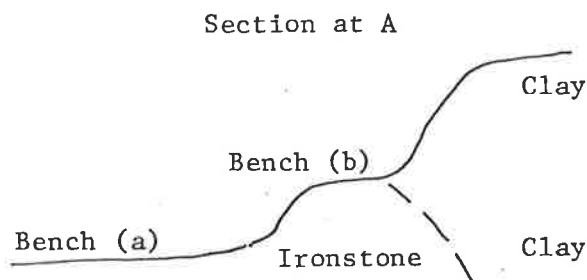
P Small wave-cut platform of ironstone with a slightly irregular surface

H 6/5/34 Wave-cut platform of clay. Above this (2 ft higher) is another of clay. The upper platform has been recently widened as shown by the undermining of the cliff & a great rockfall - All soft rocks

+

? Height (from memory) not more than 3 ft.

[MS 83 (2)]



Bench (b) Bench composed of ironstone about 4 feet above bench (a)

The sea as a rule does not cover bench (b) but the sea is removing the bench by attacking its base on three sides. 9/9/34. The inner margin looks as if it is smoothed & somewhat undermined by the sea.

Bench (a)

Is covered at high tide & is a fine marine bench, apparently without any doubt, cut at present sea level.

On its N.W. side it has been undermined & a lower one (about 18" below) formed. This lower bench passes into the sandy bottom of the small bay to the N.W. of A. This lower bench is being removed & so another platform is being formed, the depth of which is unknown.

We have therefore -

- (1) The top atmospheric erosion bench (bench(b)) but high tides could reach the surface, & probably do, for on the top is a pothole 18" deep & about 7" in diameter.
- (2) Wave-cut bench 4 feet below (1). This is bench (a).
- (3) Wave-cut bench 18" below (2).
- (4) Wave-cut bench - depth unknown.

The inner high cliff(?height) is composed of soft clay & the ironstone of bench (b) dies out horizontally practically at the base of the cliff (see section). The clay will soon be removed & then the ironstone will be left as a small stack similar to the larger ones off B & S of B.

Bench (b) has not apparently been cut, at least in the main, by the sea, but by atmospheric erosion of the soft clay above. This erosion has gone on faster than the marine erosion of the ironstone block between Bench (b) & Bench (a) i.e., the marine platform of Bench (a) has

not been cut so fast as the atmospheric platform of Bench (b).

Hence Bench (b) which might be thought at first sight to be a marine-cut bench & therefore evidence of uplift is merely due to differential erosion (atmospheric) at present sea level or alternatively it may have been cut by the waves wholly or in part at that level, as a high sea could probably sweep the bench but there was no evidence available of any recent marine action on Bench (b). [MS 83 (3-4)]

General Remarks

In some cases where the top of the ironstone is above the reach of the waves, the upper soft rock has been removed by atmospheric erosion as explained under A.

In other cases where the sea can erode at the top of the ironstone surface, it will pass along (over) the ironstone-surface, removing the soft rocks above rapidly. Should the ironstone die out or dip shoreward beneath the soft cliffs, then erosion of the soft rocks will proceed at a lower level, i.e. the more normal level of erosion, & leave the ironstone as a low stack swept over by the waves; but the edges of the ironstone are always being attacked by the waves so as ultimately to reduce the ironstone to the level of the clay platform. [MS 83 (9)]

Extract 5 includes some important observations and remarks. In the first place, Jutson gives one of the very few measures in all the literature to this date (1976) of the depth of the sea bottom at the base of the outer edge of the lower (intertidal, we suppose) platform. Plainly, such measures are not easily obtained. But bearing in mind that this outer edge of the platform is under wave attack and consequently retreating shorewards, the depth of the sea bottom at this crucial place is a valuable piece of data to obtain when possible. And furthermore, it is the retreat of this outer edge that produces what to this point Jutson has been calling "the sea bottom", but which later becomes his "ultimate platform".

The second part of the extract concerns "chimneys" in the soft cliff rock, their speculated origin by "the action of the rain", and their destruction by cliff recession following marine undercutting and collapse of cliff rock. The use of the presence of chimneys to indicate, however, crudely, the magnitude of the rate of cliff recession and hence of platform extension landward, gives a sense of the dynamics of coastal processes, and perhaps, too, gives a hint of Jutson's own desire to gain some quantitative idea of the rates of these coastal processes.

The long extract from MS 83 (3-4) is an early documentation of the supposed simultaneous development of platforms at several levels. Note the explanation of the uppermost bench in terms of atmospheric erosion of the backing cliff, although the presence of a pothole suggests the likelihood of marine action, and Jutson speculates that "A high sea could possibly sweep the bench". He thereby rules out the possibility that the bench owes its elevation to emergence. Note, too, the remark concerning "bench (a)" which "Is covered at high-tide & is a true marine bench, apparently without any doubt, cut at present sea level." Clearly, Jutson has by this time abandoned any speculation to the effect that this (intertidal) bench may have been cut at a lower level (or higher one, for that matter) relative to the sea. And again we find the invocation of relative rates of bench development to account for their existence where he says: "This erosion (atmospheric, subaerial) has gone on faster than the marine erosion of the ironstone block between Bench (b)

& Bench (a) i.e., the marine platform of Bench (a) has not been cut so fast as the atmospheric platform of Bench (b)."

In this passage too, is found an early description of the sea bottom as a "wave-cut bench - depth unknown" at the base of the seaward edge of the (intertidal) platform. This, as pointed out earlier, becomes his "ultimate" platform in later published works.

Finally, in the latter part of the extract is found use of the term "normal" in a context which explains openly what Jutson has in mind when he applied this term to the intertidal platform. "...then erosion of the soft rocks will proceed at a lower level, i.e., the more normal level of erosion...." The term normal here has plainly no reference to the "normal profile of equilibrium" of Fenneman (1902), nor to the derivative term "abnormal" as employed by Bartrum (1926). This is the earliest occurrence of its use in this sense found by the writer in the Jutson Documents, and is interesting particularly with reference to the problem of platform nomenclature to be discussed below.

Extract 6 consists of selections drawn from notes made at or in the vicinity of Torquay, some 30 km west of the mouth of Port Phillip Bay, on a stretch of the coast facing S.E. and fronting Bass Strait. Selections were made from this book because it is one of the few still extant belonging to the period of present interest, and because it deals with platforms on another rock type than those discussed above.

Extract 6 [Field Book No. 45, 28/1/34 to 1/4/35]

At the mouth of Break Creek (W side) there is a wave-cut platform exposed at low tide, composed of hard limestone the strata of which have a gentle dip to the N. A width of 20 to 30 yds is exposed by a length along the shore (intermittently) of about 100 yds. This platform has been cut very smooth & has a very low slope seaward (it is really almost horizontal). It is being removed by the waves & so another platform of unknown depth is being formed. The rocks are traversed by widely-spaced joints by which they are divided into large (i.e. say up to 8 feet in greatest length) polygonal rectangular blocks. The sea uses these joints to erode channels along & then in addition to the gradual reduction in size by the widening of the channels the rocks become undermined at a depth of about one foot (not determined whether there is a soft layer below which facilitates the undermining) & collapse, on which they are further attacked & gradually removed. The gentle northerly dip of the strata give rise to a succession of low (not more than 1 foot high) projections on the platform.

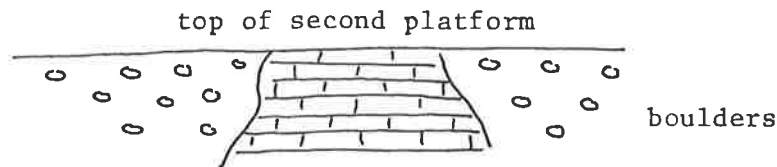
Thus in section



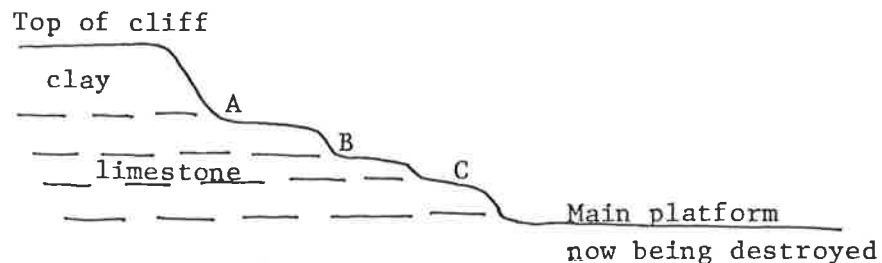
At the landward edge there are the remains of three other platforms (only a few ft wide & a few yds long) rising above the main lower platform just described, to a total height of 6 ft or more above the present beach. The top one appears to be out of reach of the ordinary high tides, as there is some grass growing on it and above there is an absence of boulders or pebbles on it.

The other two platforms have plenty of boulders up to 9" or more, all well water worn. Recent seaweed has been thrown up by the waves & in the incipient potholes there was sea water. The top platform is being undermined as well as the others, altho' no distinct beds of clay between the bands of limestone, as occurs at Torquay Bluff have been noticed.

Where these platforms are missing in places, boulders (well waterworn) & pebbles have been thrown up in abundance to a height equal to the top of the second highest platform. - Thus in section



Section through the platforms



B & C Platforms become covered by the sea at high or very high tides. Consequently the front of A is being eroded & A cut back. Similarly with B & C.

A. This platform, if never within the reach of the waves, could be formed by atmospheric erosion on the soft clay, the cliff of which would rapidly retreat leaving the platforms exposed. The dip of the rocks of the higher platforms is very close to the horizontal here so that their surfaces have the almost horizontal line. The total height above the present beach to which seaweed & boulders are carried by the waves is quite 5 feet.

In the upper platforms, joints also play an important part in the removal of the platforms, as when large thick blocks are undermined, they collapse along the joint planes....[pp. 29-37]

Some of these hard bands are well above the general level & yet the sea has acted along their surface. They are being undermined and removed. Their height is due in part to the folding of the strata, which tends to produce flat domes or half domes.

At the western end of the Point Danger limestone cliffs there are heaps of limestone shingle heaped well up at the back of the beach. The top of the heap would be on a level with some of the higher portions (such as the domes or half domes referred to above) of the platforms.

There is very little shingle on the main portion of the platform. There are a fair number of angular fragments of the platform rock, but practically no waterworn pebbles or boulders like the heaps on each side of Point Danger.

On the E side of Point Danger there is the continuance of the wave-cut platform in grey and red clay. On the W side 2 channels (about 20 to 30 ft wide) have been cut at right angles to the coast where the hard limestone band forms a tiny flat syncline & some of the grey and red clay forms the platform. Thus in cross section



Point Danger to Rocky Point

Narrow platform (exposed at low tide for about 1/4 mile W as mentioned above).

D (see p. 45)

Slightly undulating limestone cliffs with a narrow platform exposed in front of it at low tide. Irregular induration of the platform rock & consequently the small irregular weathering as before. At the landward edge there is apparently a more homogeneous rock - smooth at the surface in which potholes are formed.

Between Point Danger & D. The cliffs are low and are formed of grey and red clay. They are eroded vertically and cleanly to the base. A band of sand about 3 feet thick occurs at the top sharply marked off from the rocks below. The clean-cut face of these low soft cliffs is rather striking.

From D there are sand dunes to the mouth of Spring Creek with some rock exposures in the cliffs in places.

Rocky Point (immediately to the W of the mouth of Spring Creek). The mouth of Spring Creek is closed at low tide. Probably open at ordinary high tide. [Rocky Point] consists of limestone mainly in horizontal bands. High rocky cliffs and small stacks in front resting on the usual wave-cut platform exposed at low tide. The cliffs and stacks are frequently notched at their base just above the platform so

that they are being undermined and thus the platform extended. This action is very clear here.

The platform is much broken up by the waves and so a second one is forming at an undetermined depth, owing to a cover of sand and probably shingle.

A later inspection when the tide was in, showed the waves beating around the stacks and at the foot of the cliffs of the mainland. The undermining is well shown in the cliffs on the N side of the actual point. There are also some small natural bridges.

Small live mussels and other marine organisms occur on the rocks up to about 4 feet above the upper platform.

The latter is usually fairly level but it has the small irregularities noted earlier on the other portions. There are some fine rock pools, due to pothole action, up to 3 to 4 feet deep and 5 feet long. 2 or more may combine and an outer channel may break through one side of the hole and thus suddenly increase in length. The cliffs in places are undermined by narrow tunnels.

The rocks above the upper platform to a height of 4 to 5 feet are very honeycombed, the holes being up to 1-1/4 inches in diameter and up to 1/2" to 1" deep. Must be due to sea spray since the rocks above are only cavernous in places and then on a comparatively large scale.... [pp. 53-59]

A truly magnificent section, both geologically and from a scenic point of view. Bird Rock as a low stack with a platform around it exposed at low tide. This platform is being extended by the mainland cliffs and Bird Rock being cut back. Very clear and emphasized by the occurrence of notches and rock hollows (20 feet - at right angles - under the cliff) here. There are also long narrow passages under the cliffs.

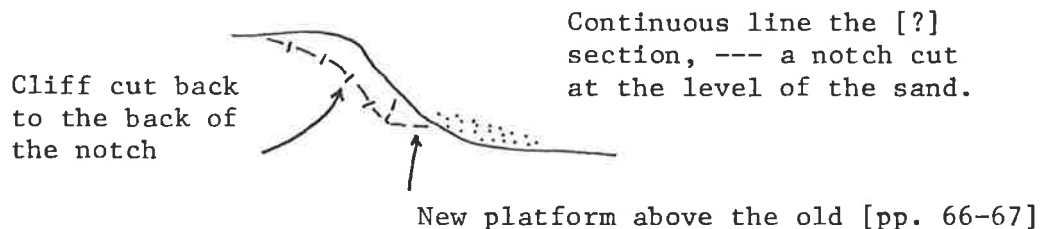
There is not the slightest evidence of elevation anywhere except possibly the high shingle bed on the E side of Point Danger.

The cliffs at Bird rock show hard and soft bands and so there are numerous hollows and ledges due to the waves at the base and higher up to atmospheric erosion.

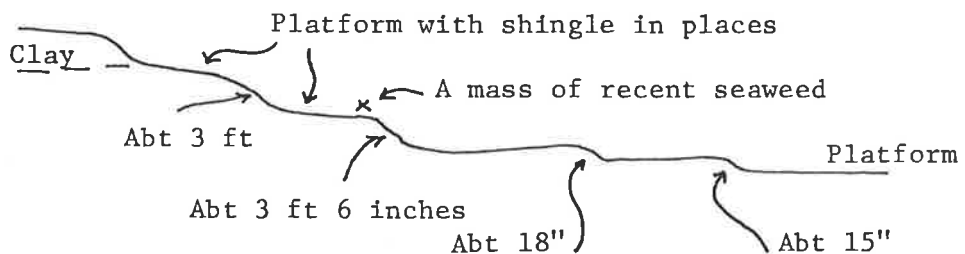
Note

Along the cliffs in places where the ordinary high tide

reaches to the foot of the cliffs, considerable quantities of sand (probably several feet thick in places) is banked up amongst the cliffs to make a smooth seaward sloping beach. This protects the rocky platform beneath from extension into the cliffs for a time at least. It is possible however (but this has not been verified) that the cliff may be cut backward at the level of the sand. If so and the sand is subsequently removed we would have a tiny platform above the general platform below, which might be taken as evidence of elevation. Thus in section -



....The outcrop of limestone and the wave-cut platforms on the western side of Bream (or Thompson's) Creek were well shown today. A cross section would be (in one place)



Below the lowest platform exposed at low tide (that shown above) there is another platform, the depth of which is not known. Its surface is covered with sand or shingle or both. This is the typical lowest platform found everywhere along the coast.

The various platforms are made up of beds of hard limestone. The water probably reaches to the top of the highest but not everywhere along that platform as it is not level but curved....[pp. 81-83]

On Note Book pp. 29-37 are described platforms near the mouth of Bream Creek formed on bedded limestone. Of particular interest

is the occurrence of platforms at various levels to six feet above (mean?) sea level; the observed influence of structure in producing minor irregularities on the platforms' surfaces; the formation of the lowest platform below sea level; and the destruction of all platforms of marine origin at and above sea level, in which erosion along joint planes and subsequent collapse of blocks plays a primary role.

Again, Jutson's notes display an (unstated) commitment to the notion that the "lower" platform (intertidal) is the one ordinarily to be expected, for he says of the site at Rocky Point (ibid., p. 54): "High rocky cliffs and small stacks in front resting on the usual wave-cut platform exposed at low tide. The cliffs and stacks are frequently notched at their base just above the platform so that they are being undermined & thus the platform extended." At the same time his observations show that this platform is "much broken up by the waves & so a second one is forming at an undetermined depth, owing to a cover of sand & probably shingle." (Ibid., p. 57)

On the following page of the note book, Jutson gives passing attention to honeycombing of rock four to five feet above the upper platform; a feature, he states, which "must be due to sea spray since the rock above is only cavernous in places & then on a comparatively large scale." (Ibid., p. 59). Jutson is here open to the criticism that his exploration of the process here involved falls short of desirable penetration. It is in fact a little odd that his enquiry should stop so abruptly, since it is not altogether in keeping with his earlier style as exemplified in his work on arid zone geomorphology.

And furthermore, in none of his published works, nor in his notes, did he link weathering on the fringes of the lakes of Western Australia particularly by fretting induced by salt crystallization with processes at ocean or bay shorelines. One brief reference in the closing remarks of a letter to J. A. Bartrum dated 14th August, 1937, indicates that he at least had his Western Australian work in mind, although he apparently saw no application of it to the present problem, for he says: "I hope shortly to be out in the field again when I shall pay particular attention to pitting amongst different classes of rocks. I have studied pitting pretty closely in semi-arid Western Australia, and, if you would like a copy of my paper on that subject, I shall be pleased to send it to you, not of course that it will help in the spray-pitting problem." [Letter No. 176, Vol. II]

In fairness to Jutson though, the following letter written to Bartrum and dated 26th June, 1937, ought to be placed before the reader, and now follows -

Dear Professor Bartrum,

Many thanks for sending to me a reprint of your paper "Honeycomb Weathering of Rocks near the Shore-line" which I have read with much interest.

Similar weathering occurs along the New South Wales coast between Sydney and Broken Bay in homogeneous Triassic sandstones and shales (which are very similar to the Upper Permian sediments at Newcastle) and in the Jurassic sandstones and shales at San Remo (to which you refer), and there is no doubt that, in practically every case, the honeycombing is due to sea spray. Neither wind nor rain has any influence in the formation of the pits, except that probably the wind may remove some of the disintegrated material. I, therefore, agree with you as to the general origin of the pits.

In the localities mentioned by me, pitting occurs on vertical walls as well as on the gently inclined or sub-horizontal surfaces to which you refer. Even on vertical faces, the minute irregularities that occur would permit the lodgment of some water.

Your statement as to crystallization and solution being the probable cause of the pits, I am disposed to agree with, although I have not paid enough attention to the phenomena to express a definite opinion. I am inclined, however, to think that, in addition to the causes mentioned by you, the mechanical force of the sharp blows of the drops of spray must find weak spots, and so commence or aid in the initial formation of the pits.

Along portions of the Victorian coast where the rocks consist of dune limestone, I believe pitting occurs both by spray and rain, for the carbonate of lime loosely contained therein in the form of minute fragments of shells is easily dissolved both by rain and sea spray. I have not hitherto paid much attention to the pitting of these rocks, but will extend my observations as opportunity permits.

Along that portion of the New South Wales coast to which I have referred above, I am convinced that the action of the spray is not only strong enough to honeycomb the rocks, but is also largely responsible for the formation of narrow shelves well above ordinary high tide mark. This I pointed out in the paper that was read at the Auckland Meeting of the A.N.Z.A.A.S. in January last. I hope now to have the substance of that paper published either as a whole or in parts. [Letter No. 175, Vol. II]

Continuing with the consideration of the note book extracts, Jutson describes on page 67 a platform exposed at low tide at Bird Rock, citing evidence to support the view that the platform is being extended. Following this description, he states: "There is not the slightest evidence of elevation anywhere except possibly the high shingle bed on the E side of Pt. Danger." One would suppose that this latter bit of evidence, the "high shingle", would warrant close scrutiny, for if indeed emergence had recently occurred there, interpretation of the platform occurrence could be radically different,

a matter to which attention is given shortly.

Jutson goes on to describe "hollows & ledges" in the cliffs "due to waves at the base and higher up to atmospheric erosion.", touching on an aspect of coastal processes that receives expanded and published attention in his 1939 paper entitled "Shore Platforms near Sydney, New South Wales."

In the final part of the extract, from pages 81-83 of the Note Book is found description of a sequence of platforms in limestone, and then the statement: "Below the lowest platform exposed at low tide (that shown above) there is another platform, the depth of which is not known. Its surface is covered with sand or shingle or both. This is the typical lowest platform found everywhere along the coast." (Note Book p. 83. Italics added)

Turning again to published accounts of shore platforms, Bartrum's 1935 paper contributes nothing substantive to their study, reiterating his earlier published views (1916, 1924, 1926 & 1928). He sustains the recognition of the Old Hat intertidal and storm-wave platforms as fundamental forms, notes the importance of the relative rates of platform growth and destruction in their genesis (see Bartrum, 1935, p. 139), concludes that "maximum wave efficacy is above, not below high-water level", and recognizes the importance of subaerial processes - wetting and drying, and salt crystallization - in producing plane surfaces on originally wave-cut platforms. This last idea Hills (1949) rightly notes resembles Wentworth's (1938) water-level weathering.

C. K. Wentworth's 1938 and 1939 papers are notable for several reasons. The style is excellent, material being presented in a clear and unequivocal way; they are based on close field observations; in them, platform morphology gains clear definition establishing the terms visor, nip(notch), pitted zone (for platforms on limestone), abrasive ramp, rampart on the seaward margin of the platforms, and marginal cliff; and processes in platform genesis or modification receive a definition and detailed treatment unmatched in previous published works of any author.

Oddly, Wentworth's papers appeared without any reference to the publications of Australian or New Zealand workers. Indeed, to this time, even among Australasian workers, there was an independence of activity that is somewhat surprising; Bartrum's 1916 failure to acknowledge (from ignorance) the work of Bell and Clarke (1909), and Jutson's work in the early 1930's in seeming ignorance of Bartrum's contributions are cases in point. However, this pattern was changed in the late 1930's following the publication of Wentworth's papers of 1938 and 1939, and of Jutson's 1939 paper on shore platforms near Sydney. By this time, Jutson at least had written communication with both Wentworth and Bartrum. And from this time until the mid 1950's several workers published papers which evince awareness of each others contributions on platform morphology and genesis. To some of these we now turn, beginning with a brief overview of Wentworth's ideas.

Wentworth (1938) postulates four main processes operating in

"forming or modifying benches on Oahu shores" (p. 12), namely, water-level weathering, solution benching, ramp abrasion, and wave quarrying. A necessary prerequisite to the operation of the first two of these is a surface formed by some other means. For the first we read:

Water-levelled benches are developed and widened at whatever level water can accumulate and be retained in initial basins produced by the erosional work of some other agent. (p. 20)

Later he identifies the means whereby the initial surface may be produced, stating:

Within the zone which can be reached by waves or heavy spray action, differential weathering of rock such as tuff produces pits and irregular depressions. Weathering is aided by wind action, wash, and other erosional processes. Wave quarrying may also produce undrained depressions....Given such pits or a rough bench due to merging of such pits, in this zone, which is higher on exposed coasts, the retention of water pools initiates the water-levelling process." (pp. 29-30)

Note that effective action is higher on exposed than sheltered coasts, reminiscent of Bartrum's cited view, and that of J. D. Dana (1849).

For solution benching restricted to limestone coasts, there is again a prerequisite surface, not specifically defined as such, but implied, for we read:

The solution bench is found on both types of limestone [reef limestone and detrital limestone] but is more typically and extensively developed on the reef limestone. This is probably because the bedded structure and less perfect induration of some of the detrital limestone tends to favor a structural control of bench forms and the process of wave quarrying and ramp abrasion.

The exact form of shore profile developed by solution on limestone apparently depends on steepness of the initial shore....(1939, pp. 4-5)

Ramp abrasion and wave quarrying do not, ideally, produce a level surface as the following shows:

Wave quarrying, subject to the structure of the rock, develops a shore profile with a broadly rounded nip, and a vertical to strongly sloping curve passing below sea level. Ramp abrasion tends to produce a profile comparable to that of a beach formed of the materials by which the abrasion is carried on, and often at the margin of its action cuts a sloping kerf into the adjacent rock. Where wave quarrying and ramp abrasion merge and somewhat combine characteristics the slope and forms are intermediate. (1938, p. 29)

Furthermore, Wentworth applies the term "wave planation" (Ibid.) to processes which result in the "degradation of a surface below sea level, to approach the level of effective wave movement" which "can obviously not be invoked to explain a kerf of such limited width and with generally higher elevations on more exposed points. Both wave quarrying and ramp abrasion are eliminated by the levelness of the bench surface in question and by the presence of the rampart." (Ibid.)

Thus, while ramp abrasion is a process operating in a defined zone between the level bench and the nip (notch), wave quarrying plays a twofold role in Wentworth's scheme. Firstly, a genetic one in producing "undrained depressions" during the initial phases of platform growth which allows the water-levelling process to operate, and a destructive role at the seaward margin of the platform, which, we suppose, is part of the process of "wave planation" which produces the surface below sea level mentioned above.

Finally, it should be remarked that water-levelling "is effect-

ive between two and twenty feet above sea level. Locally the height of most effective weathering depends on the prevailing vigor of wave action." (1938, p. 20); whilst the solution bench "if ten or more feet wide, is, without significant exception, formed between one and three feet above mean sea level, and hence furnishes one of the most definite sea level indicators known." (1939, p. 9) And in addition, "It is the writer's belief from close study of the entire coast of Oahu that the present benches affected by water-levelling were mostly cut when the sea was somewhat higher than it is now." (1938, p. 30)

In his first published account of the morphology and genesis of shore platforms, Jutson refers, in passing, to "the platforms of the textbooks" (1939, p. 244) with reference to the "ultimate" platform. It is instructive at this point to consider some of the textbook treatments of the topic as expounded in the works to which Jutson had access; these not only expose the background of Jutson's thought, but also may be taken as fairly representative of the general state of knowledge in the nineteenth and early twentieth centuries. The source of the references is Jutson's own notes on his reading.

MS 78-2 includes a brief extract from H. T. de la Beche's Researches in Theoretical Geology (1834), wherein de la Beche describes the wasting of the cliffs in London Clay on the Kentish coast, and the growth of a surface (platform) below sea level. Jutson notes: "See also p. 65, Fig. 4. Diagram showing an uneven sea-cut platform below sea level in front of the wave cut cliff." (MS 78-2)

MSS 79-2,3,4 consist of extracts from A. C. Ramsey's "Denudation of South Wales", being Memoir No 1 of the Geological Survey of Great Britain (1846), and from which we read:

The line of greatest waste on any coast is the average level of the breakers....The effect of such waste is obviously to wear back the coast, the line of denudation being a level corresponding to the average height of the sea [Jutson's italics]....

To such combined forces, namely, the shaving away [Ramsay's italics] of coasts by the sea, the spreading abroad of the material thus obt'd the great plain [Jutson's italics] of shallow soundings which generally surrounds our isles is in all probability attributable. (MS 79-3)

Jutson's own remark appearing on MS 79-4 not only applies to the work in question, but generally to treatments of the subject in the nineteenth century to the best of the writer's knowledge. But let the reader judge for himself.

No details as to the exact nature of the platforms cut by the waves. The plain of denudation is only ref'd to in the general terms stated above, and practically no other information is given than that noted above. There is just the general conception of a plain formed by cutting away the cliffs & spreading the debris over the sea-bottom - (i.e. on the continental shelf altho' that term is not used). (MS 79-4)

C. K. Gilbert's treatment of lake shore features in the Report of the Director of the United States Geological Survey, Volume III (1884), does not reflect any substantively new thought on shore platforms, but we note, as Jutson did (MS 133-3; from H. Ries & T. L. Watson, 1936) that Gilbert applies the name, newly we believe, "wave-cut terrace" to the "submerged plateau whose area records the landward progress of littoral erosion" (Gilbert, 1884). This feature he diagrams as a subaqueous surface sloping away from the base of the

cliffs. (Ibid. Plate IV, Fig 1)

A Geikie's Textbook of Geology (4th edition, 1903) which Jutson consulted (MS 166-2) gives graphic descriptions of the role of wave action in coastal denudation, but, other than accepting the notion that land masses will ultimately be reduced to a marine platform such as the North Sea and the Atlantic Ocean bottoms adjacent to the British Isles and Norway (ibid., p. 595) by both subaerial and marine erosion, cut-rock platforms do not catch his attention, to judge from this work. Indeed, a generalization he made that "A precipitous coast commonly rises out of deep water; a low coast is usually skirted with shallow water, the line of slope above sea-level being in a general way prolonged below it." (ibid., p. 595) would preclude the development of cut rock platforms along cliffed coastlines.

MS 82-2,3, includes extracts from J. Le Conte's Elements of Geology (1902), one of which is as follows:

Waves, cutting ever at the shoreline only, act like a horizontal saw [*italics Le Conte's*]. The receding shore-cliff, therefore leaves behind it an ever increasing subaqueous platform which marks the amount of recession. This is shown in section (Fig. 28),..." (MS 83-2).

On the following page of Jutson's notes (MS 83-3) is a copy of Le Conte's section showing "sea cliff, wave-cut terrace, and wave-built terrace". It is clear that, like de la Beche, Ramsay, and Geikie, Le Conte viewed the platform as a feature formed below low-tide level.

Jutson's notes from The Scenery of England by the Rt. Hon Lord Avebury (1902) are of interest because unlike the other authors to

whom Jutson turned, Avebury specifies that:

If the tides acted alone on the shore, the outer or (p. 126) lower edge of the platform could correspond approximately to the low water, and the inner or upper line to the level of the waves at the highest tides; but to these limits some allowance must be made for the height to which waves must rise in storms, and the depth to which they act below the surface....

The planing action of the sea does not, indeed range much below low-water mark (Godwin Austen "The English Channel" Q.J.G.S. vol. vi, 1851)....(MS F2 134).

The extract continues with references to the depths of subaqueous plateaux in the vicinity of the British Isles. These examples, oddly, do not well substantiate the case that marine action is concentrated at about mean sea level, with allowances for storm waves, for the cases cited range in depth from 36 feet below sea level for The Pomier, to fifty-sixty fathoms for the Little Sole Bank.

Two other authors demand brief attention, namely, N. M. Fenneman and D. W. Johnson. Fenneman's 1902 paper "Development of the Profile of the Subaqueous Shore Terrace" stands apart from the works considered above in that it is almost entirely deductive in its presentation. The physics of wave motion, oscillatory and translatory, is outlined in detail and applied to the coastal scene, together with the subsidiary motions of tides and currents. From this follows a treatment of the factors in shore profile development, namely, oscillatory wave action and undertow which carry material away from the shore; on-shore currents and translatory waves which carry material towards the shore; and longshore currents. And finally, Fenneman describes the "normal profile" of the "cutting coast" and of the

"building coast", with an account of variations of these. The forms so described are deductively constructed, and the treatment is poorly supported, lacking specific examples, and has no diagrams to illustrate the profiles described. In this scheme shore platforms do not assume any prominence, being merely hinted at in a rather muddy passage of the paper (p. 28, para. 2). The failure of both Johnson and Fenneman to take shore platforms into account may be explained, as suggested by E. S. Hills (personal communication), by their rare occurrence on North American shores.

D. W. Johnson's well known classic (1919) in which coastal morphology is cast into an evolutionary framework reminiscent of W. M. Davis' schema for landscape evolution, does not deal with shore platforms of the kind encountered in Australasia, as the remark above foretells. To be sure, he defines the "wave-cut bench" as "a sloping erosion plane inclined seaward" (*ibid.*, p. 162) in front of the cliff and including all of the shore and part or all of the shoreface. He describes also the "abrasion platform" which extends seaward beyond the low tide shoreline. But these are not to be confused with shore platforms of the kind in question, as Hills points out in reference to Johnson's abrasion platform (1971, p. 148). We note too, that like others before him, he believed that marine planation would ultimately reduce a landmass to the level of the wave base. For, in his own words: "A cycle of wave erosion ends, therefore, when all the land is reduced to a plane surface about 600 feet below sea level." (*Ibid.*, p. 245)

If Wentworth's papers (1938, 1939) served to focus attention on process in shore platform development, then it is Jutson's 1939 paper which played a pivotal role in establishing the idea that several levels of benches can be formed simultaneously above and below mean sea level. These are spelled out on pp. 239-240 of the paper thus:

At the foot of practically every headland, and at other places along the coast where rocky cliffs occur, are one or more wave-cut platforms above which may be seen narrow shelves and ledges. The "platforms" or benches exposed between mean low tide and mean high tide may be called "normal" platforms, and those with an average height of about three feet above the normal platforms may be termed "high-level" platforms. The "shelves" are benches from three to twenty feet wide; and the "ledges" are benches up to three feet wide. The shelves and ledges are usually narrower than the platforms, and the high-level platforms are usually of much less width than the normal platforms. Well below the low water mark is a third platform, which may be designated the "ultimate platform."

It is clear from this that Jutson's "normal" platform is the Old Hat type in the sense that it is an intertidal feature, the one recognized by many both before and after Jutson entered the scene. The term "normal" is meant to imply no more than that it is the platform normally (usually) encountered in the intertidal zone. Jutson's "high-level" platform is plainly equivalent to Bartrum's "storm-wave" type, while the "ultimate" platform is here newly recognized, although Jutson explicitly equated it with the "sloping erosion plane inclined seaward", to use Johnson's phrase, featured in early treatments of the matter as we have seen above.

Later Jutson distinguishes between the "primary ultimate platform" and the "secondary ultimate platform". The first "...coincides

with the wave-cut platform of the textbooks" (1940, p. 173), develops in fresh granodiorite in case of the platforms discussed in this paper, and unlike the normal platform, is not exposed at low tide. The "secondary ultimate platform" results from the destruction of normal platforms at their seaward edges.

The processes whereby these, and the normal platform and shelves and ledges above it are produced are described throughout the 1940 paper. In summary, the shelves and ledges are formed by removal of debris, by waves or spray, "formed by atmospheric erosion and perhaps even by direct abrasion...." (p. 173). The normal platforms are extended landward by "abrasion by the sea at the foot of the cliffs" (p. 169) which Jutson elsewhere describes as "wave planation", and this is aided by "alternate wetting and part drying, which may aid in the disintegration of the rocks and thus facilitate their removal by the waves." (p. 170). This latter process Jutson states does not "fulfil the conditions of (Wentworth's) water-leveling," for reasons that are not made clear. The ultimate platforms are produced by the "quarrying action of the sea and wave planation combined" (p. 174).

In these two papers Jutson expounds the essence of his ideas on platforms, ideas which were established in the course of his field work at Victorian sites in the early 1930's. Subsequent papers, as we shall see, add qualifications but no essentially new concepts.

A. B. Edwards' 1941 contribution entitled "Storm-wave Platforms" deserves brief consideration here because it impinges on Jutson's

work through Edwards' critique of Jutson's platform terminology. The paper, however, is defective in two regards. Firstly, it includes a fundamental misreading of Bartrum's 1926 work, since, although Edwards' paper is about intertidal platforms, and hence concerns Bartrum's Old Hat type and Jutson's normal platform, he states that he prefers the term "storm-wave" platform for them in seeming ignorance of the fact that already Bartrum had applied that term to the narrow ribbon-like platform occurring above high tide level on exposed coasts. And secondly, the paper concludes with a number of summary points several of which are of questionable merit, for points 1, 2, and 3, are non-falsifiable and hence non-scientific (Ackerman, 1970, & Popper, 1972), and points 4 and 5 cannot be substantiated although they could be falsified (see Edwards, 1941, p. 236).¹²

Taken as a group, Jutson's papers on the shore platforms at Flinders (1947), Lorne (1949a), and Point Lonsdale (1949b), Victoria, are significant not because they impart any basically original ideas, but because of the close attention to detail of the form of, and the processes operating on, shore platforms; and this couched in a well structured framework, blending description of morphological features with assessment of origin in terms of structure, weathering, and

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For example, point two reads: "They only develop where a suitable combination of wave vigor, rock hardness, cliff height, and tidal range exists." This statement says, in effect, that where platforms occur the conditions are suitable for their development, and where they don't, they are not, a proposition that plainly cannot be falsified. And point four which reads: "Other conditions being equal, the higher the cliffs, the narrower the storm-wave platform." could conceivably be falsified but cannot be substantiated, for who is to decide if other conditions are equal?

erosion. Occasional lapses from clarity and sound logic, and application of the term "high-level" in his 1939 paper as applied to platforms above the level of the normal variety, do not detract seriously from the value of the contribution these papers make to the topic.

In addition, he replies to Edwards' (1941) criticism of his use of the term "normal" as applied to intertidal platforms in a logical and valid way.¹³

Jutson (1950b) in his penultimate publication, tackles the question of shore platform terminology by first outlining the historical development of it, then presenting a series of objections to the terms then in use, including his own, and finally presents a new terminology and classification. Briefly, he recognizes two fundamental types which he designates "The Major Platform" and "The Minor Platform", the former including his earlier primary and secondary ultimate platforms (now the "primary" and "secondary" platforms), and the latter includes five sub-types designated "'Old Hat' type"; "Wave-erosion types" which include "the 'abnormal' or storm-wave platforms of Bartrum,...the two upper platforms of Jardine (1925), the 'normal' platforms of the writer, the two-meter bench of Douglas Johnson, and, apparently, the storm-wave platform of Edwards." (1950b, p. 75); "Spray-erosion types" which include his previous ledges and shelves at considerable elevations above sea level; "Water-level-

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The reader is referred to Edwards (1941, p. 228) and Jutson (1949a, p. 48) where the criticism and reply are found.

ling types" answering to Wentworth's platforms (Wentworth, 1938); and "Solution types" again agreeing with Wentworth's platforms, those formed on limestone (Wentworth, 1939).

This attempt to impose some semblance of order onto what had become a confusing array of terms is indeed laudable; but for unknown reasons the system failed to take hold, or even to evoke, as far as the writer is aware, any serious discussion. What some may regard as its principal merit is that embodied in it is the notion (not explicitly stated) that terminology should not only be descriptive of the feature in question, but should also carry genetic implications. In Hill's (1949) words:

To be acceptable, a physiographic term should relate to a clearly defined set of conditions and processes, so that the genesis of the feature designated by the term shall be understood. Jutson's terms [pre 1950] are, however, largely descriptive and therefore highly subjective....

It is hoped that a nomenclature based on genetic conceptions, as partially outlined by Wentworth (1938, 1939), may eventually be arrived at. (pp. 140-141)

Note, however, that desirable as the above principle may seem, it is not usually applied in practice in scientific work. And furthermore, in Hills' 1971 treatment in which shore platforms and associated features receive the most comprehensive descriptive analysis yet published, the terminology used is, with few exceptions non-genetic. Jutson's classification on the other hand embodies terms which carry genetic implications, for example "Spray-erosion type", others which are only descriptive such as "Old Hat type", and others which are neither, like "Primary" as a sub-type of the major platforms.

But surely a term to be useful should clearly designate what morphological feature, regardless of genesis, is in question, and such a term is "intertidal platform" used by the writer in this text following Hills' use of it for all platforms between low and high tide levels.

Along these lines, a logical primary classification would include "super-marine platforms", "intertidal platforms" and "submarine platforms". Within this framework may be placed all those recognized by the workers who have contributed to the topic as having this or that genesis as expressed by such terms as "storm-wave bench" (Bartrum) or "solution bench" (Wentworth), according to the interpretation of the worker.

These considerations aside, Jutson's classification in the writer's view further complicates an already confused situation. For example, the writer earlier equated the Old Hat type with Jutson's normal platform, principally because both are intertidal features. And yet Jutson (1950) set the Old Hat type apart because, accepting Bartrum's view that they are formed by the removal of sub-aerially produced rock waste by weak waves, added that he "has not observed similar forms, nor does he know of any." (1950, p. 75) In addition, ledges and shelves included under "Wave-erosion types" also feature under "Spray-erosion types" (*ibid.*, p. 77).

Jutson's final statement on platforms (1953) focusses on the processes operating thereon, with particular reference to those at Lorne, Victoria. Whilst no detailed review of the contents are here warranted, we observe that it is fitting that his last published

paper should focus on process in landform genesis wherein, as in his earlier work, he draws on that for which his life's work is most valued, namely, the close attention in the field to processes now operating.

A reassessment

In the course of reading Jutson's 1931 paper, and particularly pages 147-149 quoted above, the writer was struck by the passage:

This raises the question whether the platforms have been cut as they now stand at present sea level, or whether since their cutting there has been an uplift of the land. If the latter has taken place, then it is a striking coincidence that the platforms should be uplifted just to low water mark. (p. 147)

This question is not newly raised by Jutson, for Andrews (1916) before him, referring to the benches around Sydney at Long Reef states:

These platforms vary in width from a few yards to as much, perhaps, as 150 or 200 yards. They are awash at high tide....(p. 175)

And to substantiate his case that "the present shoreline of eastern Australia is a feature of emergence of very recent origin, probably not exceeding a few hundred years." (xxxix), he states as one piece of evidence:

The fact that along the whole south-eastern and eastern side of Australia rock platforms and lines or zones of marine erosion occur at and above high-water mark. (xxxviii)

And shortly thereafter comments:

The main attack of the storm waves today appears to be on the edge of the lowest platform, in the case where more than one exists, and there is thus evidence

of two or more lines or zones of marine cutting vertically above the other. This in itself is in direct contradistinction to the idea that the sea has cut the platforms at the same time, inasmuch as the tendency of the sea wave is to establish a simple profile of erosion on the stable shore line. (xxxviii)

Jutson's rejection of the idea that platforms were cut at a lower level then uplifted is based firstly on the coincidence problem, and then upon an argument that is forceful and hard to put aside. But in reflecting on the coincidence of elevation of a supposed submarine platform to about mean sea level, the writer asked why this need be regarded as such, and the following alternative argument arose.

Suppose that in fact Fenneman and others were right; that the sea cuts but one platform sloping variously, depending on local conditions, from the high-tide shoreline seaward, and molded by submarine erosion and deposition. If this coastal zone emerged above sea level, then plainly it will be ultimately removed by coastal marine processes or modified by subaerial agencies. If it emerged to within the intertidal range it would be observed as an intertidal (normal) platform and subjected to modification by coastal processes. If it were raised (relatively) to a level below the intertidal range it would not catch attention and merely be part of the submarine morphology.

To state, as Jutson does, that "it is a striking coincidence" implies that the level of probability that a submarine platform should be raised just to mean sea level is too low for such a notion to be acceptable. However, if intertidal platforms occur

only along a low percentage of the total length of the world's cliffed coastlines, then it need not at all be regarded as coincidental that a submarine platform should be elevated to within the intertidal range. Just what percentage of the world's cliffed coastlines have intertidal platforms is unknown to the writer, but at a guess it is very low.¹⁴

Attention has already been drawn to Jutson's and others' repeated observation that the "normal" intertidal platform is fringed seaward by a "low-tide cliff" which is being actively eroded by wave quarrying and thus receding landward, and have noted that Jutson early explained the absence of any platform at all, as often occurs, at some localities by invoking his relative rate hypothesis to the effect that if the rate of low-tide cliff recession is greater than the coastal cliff recession rate, then the intertidal platform would be destroyed and be replaced by the "secondary ultimate platform".¹⁵

But in addition to the destruction of the seaward margin of the intertidal platform, all the processes that Jutson described as operating on the platform, namely, quarrying, planing, grooving, flaking, pitting (honeycomb weathering), and minute furrowing (1953),

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The study of Inman and Nordstrom (1971) is interesting in this regard. In their presentation of new classifications of coasts, shore platforms do not get a mention as such, the closest being "raised terraces" (undefined) that, together with sea cliffs and narrow continental shelves, characterize "collision" coasts. But no hint of the proportion of cliffed coasts that feature "raised terraces" is given.

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This relative rate hypothesis is very appealing. It is logical; it

p. 426), are destructive of it. And as Edwards (1951) points out "All the processes enumerated by Hills (1949) are negative or destructive processes." (*italics Edwards'*) in substantiation of his own case that storm wave action is "the one positive or constructive process in platform building." (*ibid.*) by causing recession of the main cliff. The writer does not wish to imply that coastal cliffs do not recede under wave attack, but acceptance of this does not carry with it the necessity to accept the view that intertidal platforms owe their primary genesis to this process operating at mean sea level.

If there is one matter regarding shore platforms that has received minimal attention it is that of their initiation. In all the papers cited by the writer, as well as others not cited, this question receives but cursory treatment, which commonly in-

could conceivably be substantiated; and it could conceivably be falsified. And after all, it embodies a principle of universal application in geomorphology, as of other sciences, namely, that the form of all features of the earth's surface, both small and great, are in the last analysis the consequence of an interplay of processes (forces) operating at different rates, and the relative magnitude of these rates will determine the present observed forms. These forms may be in quasi-equilibrium or may be in an unstable state moving towards a new equilibrium in response to changed or changing relative rates.

It should be added, however, that there is not general agreement that the low-tide cliff at the margin of the platform is significantly eroded. Sunamaru (1975) cites laboratory flume experiments evidence to show that it is not. "The cliff portion below this level was always submerged, and, therefore, the water acted as a sort of shock absorber. The result of this buffer action was that this zone remained unchanged. This indicates that the subaqueous bluff at the seaward margin of actual platform could quite possibly maintain its initial slope, as Cotton (1963) suggested." (p. 396) This suggestion of Cotton (1963, p. 101) was made with reference to the Old Hat type in sheltered bays.

volves submergence of a "mature" landscape by one means or another, and the proposal of a level of optimum wave attack, particularly of waves of translation.¹⁶

On the other hand, there is universal agreement that with few exceptions (atolls and islands of volcanic origin) landmasses are fringed by a submarine slope, the coastal margin of the continental shelf. The emergence of this to within the intertidal zone by eustatic or tectonic means provides the simplest explanation for the initiation of shore platforms. This hypothesis is appealing because it is simple, and it is falsifiable. And, it is argued, it does away with the need for the plethora of arguments that cited authors and others invoke to account for this or that feature of the platforms, or why they do or do not occur at one place or another.

The hypothesis is falsified if, where platforms occur, there is no evidence of emergence. Applying the test, then, we note that although Jutson could find no evidence of uplift in Port Phillip Bay (although he was indecisive on the matter; see Jutson 1937), Hills did, as recorded in 1940, and as we have noted above Andrews found evidence of uplift along the eastern shoreline of Australia. And in the writer's own experience at all localities on the Fleurieu Peninsula, South Australia, where intertidal platforms occur, there

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Waves of "translation" would include the "breaking wave" and "broken wave" of Sunamaru (loc. cit.), both of which forms would supposedly, be encountered in nature. Sunamaru's flume experiments show that breaking waves produce a platform below stationary water level, while broken waves form one at this level.

is unequivocal evidence of uplift provided by both geomorphological and geological evidence (Brock, 1971, 1974).

The single exception to this hypothesis for the origin of shore platforms is the case of such on reef limestone (Wentworth, 1939; Fairbridge, 1948, 1950). But since reef-building organisms flourish in a zone close to sea level (Bird, 1970), this case is not in fact an exception, but rather a different one altogether.

The suggestion offered above, incomplete as it is, is made to invite a renewed examination of the question. Perhaps, after all, Bartrum was right in calling his intertidal platforms "abnormal", but for the wrong reasons.

Reflecting on this history of the study of shore platforms with a view to seeing Jutson's place in it, it is clear that he was not a pioneer in their study. Jutson (1950a) acknowledged Bartrum as the pioneer, and in the sense that he captured the attention of the geological world regarding these features, he was indeed.¹⁷ But Jutson's role was clearly important to the development of shore platform study, building on the work of others, adding insights of his own, and providing a wealth of observation to serve as a basis for his own speculations and those of others.

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We have earlier noted the general agreement that Dana, as official geologist on the Wilkes Expedition, was the pioneer of the study of these features, at least in the Pacific region. Hills ascribes to Dana the part of naming the "Old Hat" type (Dana, 1849). However, a rereading of Dana's commentary strongly suggests to the writer that Dana did not originally name the feature, but merely accepted the name already in use, perhaps by local residents in the vicinity of the Bay of Islands, for he says: "A small island in this Bay is well named the 'Old Hat'...." (*Ibid.*, p. 110)

PART TWO

Introduction

Part Two is more than an appendix of material supplementing Part One. Although its contents have been drawn on in writing the critical analysis of some of Jutson's contributions of which Part One consists, it yet stands alone, embodying a portrait of the circumstances under which field work was done in the early part of this century written by Jutson; a dialogue between the late Dr. W. R. Browne and the writer; and a bibliography of Jutson's library.

In themselves, these represent primary material which contributes in a special way, it is believed, to the appreciation and study of the past out of which the modern scene has emerged.

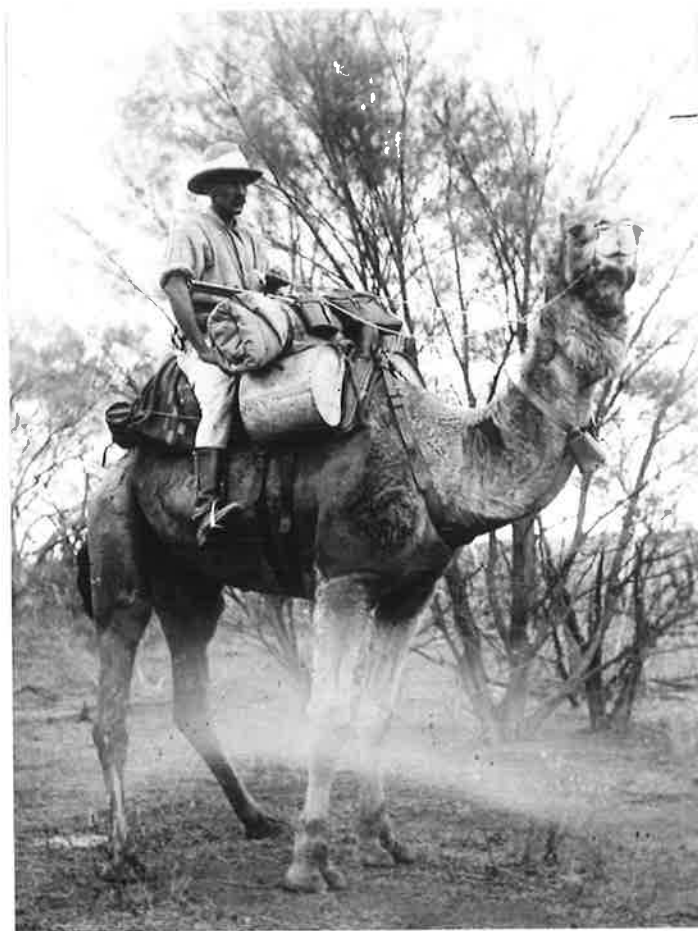


Plate 6

H. W. B. Talbot on field assignment in Western Australia. Reproduced from a post card prepared in the period 1911 - 1918.

Chapter 4

A Portrait of field work 70 miles east of
Kookynie, Western Australia

Preface

E. C. Andrews' statement "...for the history of geological progress in Australia is bound up with a wealth of heroic deeds; with herculean struggles with Nature when in unbending and savage mood;....", was quoted at the outset of this study. And although at the time when Jutson worked, men on field assignments had amenities and freedom from excessive hardships unknown to their explorer forebears for whom the above quotation was written, nonetheless, they experienced trials rarely encountered by modern workers.

The following account of part of one of Jutson's field trips graphically portrays the circumstances in which he and his colleagues worked in Western Australia. Found amongst the Jutson Documents, it was written out in longhand by Jutson's wife Lilian and was apparently included in a letter to a friend, as the final sentence (a postscript) suggests. The account is entitled "Jack's Outback Trip", and is dated March 1915. The "chief" of whom Jutson wrote was A. Gibb Maitland, Government Geologist at that time.

We had spendid [sic] weather, a little warm at times in the daytime, but the nights were perfect, that is in themselves. But they were completely spoiled by the hosts of mosquitoes, & at times sandflies, that appeared. They were so bad, that we always had to retire to bunk immediately it got dark. The Chief has his mosquito net so he was alright. Dave & I however had none. When we could get any cow manure we made a fire, but that was not often & even when we had the fire going there was so little wind that the smoke rose straight up, instead of coming towards us. I used to try putting the blanket or a towel over my head, but this was unbearable in the early part of the evening, as it put me into a vapour bath. For hours, at times I lay awake, waving my handkerchief in a vain endeavor to keep the pests away. I was marked by dozens of bites over the arms & face & if I had suddenly appeared in Perth, I might have been taken for a small pox patient. On a couple

of nights in addition to the mosquitoes, we had the sand flies, my first experience of them. They are only about twice the size of a pin's head, but there were shoals of them & they settled on one & bit just as badly as the mosquitoes. The early morning between dawn & the sunrise was the best time of all, for then we had a triple combination - mosquitoes, sandflies, & ordinary fly which appeared in swarms immediately light appeared. The first 20 minutes I had of this combination I thought I would go silly. In the morning & towards dusk, at breakfast & tea, the flies were very bad, sometimes so bad, that if one wanted to eat the food without swallowing plenty of flies, it was necessary to have one hand quite free so as to wave it over the spoon or fork, as I transferred the food to my mouth. This is absolutely true. Of course we had to camp near water & this provided the mosquitoes.

We carried some butter but it was difficult to keep it decent. The Chief one day got some bagging, wet it & put it round it & placed the billy with the butter under a tree. At lunch time it was served up & was full of ants. Result, exit butter.

We had 2 horses & carried compressed fodder as well as a bag of oats & a bag of bran. There was also good natural feed but in spite of this, the horses at times were in for some rough work. We had to cross & recross a lake & there was much sandy country. The result was that thro' the sand, there was much stopping of the horses & walking on our part, with two of us frequently pushing the wheels to start them & one driving. Dave particularly had a rough time, as he was practically in charge of the horses. One day in the sand, they wouldn't move, so all 3 had to get out & Dave drove them on foot. When they did start they would often go off at a gallop. Dave with the reins had to keep up. It was only a bush track, & as he ran, he had to guide the horses, jump over small shrubs, run round larger ones, holding the reins over the shrubs, & even at times when the vegetation was too close, get at the back of the trap with his arm round the side holding the reins. He would get puffed out & would have to stop them. The Chief & I would then catch up, give the wheels a push & the performance would be re-started. This went on for about 2 miles & finally we came in sight of the lake we had to cross. The horses went over the sand dunes to the brow of the ridge, where Dave missed his footing & went head over heels over a bush. He however hung onto the reins & the horses stopped. After all this toil we found there was too much water in the lake & it was too boggy to be crossed. We had therefore to retrace our steps some miles & then attempted to cut across to another road. 50 yards off the track resulted in a stoppage which nearly caused an unloading of the trap to get it out

of the sand. On again along the old track; at this time the heat being considerable & the horses getting done. They were fearfully thirsty & the sweat was dropping from them onto the road. But we had to go on in order to get water for themselves & ourselves. We thought we would have to go back to the morning's starting point, but we fortunately found some pools of water where we camped. We washed the horses down & gave them a good drink. With a good rest & feed they were alright in the morning.

We that day got on to another road & reached the lake. Here it was only about 60 yards wide with a small backwash. We crossed the main portion safely, but when within 20 feet of the shore at the backwash it became very boggy. The horses floundered & finally Darkie lay right down. (This lying down is really an instinct of self preservation on the horses part. He feels his feet going from him & getting deeper. If he throws himself down, his bulk will prevent him sinking in.) Here was a picnic. All out into the water & the horses had to be unharnessed & taken out of the lake. Then everything had to be carried by us out of the lake to the shore, - bags of fodder, equipment, provisions, boxes, &c. We took off our boots & stockings to do this job, but we had to be careful we didn't fall down in wading thro' the water with the slippery & sticky mud below. Then the trap had to be got out. The horses of course were useless. I was out at the head of the pole with the 2 polestraps, one over each shoulder. I pulled like a cart horse while Dave & the Chief lifted the wheels, but no good. It was about 100 in the shade, but the water made things more pleasant in this way. After much struggling, we cut boughs & branches from the shrubs, placed them under the wheels & gradually lifted the trap out bit by bit. You would have been amused by the chief. He took off everything except his underpants, singlet & helmet. The water was salt & couldn't be drunk by the horses or ourselves & as the country was sandy, along which we had to travel we decided to camp at the lake. The result was that I re-crossed the lake with 2 water bags & a billy to fill with fresh water from the waterholes, about 1 1/2 miles back. This walk was over sandy country with no shade & a strong glare & the bags on the returning were bulky. Later we wanted more & the horses had to have a drink so Dave had to ride one bareback & lead the other. It took about 2 hrs to get the trap out. I took a photo of the scene but unfortunately the camera had got choked with dust & didn't work.

Another experience Dave had was getting in when the trap was going. With one at the wheel & the other two aboard, a start could often be made, but if we stopped, to let the other in, the horses jibbed again. Consequently Dave would give the push & I would drive. Dave would then



Plate 7

"Trap" used on field assignment by J. T. Jutson and colleagues in Western Australia. Locality unknown. On the shore of a "dry" lake after a wet crossing. Photograph by J. T. Jutson in the period 1911 - 1918.

run along, get one foot on the front wheel, keeping it there as it revolved, until he could get into the trap. This was pretty clever as well as being risky. There are no steps to the trap, we always have to get in by the wheel.

Later on we struck 15 miles of sand. The day was hot & there was no shade, nothing higher than 2 feet occurring in the way of vegetation. We got on fairly well for some distance, the Chief driving & Dave & I walking. But the horses, as the afternoon got warmer, began to tire, when we were miles away from water. The journey became interesting, a stoppage about every 100 yards & then about every 20 yards, until Dave & I came up & started the wheels. Sometimes this even was not effective, even with whipping the horses. One of the latter wouldn't move & the other would let fly with both of his hind legs, getting one over the traces at times. If I were at the front wheel I had to keep my weather eye open for this business. At last we found it was useless attempting to go on. So we turned the horses loose & boiled a billy of tea with almost our last water, which we had with some bread & butter. After about an hour's spell we went on again till dusk, but there was the same business & we found it impossible to reach the water for the horses. We therefore turned them loose & put down our bunks & just turned in. Nothing further to eat or drink & of course no water for washing. The next morning, with the horses fresher we got on better, reached the end of the sand, re-crossed the lake safely & obtained good water at about 11 o'clock. We had enough water for a billy of tea before starting in the morning. The horses were without water for nearly 24 hours. After this we had no more trouble. I forgot to say that after leaving the lake where we were bogged, we met another arm. This was corduroyed & Dave, who was driving alone, got safely across with the horses. When the Chief & I came up, we had to take off our boots & socks & wade across it. Throughout our little troubles, the Chief & Dave took everything as a matter of course, each being used to the game. As a recompense we had some good dips in the lake & frequently managed to get a bath from a rock hole.

The country was in splendid condition, feed of all kinds being abundant & grass growing luxuriously in many places. Plenty of water away from the sand. At one place the grass was just like a cornfield, being nearly 3 feet high & in seed. We were about 70 miles east of Kookynie & within about 15 miles of the desert.

Dingoes were bad. At Yerilla they had just killed 2 sheep & injured several others. They were howling in the night & early morning, the Chief & I saw one stealing thro' the long grass. On some of the high rocks we saw some "bung-arrows." These are big lizards, which reach a length of 10

or 12 feet. The largest I saw was between 4 & 5 feet. They are very quiet & just amble away as they are approached. They walk with a very shoggly motion, turning their hind legs outwards as they move. Many are just the [same] colour of the soil & rocks (a reddish brown).

Where we crossed the lake it was very narrow, but another part we visited was about 4 miles wide & many miles long. It was one vast sheet of shallow water, forming a beautiful sight. The water quickly evaporated & for most of the year the lake is quite dry, its surface being as level as a billiard table for miles & glistening white with the film of salt left behind by the water as it evaporated. In this monotonous country these lakes form a most striking & wonderful picture & they are full of fascination to me on account of their beauty, uniqueness & the many scientific problems connected with them.

We saw a fair number of flowers, amongst others being 2 small patches of that most beautiful one - Sturt's Desert Pea.

We once ran out of bread, so Dave made some scones. He omitted the baking powder, so that they were pretty weighty. The Chief complained all the afternoon after eating one of these productions.

P. S. There was lots more of course but it takes such a time to write it. Wouldn't it be nice if we could have a good "cal". Lily.

Chapter 5

Dialogue between the late Dr. W. R. Browne and Mr. E. J. Brock.

Preface

On the morning of July 16, 1975, the writer had the pleasure and privilege of interviewing the late Dr. W. R. Browne, formerly of the Department of Geology and Geophysics at the Sydney University. Dr. Browne died two months later, early in September.

At the time of the interview, Dr. Browne was ninety years of age and in excellent health. Gracious in manner and spirited in speech, he gave the writer many valued insights into aspects of Australian geology and geomorphology through his recollections of men who have contributed to the establishment and growth of these sciences in Australia.

Although the immediate value of the dialogue is to the writer, others may benefit from it. And for this reason, a transcription of it follows this preface.

After making a verbatim transcription of the tape, the writer edited it to improve the flow and clarity of thought. All changes have been placed in square brackets, together with a few editorial comments. The addition of the latter will be evident from the context in which they are placed.

Following the suggestion of Dr. D. F. Branagan of the Department of Geology and Geophysics at the Sydney University, a copy of the taped dialogue has been placed in the Archives.

It is noted in conclusion, that Dr. Browne's wife, Dr. Ida A. Browne, died on October 21, 1976, her passing being recorded in The Australian Geologist (Newsletter No. 12, December 1976).

Dialogue transcribed

EJB Let me start by saying that during last year I was wanting to get some biographical background to Jutson, and [to this end] I met his niece who lives in Caulfield, Melbourne, [and in whose] home was [the remainder] of his library. Subsequent to that I encountered a short obituary to him by John Knight published in the Mining Journal of the Victorian Mines Department. In that article there was reference to Jutson's correspondence and professional papers that he had bequeathed to the Victorian Mines Department. So last

August I went there and [was offered for examination a trunk, full] of his lifetime's correspondence and papers, unpublished [articles] as well as his notes on his reading, [organized by Jutson]. It was an historian's delight to find this sort of thing. He corresponded very widely with men in Australia and overseas; with William Morris Davis, Kirk Bryan, Lester King, Edmund Teale; (Sir Edmund Teale); did you know of him [Teale]? An Englishman I think he was.

WRB No. He was Victorian. His name was Thiele, and during the First World War he changed it to Teale. He wrote on Victorian geology.

EJB Well, the correspondence was from England and I had assumed that he was an Englishman. Jutson worked in the Survey up until 1918 [when] he was retrenched [for] financial reasons; and at the same time a number of the men he was working with left the Survey.

WRB Oh yes, I remember that.

EJB There was Honman, Feltman, Blatchford, Gibb Maitland [who] was the Chief at the time, and deCourcy Clarke. Well, subsequent to that time Jutson corresponded with a number of these people throughout the rest of his life, not merely personal material, [for] they discussed ideas and problems. It was through this material that I began to get a feeling for the problems these men thought about and worked on, and [I want] to speak with people like yourself who knew men of this generation, and I'd like to get [a feeling] for that time from people like yourself. I'm concentrating on the geomorphological work and only on geological problems in so far as they have a bearing on physiographical [geomorphological] questions. Amongst Jutson's work there was correspondence with Andrews and Sussmilch; now Sussmilch was a geographer...excuse me...both of them I think came out of the Geological Survey, the New South Wales.

WRB Who?

EJB Sussmilch and Andrews.

WRB No. Sussmilch never was [in the Survey].

EJB Andrews [was]. I see.

WRB Sussmilch was in the Technical College. [He] was a lecturer there, and afterwards in charge of the Newcastle Technical College.

(WRB inspects letter from Sussmilch to Jutson)

EJB Now I must confess to a certain amount of ignorance with respect to your own life. You were not in the Survey at all; you were in the Sydney University. I remember reading your preface to the Geology of the Commonwealth of Australia. You made some remarks about physiography, and I gather that geologists in Australia were not disposed to think in physiographic or geomorphological terms.

WRB Well, that's a generalization. The professional geologists were all in the Geological Survey and their work was strictly geological, and any physiographical work they did was incidental. Andrews was the only one who had any pretensions to be interested in geomorphology. David was of course, incidental to his geology. Yes, Andrews was the only one, and Sussmilch was interested in it too; he [Sussmilch] was interested in geology and he became interested in geomorphology. But [he] never had the experience that Andrews had. He did one paper on the southern islands, and he did short articles about geomorphological matters. But Andrews was the outstanding man, and he was I think, the only man who really was interested in geomorphology as such. I think it would be correct to say that Benson was interested to some extent. He was a geologist [and] he used to be interested to some extent from the geological aspect. As a matter of fact geomorphology has been taken up almost entirely from the geological side until comparatively recently.

EJB What do you mean there?

WRB Has been taken up by geologists; you might call it [geomorphology] the superficial part of geology. [Stated with a touch of good humour].

EJB That's probably true until the post war era [which] has seen a blossoming of Geomorphology.

WRB Well you see any geologist who thinks in three dimensions, as he has to, must be impressed with the relation between the landscape and the rocks which compose them - he can't get away from that - and that means that he starts from the rocks upwards. The geomorphologist as such is not so interested in the rocks as such, and sometimes he slips on things for that reason.

EJB It's certainly true in Jutson's case. His physiography in his early years was [done in his] leisure-time, even his first Bulletin No. 61 [of the Geological Survey] - The Physiography of Western Australia. In the preface to that his Chief points out that this Bulletin represents the work of leisure time. Now if you go back to Jutson's field notebooks, there is quite a clear distinction made in his

own thinking about physiographic matters and geological ones, and his field notes pause in discussing geological matters, and he will say "now turning to the physiography" this is instructive for one reason or another, and then there will be a series of notes and comments that he makes directly in the field about problems he recognizes there. Of course Griffith Taylor in his early years thought [in physiographic terms] when he was - going back to about 1910 - working on the Canberra physiography.

WRB [H]e's dead now; he was a good friend of mine. But I always thought his work was altogether too superficial. Nothing much was known about the detailed geology of a lot of the country, and he looked at it and interpreted it, that's all he could do. And the easiest way to interpret a good deal of the geology or physiography, is by faulting; and a lot of his stuff was wrong in detail. And sometimes he had a habit of saying things dogmatically which were not capable of proof. He didn't have time to enter into the proof of things that he said. Mind you, he did good work in giving ideas to other people [in] his teaching, but he was never a physiographer, and actually in his last book Sydneyside Scene, that undoes a good deal of what he had done before. He wrote that in his old age (I suppose he was in his seventies), and in the meantime a number of his students and other people had gone over some of the ground that he had interpreted and put other interpretations on it, and he, in this book accepted those new interpretations and [thereby] undermined his own work.

EJB He was a prolific writer and producer of ideas, I gather.

WRB He was; he was too prolific. Mind you, he was a very inspiring teacher, and his students swore by him. I knew him very well (he was a demonstrator in my first year), and later on he was a colleague here. We had many fights, friendly fights, and on one occasion I told him that what was new in his paper wasn't true, and what was true wasn't new [spoken with good humour]. But he was always good-humoured over that sort of thing, and he didn't pretend to be anything but a pioneer.

EJB That's an interesting point [your] remark that he considered himself in a pioneering role. Now I suppose that if you [go] back to the latter part of the last century and the early part of this one, I guess all men regarded them[selves] as pioneers; I mean in geology. Australian geology and geological activity is in a sense very recent, isn't it? Selwyn [was] in Victoria about 1850, 1855.

WRB It grew gradually in New South Wales.

EJB Tate was active in the late 1880's and 1890's.

WRB Yes, [and] David came out here in the early 1880's, 1882 I think it was, or 1884. The Survey, which was under the Mines Department, was concerned mainly with economic matters, and it was under what was called the Geological Surveyor-in-Charge, Wilkinson, then afterwards Pittman succeeded him, and he was Government Geologist, and since then the chief man has been known as the Government Geologist.

EJB Is that still true today?

WRB I believe now it is called the Director of the Geological Survey. But the Geological Survey is a section of the Department of Mines.

EJB Excuse me, is the sun on the back of your neck there?

WRB No. Thank you.

EJB So in the latter part of the last century the surveys were active under Government Geologists. Now the universities were very much in the infant stages, wouldn't they have been. Woolnough was Foundation Professor in Perth in 1912 or 1913; Tate must have been first Professor of Natural History, I think it was, at Adelaide, and that would have been in the 1880's.

WRB He was succeeded by the one man whom he despised; Howchin. He despised Howchin as an amateur. Of course Howchin was an amateur. When Tate retired they established two independent lectureships, one in stratigraphy and paleontology (that was Howchin) and Mawson, mineralogy and petrology. They were quite independent. Howchin, I think was a friend of the Chancellor.

EJB He was a minister, wasn't he?

WRB He had a peculiar career. He was born in the Fens of England where there was no geology, apparently; and he entered the ministry in a sort of way, and he did a lot of work in some of the industrial centers, cities, in England; and he got a liking for geology, particularly for glacial geology, and his health gave way (I think it was TB that was the matter with him) and he came out here. And his friends in farewelling him said what a calamity it was that his career should be cut so short; they had evidently written him off, as people did in those days with TB, and he came out here and he had to be carried ashore at Adelaide. [But] he postponed his departure from this world for a while! and the Methodist connection in South Australia got him something to do; and among other things,

he did occasional preaching in the country, Sunday work, and he always made a stipulation that when he was out at a place, somebody should drive him around the country on the Monday, and in that way he got a lot of notes which he carefully kept on the geology of various parts of the inhabited country in South Australia. And eventually on Tate's retirement or death, he became one of the two lecturers in Adelaide, and I've always thought Sir Samuel Wade who was the Chancellor had something to do with his appointment. Anyhow, it was rather ironical that the man whom Tate despised should succeed him. Then later on...

EJB But Woolnough had a lectureship in mineralogy and petrology.

WRB Oh yes, I should have stated that there was in an interregnum between Tate...a division into two lectureships (sic). Woolnough went over there in 1905 (the early 1900's anyhow), as lecturer, and he established the lectureship in mineralogy and petrology; and he came back to Sydney at the invitation of David, and was succeeded by Mawson.

EJB Right. And then he went to Perth.

WRB Then he went to Perth. He was in Perth for seven years from 1913 to '20, and he got fed up with Perth. There was a panic among the [?] Perth started off bravely, and then there was a panic among the older professors because appointments were only for seven years, and Western Australia had fallen on unfortunate times, apparently, in the meantime, and there were rumours that the number of full chairs was going to be drastically reduced at the end of seven years. And Woolnough took fright, and he entered the service of Brunner Mond looking for alkalis. He went all over Australia looking for salt and other deposits without success.

MAID Excuse me, would you like a cup of tea?

WRB Would you like a cup of tea?

EJB That would be very nice thank you. Yes, thank you.

WRB Then, when he had looked all over Australia in vain, Brunner had no more further use for him, and he happened to meet a friend who was in the government. He was [then] appointed a kind of Commonwealth Government Geologist for special purposes. I don't remember if it was for gold or for iron ore, or something [else]; but anyhow, he took everything. Everything was in his province, oil and gold and various minerals; and he entered the service. For him it was a very exciting [role] for he was over forty-five, so he couldn't qualify for a pension. And when it was time for him to retire there was

nothing left for him to do. He was an extraordinary linguist; he knew fourteen languages. He wasn't proficient in speaking them, but he had learned them from grammars and so on, and for some time he acted as a kind of interpreter in government departments when they wanted a translator. But the poor man, his eyesight failed, and his legs failed. He got something or other in his early days in Fiji about 1905 and his end was rather miserable.

EJB Yes, I heard this from Prider.

WRB Who?

EJB Rex Prider mentioned [it].

WRB Yes, he wasn't altogether a success in the way of science.

EJB Is that so? Yesterday I was at Canberra, and the National Library there has two boxes of his correspondence and some diaries. And interestingly enough, there was one envelope labeled "Translation of Poetry," and I wondered what on earth he would be doing translating poetry. But your comment a moment ago about his linguistic abilities explains that.

WRB He knew a lot of the European languages, and he did a lot of translation.

EJB Edgeworth David had a very high regard for his early promise and academic ability.

WRB Yes, his temperament was always against him. It was a terrible pity because those of us ([a] small number of us) who [went] right through the course, and had been out with him in excursions and one thing and another, got a great affection for him. But he used to do silly things and antagonize people. And I understand that he antagonized people over in the West. I've heard it second hand so I don't really know much about it, and he [got] prejudices and so forth. But he was a fine man.

EJB Did you know deCourcy Clarke at all?

WRB Yes. A very nice fellow.

EJB Well, I've got that impression from his correspondence with Jutson. They were great friends, and wrote quite intimately to one another about life in general, and their reactions to life; of course, they were together as field geologists in the Survey.

WRB Jutson was a lawyer.

- EJB Yes, that's right. He had a most peculiar career in many respects, and I still don't know where he developed his interest in physiography. But I came across a note book very carefully written; it was a diary of a trip he took to Mt. Bufallo. Now this was [in] 1900, and the book was labeled number six, notebook number six. Now this was prior to the time when he had any formal education in geology at all, and yet there he is [showing clear evidence of interest in physiography]. He had [drawn] topographic sketches and so on. There was this early evidence of interest in physiography, in a sense more than [in] geology. Now at that time he was twenty-six, and his parents apparently wanted him to go into law, and he had a position then as a law clerk. Well then, in the early 1900's he took courses at the Working Mens' College in Melbourne, and at that time Gregory, J. W. Gregory, from Scotland, was teaching there. [This is in error. Gregory, a Londoner, was at this time Professor of Geology at the University of Melbourne].
- WRB Excuse me. Incidentally, he was rather like Griff Taylor.
- EBJ Who was?
- WRB J. W. Gregory.
- EJB In what way? In appearance?
- WRB No, in [other respects].
- EJB Yes, that's right. Now that's the impression I get from his writing because he wrote a lot of books on all sorts of subjects.
- WRB Well, you know Skeats?
- EJB Yes.
- WRB Skeats told me that he spent the first five years, I think it was, of his occupancy of the chair in Melbourne in correcting the mistakes of Gregory.
- EBJ I haven't read widely in Gregory. I remember looking up his book on Australasia, for example, and he made there a comment about the boundaries of the States in Australia, and he said that they are all natural boundaries, which is way off the beat; and I thought that, well, if a man makes mistakes of that kind, I just wonder to what extent...
- WRB Well now, so far as South Australia was concerned that was the case, that was correct; but I happen to know something about that. It's not a natural boundary now between South Australia and New South Wales; it's a quarter of a mile out.

EJB But which boundaries would you call natural?

WRB He probably meant determined by mountain chains and watersheds. But a number of them were determined astronomically (meridians) and that sort of thing. And that's the case between South Australia and New South Wales, and South Australia and Victoria. But if you looked at a map (I don't know if you've ever noticed it), if you looked at a map of Australia you would find that there is an off-set, as if there was a fault on the vertical boundary between New South Wales and South Australia. I happened to be mixed up in that.

EJB What was the cause of this?

WRB Well, the country wasn't well known, and the trouble used to be that when a wrongdoer was being chased by the police, he went just to this no-man's-land, and nobody could prove anything against him; he could say he wasn't there, he was in Victoria, and I suppose it was the same way with Victorian criminals. And so, the States at last (well, the same [applied] to New South Wales) decided to try and amend the position, and in New South Wales they did it astronomically by mutual consent, and that showed that the one they had against Victoria was a rather dog-leg sort of thing (for purposes of convenience they determined the position more or less, I think, on the coast), and the surveyors ran a rapid north-south line, and when they came to a [patch] of timber, they would go around that, they couldn't [get through it], they would go around and take it up [?]. The consequence was that it was very unsatisfactory, and in about 1910... yes...there was some trouble between Victoria and South Australia,...

MAID Excuse me, Dr. Browne, you are wanted on the 'phone.

WRB I've been talking partly about Howchin [on the 'phone]. Well, where were we?... Oh yes, about the boundary between South Australia and Victoria. South Australia went to law about it, and the High Court under - a Jewish name - Sir Samuel...No it wasn't Griffith...yes it was Griffith, Samuel Griffith, decided against South Australia. It was found that the actual boundary that they were using was a good way away from the meridian; and it was a question whether they should use the de facto boundary, or the de jure one. And they sent the Government Astronomer (I was an astronomer at the time, Assistant Government Astronomer), they sent him over, the South Australian Government sent him over to a place on the Murray where the spot had been determined from New South Wales and Victoria; it was marked by an obelisk; and I remember I had to look after the observatory in his absence. I was coming away at one o'clock in the morning and a police-

man saw me coming, and he tackled me...

EJB Apprehended you, did he?

WRB Yes [with mirth].

EJB I've got here a couple of prints from Jutson's negatives, and that's Jutson. Did you ever meet him?

WRB Yes, I was going to say I met him once when the ANZAAS was over in Western Australia; I forget when it was. It's the only time when I've been in Western Australia, and I met him. He was deaf.

EJB Yes, that's right. How deaf was he?

WRB I think he had to wear a gadget, an instrument.

EJB We were talking earlier about his career, and that's when we got on to Gregory, wasn't it? Well, I've been trying to find who it was who may have stimulated his activity in physiography. Now it was prior to his time when he did encounter Gregory, and later Skeats, when he was a research student under Skeats. But...

WRB I think he was largely self taught.

EJB Yes, well I think that must have been the case. After leaving the Survey in Western Australia, he completed a BSc in Western Australia, at the University; but at that time he was granted exemption from math classes because of his deafness. And then he returned to Melbourne and took a law degree at the Melbourne University which he completed in 1925. He was fifty-one at the time (must have been a very active man, continually reading and studying). That's his wife [showing photo].

WRB Mind you, that Physiography of Western Australia that he wrote, as a Bulletin of the Survey, had a fair bit of speculation in it. I don't think he saw everything.

EJB No, I think that's true. [But] I haven't traced out the geography of his field work.

WRB But he was one of the men who stimulated interest in physiography in Western Australia. I think he was about the first so far as I can remember, but I haven't gone into the matter.

EJB You don't happen to know who this is, do you? [showing photograph] I think it's one of the local people that he met when out in the field around Kalgoorlie.

WRB No, I don't know him.

EJB I call him the "Mississippi Gambler."

WRB Well possibly you know more about that than I do!

EJB Did you ever meet Walther?

WRB Walther? He was out here in 1914 with the British Association. I don't remember him distinctly. I remember [Albrecht] Penck, he was here; and made a nuisance of himself.

EJB In what way?

WRB Well, you see the First World War started while the European delegation was on the water, and it was decided here in Australia to treat them just like ordinary people; they weren't interned as soon as they got here or anything like that. But they had it pointed out to them that they would have to behave, and he was found taking photographs of Fort [Scratchly] up at Newcastle. Fort [Scratchly], I think, had no military significance whatsoever. At all events, there it was. It was allegedly [of] military [significance]; he may have done it quite innocently.

EJB It was unwise to do it, wasn't it?

WRB Yes. Professor David, I think, had a lot of trouble with him, in a way.

EJB How come?

WRB Because of course anything German was terrible. And we had an excursion [in the field] up to Maitland, Newcastle, and the Maitland people gave a [shivoo?] for the party on Saturday night. Professor David was of course a patron saint for the people of Maitland, [for he] discovered the seam traced to [?], and actually there is a monument to him up there [?]. And a number of the foreigners were here, English and foreigners, and they had to speak. The [?] were introduced by the Professor, Professor David, and they said a few words of an amiable tone, then sat down. And he left Albrecht, Penck, to the last. And when he was introducing him [you could see him] looking to the ceiling as if he was seeking inspiration from on high, and he couldn't find words to describe the perfection of the man Penck and the wonderful work that he had done, and so on. The consequence was that when he [Penck] had got down to speak, he got a tremendous welcome, the biggest welcome of the lot. But he was [cuffed] in London, and they kept him there in London for awhile, and then let him go back to Germany.

- EJB Well that's interesting. When Walther was on the way home, he was jailed for two days in Suez. He wrote of it in a letter to Jutson. It was quite an entertaining letter, explaining how he had been apprehended and jailed in Suez for two days before he was let go on his trip.
- WRB Yes, well Penck was very foolish in taking photographs.
- EJB Yes, I can see that.
- WRB And I think it was supposed to be bad policy on the part of the British authorities to let him go back to Germany.
- EJB Dr. Browne, I have some friends, two former students actually, whom I taught in America and who are now living here, who drove me here this morning, and I arranged with them to see them at half past eleven out the front to see how things were going. Now I wonder if you would be happy for me to stay another half an hour or so?
- WRB Yes, yes, I'm enjoying this tremendously, if you'll excuse me saying so.
- EJB Well look. I'll just pop outside and speak to them. In the meantime you might be interested to see these letters from Davis to Jutson. They are very entertaining. There's a delightful part here about labourers and so on. I'll just pop outside. I won't be a minute. [End side one of tape]
- [Start side 2 of tape]
- WRB I met him out here [Davis, W. M.] Andrews brought him up to the University.
- EJB Yes, in a letter here [showing letter of Andrews to Jutson] ...Andrews had met Gilbert too.
- WRB Oh yes, Gilbert was almost a god to him. He was a distinct hero worshipper of Gilbert.
- EJB Well, Gilbert was certainly a renowned fellow, wasn't he.
- WRB Oh yes, I can remember his writing [viewing Andrews' letter]
- EJB Can you read that? They're talking about desert erosion. "...the efficacy of arid erosion," and here he says "I had it out face to face with G. K. Gilbert in Colorado and with Davis in Australia also against Walther in Melbourne." Oh I see; Walther was a great advocate of arid erosion, it seems.
- WRB I was with Andrews at Broken Hill.

EJB Oh I see. In what capacity?

WRB He wanted a petrologist, and there was no petrologist on the Survey except Card. He was a very good man in his line, but he was rather too old to be going [around]. So he appealed to the University, Andrews did, and I was up there for three long vacations, '18, '19, and '20, supposed to be [doing petrography]; and so I saw a good deal of Andrews then. Of course I used to see a lot of him in Sydney too.

EJB Was Andrews in Broken Hill at the time?

WRB Andrews was in Broken Hill part of the time, and most of the time that I was there. It was while he was doing it. He started doing an impossible task himself. The Mining Managers Association just asked for somebody to come up. It was supposed to be an expiring field, and he was supposed to perform the [?]; and he gave it twenty years more life, and it's still alive. And he was just up there as a geological surveyor, and I suppose the Government paid for his hotel expenses. But he had no means of locomotion except his two legs. And he used to find himself at dinner time (at the hotel) about eight miles out, and [would have] to walk back. And it's a pretty awful place; it used to be anyhow. And then the Mining Managers Association came to the rescue, and they provided 1500 pounds, and I was there for...I suppose it was eight or nine months altogether, three long vacations, and they gave me a gratuity of 100 pounds. Anyhow, Andrews was able to get a motor car to take him around, and he was able to hire a place, a disused shop, for an office, and he was able to get other things as well. But he made a good work of that. But unfortunately...have you seen his book?

EJB No. Quite frankly I've not looked closely at Andrews' work at all, but I am just wondering whether I ought to, from things that you have said. I've just noticed in passing some titles of his papers, and some are distinctly physiographic. But I wonder. Did Davisian thought dominate?

WRB Oh yes, he was a disciple of Davis.

EJB Well that seems to be true of most early Australian geologists, as I see it.

WRB There's a strange thing. Although the earliest physiographers were geologists, they don't make a great deal of use of the geology. I've always thought that geology was fundamental. And I remember when Davis was out here, I remember distinctly, I could show you the spot where he was when he said it,... [remark of Davis lost here because the tape came to an end. The remark was to the effect that, in Browne's view, Davis

gave less importance to geology than Browne would have thought appropriate].

- EJB Davis must have been an interesting man. Are you familiar with a two volume series that's been published recently on the history of the study of landforms? Chorley [and others] have written two large volumes tracing the history of geomorphological thought from Hutton right through the last century through the various controversies in England about the role of the various agents in landscape evolution. The first volume they take up to Powell and Gilbert, with a final section on Davis. Well now, the new volume is completely devoted to Davis. It's very difficult to get (I can't get my hands on a copy). But in the last thirty years or so, of course, Davisian thinking has come under a lot of attack from geomorphologists, and I think in many respects rightly so. In some of these letters he bemoans the fact that he didn't attract more students, and he has also said in here and in other places that one of the reasons for [his] constructing the cycle of erosion was, in a sense, as a means of...as a pedagogic device, as a frame work, you see, in which to convey his geomorphological ideas. But from other sources, I gather, he was very intolerant of other people's ideas. What impression of him did you get of the man?
- WRB Oh well, I'm sorry. I'm afraid I'm sailing under false colours. I'm not a geomorphologist.
- EJB No, but still, just as to the personality of the man.
- WRB Well, I didn't see much of him here. I think the impression that I gathered was that he considered himself an apostle of geomorphology. Of course, you have to do that if you want to get on, if you can put it on over other people.
- EJB Yes, that's an interesting remark. The other day I was talking to my supervisor, Dr. Twidale, about the question of truth in geomorphology, and I had given a seminar that involved us somewhat in questions, in philosophic questions about the nature of things, and the extent to which we can feel assured that we have arrived at some semblance of the truth, whatever [is being considered], whether it be a geological or other matter of scientific interest. Well now, Rowl's position, Rowl Twidale's position was [that] in a sense you never know when in fact you have arrived at the truth of the matter, that you strive for the best approximation on the basis of the evidence you've got. But in addition he remarked that it depends a lot on the character of the person, to the extent to which he projects, I suppose, his ideas and can force it on the scientific community. How do you react to this? A remark you made a moment ago suggested thinking on somewhat the same lines.

- WRB Well, I think there's a good deal in it. Although I haven't really made a general study of the matter. But to take Griffith Taylor as an example, he had the power to inspire people with his teaching, and yet if you analyse his stuff, in many respects it was all wrong, and he didn't take enough pains to verify what he said. And the effect was that he turned a number of people to the study of geomorphology, but they haven't followed him. Of course, any good teacher does that; they don't necessarily follow their teacher; some of them will. I've just written a biographical notice, a short biographical notice, of a young fellow whom I knew, Craft. He flourished about 1930, and unfortunately his health gave way, and various other accidents happened. He died eventually; he was run over by a motor car in Canberra in the sixties. Yes, it was very unfortunate. Well, he was one of Taylor's students, one of the last of Taylor's students; and it's noticeable in his works. He only wrote about six or seven papers. I'll give you a reprint. He graduated from geography. He hadn't done much geology. In those years, a first year of geology was a requisite for the geomorphology, geography, and he got a MacLeay Fellowship off the Linnean Society, N.S.W. [He] did all his work, practically all his work, on that [over] the next five years, and unfortunately he didn't get his DSc, which must have been a terrific blow to him because he so thoroughly deserved it.
- EJB There was some personality problem, some clash there.
- WRB Oh, there was a variety of circumstances. He was a very positive sort of fellow, and he rather scorned, or gave the impression that he scorned anybody who didn't have the knowledge that he had, and he rather antagonized his...I don't like this going on record.
- EJB I'll stop it if you like.
- WRB He rather antagonized his supervisor, Head of the Department, and he finished his Fellowship, - there was a limit to it, five years - in the middle of the Great Depression, and there was nothing for him to do. He had been in the Education Department, and he had got through the Teachers' College. So he had to return to teaching, and he taught for the rest of his life, taught for about, I suppose for about twenty years, or in that order. Well now, in the six or seven papers that he wrote as a MacLeay Fellow on the physiography of Eastern Australia, of New South Wales really, and various parts of it, you can see that he adhered to the Taylorian view that there were faults (faults to account for differences), and in his later papers that he has discarded that. It was the same way with John Anderson that philosopher; he produced a number of people and they thought the world of him, but none of them took his line at all.

- EBJ Well it's perfectly natural I suppose for students in their earlier years to be, to show [rather] a strong allegiance to their teacher and his ideas. I guess that's natural.
- WRB Yes, but there are people who grind out the same sort of stuff [as] technologists rather than research workers, and there are others who do thinking for themselves, and who are urged to branch out. Well, Taylor was able to put out some very good people; Stanley, who worked up on the Queensland coast and afterwards went into oil, graduated in geography and geology, and there were others. Taylor had quite an influence on his students but they didn't necessarily follow him along the same lines.
- EJB There's just one other thing I'd like to get back to. Whilst I was in Perth I went through the Annual Reports of the Survey to get an idea who the personnel were that worked during the 'teens, and at about the time when Jutson came on the scene deCourcy Clarke entered the Survey. There was Maitland as the Chief, and Honman and Feldtmann that you may or may not know; and Blatchford was there, E. S. Simpson [also], mineralogist I think he was. Now I [have gained] the impression that there was a strong esprit de corps with them, and that there was a high mutual regard for one another. I imagine that to some extent that was a great stimulus to the work that they all did; then in 1918...
- WRB Farquhar was there. He was the petrologist.
- EJB Petrologist, that's right. Farquharson, was it? Farquharson?
- WRB Farquharson, that was it; yes, right.
- EJB Well then, in 1918, between 1918 and 1919, that whole team dispersed. Jutson was retrenched; one of them died (I think it was Talbot, H. W. B. Talbot); deCourcy Clarke went to the University; Honman and Feldtmann were the only two that remained there. Now, I just wonder if you feel that it's a correct assessment to say that there was an activity there in Western Australia during that time that later fell off. Now you referred a little while back to the possibility of the loss of chairs at the University; I wonder if there was a general depression of activity.
- WRB No, I don't know whether the things were connected; [although] I think it's quite possible that they were. But I know round about 1912, I think it was, there was a sudden activity in geological circles in Western Australia, and the Government established a Geological Survey. They'd had people before then, but I don't think they had a properly organized Survey; and they put on quite a lot including a special petrologist,

Farquharson, which was a sign that they were going to do things in detail. And then, that went on for some time like the Victorian Survey originally, and then it collapsed they had to retrench. I don't know whether the State fell on hard times, or what it was. But it was something like that and I think it was something, possibly of the same sort in the University.

EJB It wasn't an Australia-wide phenomenon?

WRB I don't think it was an Australia-wide phenomenon. I wouldn't be sure of that, but I don't think it was. I know there used to be a great deal of talk about it here in geological circles, [about] the action of the Western Australian Government in establishing this fine Survey and then suddenly breaking [down].

EJB You get the impression of it now that it's a very prosperous and active place.

WRB Yes, well I think that the cause was economic.

EJB But certainly Lord and Trendall, the Director and Deputy Director, are very [active] and able men, I believe.

WRB Oh, Trendall, is he Head of the Survey now?

EJB He's Deputy Head. He was most interested in [the] type of work that I'm doing, and he would really like to see somebody interview people like yourself that still can keep us in touch with the early days of development in this country. Now, on the 'phone you suggested that there were others, "others better than yourself" you said, to see. I just wonder who you had in mind.

WRB Yes, well there are two people, both at the University. One of them I spoke to when you first mentioned coming over here, and he said he'd be quite willing to see you. I didn't know what aspects, of course, that you wanted to deal with particularly; but they have both gone into the matter of the early history of the geology here in New South Wales and in other states too, principally Victoria I think, because it's more easily accessible. One of them is Professor Vallance, T. G. Vallance, and the other is Professor Branagan, D. F. Branagan. Do you know him?

EJB No. Trendall mentioned him to me. Are they both in the Geology Department?

WRB Both in the Geology Department. Both good men.

- EJB I had in mind to go there this afternoon, to the University, to see these fellows.
- WRB Well it would be a good thing to ring up to see if they are available. I've got out of touch with the University very largely. But I don't suppose this is vacation time?
- EJB Well, no, I don't think it is. It's certainly not in Adelaide, and...
- WRB ...of course, there's the possibility of practical classes.
- EJB How long is it since you were active in the University?
- WRB I retired twenty-five years ago, in 1950. I started as a junior demonstrator in 1911.
- EJB Oh, go on. That's a big span of years.
- WRB Oh, I celebrated my ninetieth birthday about six months ago. Yes, well now, I was just wondering if I could give you some references. You want to get away, do you?
- EJB No. I spoke to my friends, and they're going to return at twelve thirty. So there's plenty of time. I'd be glad to have this biographical sketch on Craft, that you mentioned a moment ago.
- WRB Unfortunately, I have not been able to sort out my reprints properly, and my room is in rather a messy condition. It fell to my lot on various occasions to write biographical notices of people, Professor David, Woolnough, Taylor, and a number - Benson - and a number of others. But unfortunately I don't know where my reprints are.
- EJB Yes, I saw one you wrote on David in the Australian Geographer is that right?
- WRB Did I?
- EJB That's 1934. When did David die?
- WRB 1934.
- EJB Oh well, that was him. I had been reading your article, your notes on lakes of Australia.
- WRB On what?
- EJB On the lakes of Australia?

WRB Oh, that was a long time ago.

EJB Yes, 1934. I was interested in that because Jutson was all tied up with the origin of the lakes in Western Australia.

WRB Well, if I can get hold of those things I'll send some along to you. I wrote one on Andrews, for example.

EJB Oh really! Yes, I'd be delighted.

WRB But I haven't a ghost of an idea where it is. It's somewhere in that room, but that's the best I can say. And there's Taylor; I think I can let you have a copy of Taylor's. I'll see what I can do now...

EJB Good. Thank you very much, yes...

The End.

Chapter 6

Bibliography of John Thomas Jutson's personal library.

Preface

In the Prologue to this treatise the circumstances surrounding the bequest, indexing, and dispersal of Jutson's library were described. The following bibliography was prepared from the original card index housed in the Department of Minerals and Energy, Victoria.

This original index contained many deficiencies. Books were frequently indexed by abbreviated title or author only, lacking information regarding publisher, year of publication and edition. Employing the National Union Catalogue (Library of Congress Catalogues), the references were corrected. In addition, Jutson's reprints of journal articles that comprise the bulk of the bibliography were individually checked by consulting the original journals in which they appeared, employing the extensive holdings of the libraries of The Academy of Natural Sciences and The University of Pennsylvania in Philadelphia. All but about 140 articles and books (indicated by an asterisk in the bibliography) were so corrected, the failure to complete the work being due either to inaccessibility of journals in local Philadelphia libraries, or to the total inadequacy of the original index card notations of the works.

The value of this collection is twofold. In the first place

it provided a source of reference material for this study. But as well it can serve as a tool for further studies in the history of the geological sciences, both in Australia and elsewhere.

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EPILOGUE

Theories are meaningful independent of observations;
 observational statements are not meaningful unless
 they have been connected with theories.

P. K. Feyerabend, 1965

In the foregoing, an attempt has been made to portray aspects of John Thomas Jutson's life and work. In assessing his place in the history of geomorphology the writer is inclined to place him on an independent line rather than in the mainstream of thought and activity. Prior to 1900 (when he was 26 years of age) he was, it seems, self taught and yet had established a habit of making careful notes on field trips in Victoria. To this time the works of "the great old English masters" - as Jutson described A. C. Ramsey and Sir Archibald Geikie in a letter to W. M. Davis - are likely to have been his source of knowledge and inspiration. Later, W. M. Davis with his "New Physiography" as E. C. Andrews called it, exerted strong influence as did the direct contact with Johannes Walther in arid realms, and W. D. Johnson in regard to coastal geomorphology. But as has been shown, Jutson was not content to interpret his observations merely within the established (though young) paradigm. At times he had either to step outside of it and lay a path leading to a new one, or else modify the established one to give meaning to his observations.

But if Jutson's road was an independent one, who journeyed on others? From the leads given by the late Dr. W. R. Browne (see Chap. 5 above) the writer is inclined to agree with the statement of Vallance and Branagan (1968, p. 276) that "the whole subject of the scientific study of landscape was one in which David [Sir Edgeworth] played a

founder's role in this country [Australia]. His influence may be seen in the physiographic studies of E. C. Andrews, W. R. Browne, C. A. Sussmilch and Griffith Taylor, who had been members of the Sydney school." Other students of David made contributions to geomorphology, albeit secondarily to their main interests as with those mentioned above, such as Sir Douglas Mawson, W. N. Benson and W. G. Woolnough. Still others, independent of David's influence and from all the states of the Commonwealth, contributed to the growing body of knowledge on the geomorphology of Australia.

In conclusion, we turn again to the Jutson Documents for insights into the nature of the man upon whom this treatise has focussed. In so far that one singles out from literature those passages that seem to embody basic truths, at the same time such passages reveal the character of the selector. And in bringing this study to a close, the writer places before the reader some extracts selected by Jutson and jotted down encapsulating his philosophy of life and revealing the mainspring of his life-long interest in geomorphology and related fields.

The first are from a group entitled by Jutson "A. V. Dicey's Life by R. S. Rait" [probably extracts from A. V. Dicey, Memorials; being chiefly letters and diaries, R. S. Rait, ed., Macmillan, London, 1925; MS 370(2)].

There is something very odd in the looking back on one's own life: I discover mistake after mistake, but I also discover that from one's physical, moral and intellectual nature, each mistake was almost inevitable. This is the true lesson to be drawn from the criticism of the men who have lived before one. (p. 285)

And quoting a dictum of William James:

One must learn to be content with everything, even
with oneself. (p. 209)

The next selection is from page xxv of the "Autobiographical
Foreword" to Impressions of Great Naturalists by H. F. Osborn (Scri-
bner, New York & London, 1924)[MSS 366 (2-3)].

From these impressions of the lives of many natural-
ists we see that the naturalist is animated first of all
by the joy of observation, without initial hope or
thought of discovery, but surely in the end leading to
discovery; leading also to creative thought if observa-
tion is pursued with a single eye and unfaltering purpose,
regardless of all obstacles or dangers and of the
greatest impediment of all, namely, interest in self and
in self-advancement.

And finally, what is perhaps most revealing of all, a poem by
Margaret Lee Ashley published in The Australasian on April 22nd,
1905, entitled "A Byway." [MS 217]

The highway marches sturdily, to Market-town and Mill,
But I would find a little road that loiters up a hill,
A little vagrant, woodland road, gray-ribboned through the green,
Where berry brambles bar the way and orchard elders lean.

The highway is the world's way, but I would drop behind
To follow little luring paths that only laggards find.
The challenge of the bandit weeds, the tilt with startled bees-
What can the dusty highway give for journeyings like these?

The highway is the sun's way, and follows East to West;
But there are yellow, vagrant beams that love my road the best,
That linger down the weedy ways, where lady's-lace is spread,
Or slant through shady orchard paths and tint the tree trunks red.

The highway, the highway! - You follow where it calls;
I watch you through a leafy screen from crumbling orchard walls;-
I wait and smile among the green, and know that by and by
We'll lure you back through dust and dew - my little road and I.

If in the above passages we glimpse the spirit of the man Jutson, can we epitomize his work? Aside from his particular contributions, the one general characteristic that permeates all his endeavours, and evinced as much in his field notes as in his published works, centres on his constant attention to the real world of observation made in the field, for as L. K. Nash says:

...Danger arises [in science] only when obsession with theoretical abstractions denies proper attention to the phenomenal specifics in which these abstractions are rooted, by which they are secured, and through which they will ultimately come to be extended. (1976, p. 606)

And it is in this, the open-minded recording of the phenomenal specifics of geomorphology, that Jutson's work will stand as a monument to the spirit of true science.

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