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JOURNAL OF THE TRANSACTIONS  
OF  
THE VICTORIA INSTITUTE.

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VOL. XXVI.



JOURNAL OF  
THE TRANSACTIONS  
OF  
The Victoria Institute,  
OR,  
Philosophical Society of Great Britain.

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EDITED BY THE HONORARY SECRETARY,  
CAPTAIN FRANCIS W. H. PETRIE, F.G.S., &c.

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VOL. XXVI.



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\* \* \* *The Institute's object being to investigate, it must not be held to endorse the various views expressed at its meetings.*



## PREFACE.

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THE Twenty-Sixth Volume of the *Journal of the Transactions* of the VICTORIA INSTITUTE is now issued. It is a record of the various important questions taken up in papers by competent authors, carefully investigated, and impartially discussed at the Meetings by those who have studied the subjects considered. The papers and discussions in this volume are upon the following subjects:—"On the Route of the Exodus," by Dr. EDOUARD NAVILLE, who for a long series of years has taken a leading place among Egyptian explorers, and to whom the world owes the discovery of Pithom, Bubastis, and other ancient sites; the remarks of the Right Rev. Bishop STALEY, D.D., Sir JOHN COODE, K.C.M.G. (the late), Sir THEODORE FORD, and others are appended. "From Reflex Action to Volition," by Dr. ALEX HILL, Master of Downing College, Cambridge, in writing on which he, as an anatomist, describes the structural relations of the several parts of the central nervous system, the brain, "the machine with which the mind works"; combats the views advanced by Weismann and his school—that evolution takes advantage of "chance" differences between animals—and believes in

the transmission of acquired mental characteristics in man, *i.e.*, of modified brain tissue. He urges that modern research proves that man is responsible for his actions. The President, Sir JOSEPH FAYRER, K.C.S.I., M.D., F.R.S., Dr. ROBERT JONES (of Earlswood), Dr. ALFRED T. SCHOFIELD, and others joined in the discussion. "The Weak Sides of Natural Selection," by Mr. J. W. SLATER, F.C.S., F.E.S.; in considering which Professor E. HULL, LL.D., F.R.S., Dr. H. B. GUPPY, Dr. GERARD SMITH, M.R.C.S., and several naturalists took a part. "On Serpent Worship and on the Venomous Snakes of India, and the Mortality caused by them," by Sir JOSEPH FAYRER, K.C.S.I., M.D., F.R.S., who very carefully describes the characteristics of every variety of Indian snake, thus making his work of much value to those resident in India; the subject was discussed by Sir RICHARD POLLOCK, K.C.S.I., and many Indian medical men.—The circulation of proof copies of this paper among many Indian and Colonial Members, caused a valuable communication to be sent in by Dr. AUGUSTUS MUELLER, giving a careful and useful account of the success attending his method of applying an antidote in cases of snake bite in Australia.\* "Notes upon some of the Recent Discoveries in the Realm of Assyriology, with special reference to the private life of the Babylonians," by Mr. T. G. PINCHES, of the Department of Egyptian and Assyrian Antiquities at the British Museum; this subject was considered by Major C. R. CONDER, R.E., D.C.L., M. BERTIN (the late), the Rev. H. G. TOMKINS, and other Assyriologists. "The Philosophical Basis of the Argument from Design," by the Rev. J. H. BERNARD, D.D. "On Enigmatical Flint Bodies named Paramoudra," by Mr.

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\* It is understood that a further trial of this remedy has since been recommended to the authorities by the President of the Medical Board at the India Office.

EDWARD CHARLESWORTH, F.G.S. (the late); in which the Institute possesses the last work done by one of the oldest and most careful of geologists. "The Glacial Period and the Earth-Movement Hypothesis," by Professor JAMES GEIKIE, LL.D., D.C.L., F.R.S., &c. The general interest felt in this subject, and its indirect bearing on the question of the Age of Man, tended to make many take part in its discussion, and in the appendix to the volume, a *résumé* of many current opinions will be found. "Notes on Useful and Ornamental Stones of Ancient Egypt," by Sir J. WILLIAM DAWSON, C.M.G., LL.D., F.R.S.; the author describes these, and the use to which they have been put by various nations ancient and modern: the subject, so full of interest to students both of sacred and secular history, was considered by many, including Mr. W. H. HUDLESTON, F.R.S., President of the Geological Society, Professors E. HULL, LL.D., F.R.S., and J. LOGAN LOBLEY, F.G.S., Major C. R. CONDER, R.E., D.C.L., LL.D., Mr. W. BRINDLEY, F.G.S., Mr. BOSCAWEN, Mr. J. D. CRACE, &c.

To all who have taken a part in the work done, the best thanks of the Members and Associates are due.

FRANCIS W. H. PETRIE, *Capt.*,  
*Hon. Sec. and Editor.*

JOURNAL OF THE TRANSACTIONS  
OF THE  
VICTORIA INSTITUTE,  
OR  
PHILOSOPHICAL SOCIETY OF GREAT BRITAIN.

—  
ANNUAL GENERAL MEETING,

THURSDAY, 25TH JUNE, 1891.

*The President,*

SIR GEORGE GABRIEL STOKES, Bart., LL.D., Sc.D., M.P.,  
Past President of the Royal Society,

IN THE CHAIR.

CAPTAIN FRANCIS PETRIE, F.G.S., &c., Hon. Sec., read the following  
Report :—

*Progress of the Institute.*

In presenting the TWENTY-FIFTH ANNUAL REPORT, the Council desires to congratulate the Members generally on the continued advance maintained by the Institute both at home and abroad; although in the Colonies it has been less marked than usual by reason of those adverse influences which have affected commerce.

2. The steady support which both Members and Associates have accorded, and the personal interest they have taken in the Institute's welfare have given strength and solidity to its work, and the remarkably few retirements have proved how fully all realise the desirability of making use of the present opportunities of advancing a Society the value of whose aims are recognised by all thoughtful men. Such cordial co-operation is of inestimable value to the progress



of the Institute, and it must afford no small satisfaction to the Members generally to see that their individual and collective efforts to make the Society what it should be have been so successful.

3. The Council are also much gratified to be able to record that the number of those in the high walks of Science who have joined or aided in the Institute's work is steadily increasing; such support has tended to enhance the value of its investigations in regard to Philosophical and Scientific questions generally, and especially in respect to those theories which might seem opposed to Religious belief. What has been accomplished of late has been in the interest of Religion as well as Science, and its tendency has been to bring about a truer appreciation of the results of Scientific inquiry.

4. It is satisfactory to note the continued progress of the "American Institute of Christian Philosophy," an independent society, founded on the lines of the Victoria Institute, whose statement of objects was adopted by it. Its founders are still members of this Institute, and among the foremost to bear testimony to the value of our work.

5. The number of Scientific Societies and other Public Bodies exchanging or purchasing the Transactions is increasing.

6. In consequence of the growing inconvenience of the Institute's late premises, into which the Institute was obliged to move in 1889, the Council sought to secure others, and fortunately obtained the apartments which had been first occupied in 1869, and since rebuilt.

7. The Library of Reference is becoming larger, and the Council wish to call special attention to the importance of an increase in the Library Fund.

8. The following is the new list of the President and Council:—

#### President.

Sir George Gabriel Stokes, Bart., LL.D., M.P.,  
Past President of the Royal Society.

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The Rt. Hon. Lord Halsbury, Lord High Chancellor.	
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Sir J. Risdon Bennett, M.D., F.R.S.	Alexander McArthur, Esq., M.P.
Sir Joseph Fayrer, K.C.S.I., F.R.S.	Rev. Preb. Robinson Thornton, D.D.

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W. N. West, Esq., F.R.G.S., F.R.Hist.S.	

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 The Bishop of Wakefield.  
 Rev. F. W. Tremlett, D.C.L.  
 Surg.-Gen. Gordon, M.D., C.B., Q.H.P.

His Excellency Dr. B. H. Gunning, F.R.S.E.  
 \*Rev. Preb. H. Wace, D.D.  
 Rev. J. J. Lias, M.A.  
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 T. Chaplin, Esq., M.D.  
 Admiral H. D. Grant, C.B.  
 Rev. Canon Girlestone, M.A.  
 Professor E. Hull, LL.D., F.R.S.  
 Major Freeman, M.A.  
 Sir J. Coode, K.C.M.G.  
 \* *Ex officio.*

9. The Council regret to announce the decease of the following forty supporters of the Institute:—

R. Armitage, Esq., *M.*; His Excellency A. C. S. Barkly, C.M.G., *A.*; Rev. E. Bersier, D.D. (*Cor. Mem.*); R. C. L. Bevan, Esq., *F.M.*; C. H. Binstead, Esq., *A.*; J. E. Briscoe, Esq., *L.M.*; H. Butler, Esq., *F.M.*; Rev. Canon W. Cadman, M.A., *M.*; Right Rev. Bishop H. Callaway, D.D., *A.*; Professor T. Carnelly, F.C.S., *M.*; Very Rev. Dean Croghan, M.A., *M.*; Rev. Howard Crosby, D.D., *M.*; W. Delpratt, Esq., M.R.C.S., *F.A.*; Professor J. E. Dwinell, A.M., D.D., *A.*; Sir R. N. Fowler, Bart., M.P. (who rendered valuable services to the Institute, both as a Life Member and as a Trustee of the Institute from its foundation); Right Rev. Bishop T. V. French, D.D., *A.*; Rev. R. Gascoyne, M.A., *A.*; Mrs Gordon of Fyvie, *A.*; Rev. W. Guest, F.G.S. (*Cor. Mem.*); W. J. Gunning, Esq., *A.*; J. Hall, Esq., *F.M.*; Rev. J. G. Howes, M.A., *M.*; J. Jardine, Esq., M.A., LL.D., B.L. of the University of France, *A.*; Rev. J. Kay, D.D., *M.*; Rev. Canon H. P. Liddon, D.D., D.C.L., *A.*; Rev. Principal J. W. McKay, D.D., *A.*; Rev. O. D. Miller, D.D. (*Cor. Mem.*); J. Moore, Esq., *L.M.*; Capt. G. Morton, United States Navy, *A.*; Prof. G. S. Morris, M.A. (*Cor. Mem.*); Rev. Preb. W. A. Osborne, M.A., *M.*; Prof. E. Presensée, B. Th. (*Cor. Mem.*); C. B. Radcliffe, Esq., M.D., *M.*; Rev. W. H. Rule, D.D. (*Cor. Mem.*); J. S. Sutcliffe, Esq., *A.*; Mrs. Voile, *A.*; Sir F. A. Weld, G.C.M.G., *M.*; N. Whitley, Esq., C.E., F.R.M.S., *A.*; L. T. Wygram, Esq., Q.C., *L.A.*; Rev. E. Young, M.A., *A.*  
*F.* Foundation. *M.* Member. *A.* Associate. *L.* Life.

10. The following is a statement of changes:—

	Life		Annual	
	Members.	Associates.	Members.	Associates
Numbers on 10th July, 1890 ....	58	40	359	796
Deduct Deaths (6 H.C. Membs.)	3	1	13	17
„ Retirements .....	—	—	4	25
	55	39	— 17	— 41
			342	755
Changes .....	2	—	— 3	1
	57	39	339	756
Joined to June 18, 1891 .....	2	4	26	52
	59	43	365	808
Total .....	1275	102	1173	
Hon. Correspondents number 125.			Total .....	1400.

*Finance.*

11. The Treasurer's Balance-sheet for the year ending December 31, 1890, duly audited, shows a balance creditor of £28 9s. 10d. The amount invested in 2½ per Cent. Consols is £1,365 18s. 9d.

In presenting the accounts, the Council cannot forbear referring to the valuable service rendered to the Institute by one of the Auditors, Mr. Gibbs Crawford Harrison, who now retires after having acted as an Auditor for twenty years. Mr. Harrison's high position in the Pay Department of the Royal Navy peculiarly fitted him for the post he now relinquishes to Mr. J. E. Wakefield, whose public duties have rendered him a not less valuable successor.

The Council desires to urge the great advantage it would be were Members to remit their Subscriptions during the first half of the year, as a large proportion already do. Were this the rule with all, the whole machinery of the Institute would work with an ease that would greatly promote its success. Forms for the payment of the Subscriptions through a banker are used by a large number, and may always be had.

The arrears of subscriptions are as follow :—

	1882.	1883.	1884.	1885.	1886.	1887.	1888.	1889.
Members ....	3	3	11	1	2	4	2	8
Associates ....	7	1	3	7	10	16	10	16
	<hr/>	<hr/>	<hr/>	<hr/>	<hr/>	<hr/>	<hr/>	<hr/>
	10	4	14	8	12	20	12	24

## 12. MEETINGS.

MONDAY, DECEMBER 1, 1890.—“A Sketch of the Geological History of Egypt and the Nile Valley.” By Professor E. HULL, LL.D., F.R.S., F.G.S., late Director of the Geological Survey of Ireland.

The results of the surveys of the author and many leading geologists are given, and the paper is all the more valuable considering certain controversies.

MONDAY, JANUARY 5, 1891.—“On Human Responsibility.” By the Right Hon. Lord GRIMTHORPE.

MONDAY, JANUARY 19, 1891.—“Notes upon some of the Recent Discoveries in the realm of Assyriology with special reference to the private life of the Babylonians.” By T. PINCHES, Esq., Assistant Keeper of Egyptian and Oriental Antiquities at the British Museum.

MONDAY, FEBRUARY 2, 1891.—Lecture on “Results of Recent Babylonian Archæology.” By W. ST. C. BOSCAWEN, Esq., F.R.Hist.Soc.

- MONDAY, FEBRUARY 16, 1891.—“On Agnosticism and its Tributaries.”  
By Professor H. LANGHORNE ORCHARD, M.A., B.Sc.
- MONDAY, MARCH 2.—“Deontology.” By Rev. H. J. CLARKE.
- MONDAY, MARCH 16.—“On the Reality of the Self.” By W. L. COURTNEY, Esq., M.A., LL.D.
- MONDAY, APRIL 6.—“On the apparent Cruelty of Nature.” By Rev. THEODORE WOOD, F.E.S.
- MONDAY APRIL 20.—“Notes on Philosophy and Medical Knowledge in Ancient India.” Surg.-Gen. GORDON, M.D., C.B., Q.H.P.
- MONDAY, MAY 4.—“On the Enigmatical Flint Bodies bearing the name Paramoudra.” By EDWARD CHARLESWORTH, Esq., F.G.S.
- MONDAY, MAY 11.—“Notes on Useful and Ornamental Stones of Ancient Egypt.” By Sir J. WM. DAWSON, C.M.G., F.R.S.
- THURSDAY, JUNE 25.—*Annual Meeting held at the Society of Arts House*—  
Address by Monsieur E. NAVILLE “On the Route of the Exodus,”  
and his researches in regard thereto.

The meetings have been fully attended this session, and the advantages of the Institute's new premises have greatly added to the general comfort.

### *Publications.*

13. The Twenty-Fourth Volume of the Transactions is now about to be published; a brief delay in its issue has arisen through the illness of some whose writings it contains. It includes papers and communications of much importance, including a translation of a recent address by Professor Virchow, in which he summarised the results of the researches of European Anthropologists during the last twenty years, in regard to Man and his place in Nature. The other papers, and the discussions thereon, are by men whose names and the value of whose scientific researches are a guarantee for the “full and impartial” character of their investigations, and for the manner in which they have considered the mutual bearing of the various scientific conclusions arrived at in the several distinct branches into which Science is now divided, in order to get rid of contradictions and conflicting hypotheses, and thus promote the advancement of true Science.

The arrangements whereby foreign supporters may not only contribute papers, but take a part in the discussions by communicating opinions in MS., have added much to the value of the *Transactions*, and have led to a marked increase both in the number of the Institute's Foreign and Colonial



Members and Associates, and in the circulation of the Journal among the general public; one reason assigned by many for seeking to obtain the *Transactions* being that "the papers and discussions often contain careful and impartial examinations of questions and theories of Philosophy and Science, which are said to militate against the truth of Revelation."

### *The People's Edition.*

14. The publication in a cheap form—in a People's Edition—of twelve of the more popularly written papers in the *Journal of Transactions* continues. This Edition has been brought before the public at home, abroad, and in the colonies, and it is very desirable that the contributions to the Special People's Edition Fund should be sufficient to enable that edition to be largely introduced into India, as has been urged by many correspondents.

15. The translation and publication of papers in the Journal into the languages of Europe and Asia by local Members continues.\*

[In addition to many referred to of late: further Translations in India, into Urdu, and in South America, into Portuguese, have been recently notified by local members.]

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\* *On some Uses made of the Institute by its Members.*—The Victoria Institute has been found to meet a need felt both at home and abroad, especially in our Colonies and India, where *the want of a true appreciation of the actual results of scientific inquiry* has led many, especially the less informed, to credit such fallacious statements as that "Science and Philosophy are alike opposed to Revelation," and that "the progress of Science has given a death-blow to all belief in the truth of the Bible." (As one result of this, the Bible is a forbidden book in many a Board School at home and in our Colonies.) Under the Victoria Institute's present organisation, its Members, both at home and in other parts of the world, are now able to make use of the results of the Institute's investigations to dispel such erroneous ideas. The thought of so utilising the Institute originated with its country and Colonial Members: and these have used the papers in the Journal as lectures, or to lecture from, in their respective localities, or have corresponded with the Institute as to the preparation of such lectures—have reprinted portions of the Journal in foreign and Colonial journals—have published translations of its papers (in many countries)—and have got Local Associations and Public Libraries to subscribe for the Journal.

*Conclusion.*

16. Whilst the Institute's continuous progress is a subject for thankfulness, the impartial observer will not fail to notice that its numbers are small, considering its now world-wide character, there is no country in which its Members are not to be found, or where the necessity for its existence is not felt, but it has become necessary that such a Society, with so widespread a constituency, should be stronger in numbers both at home and abroad, and the Council would, in conclusion, specially ask the co-operation of all in adding to the Institute's numbers. Were each Member and Associate to seek to add one more to its adherents, the Institute's power for usefulness would be doubled, and surely no higher incentive could be found to impel to so needed a work, than that expressed in the words of its motto : *Ad majorem Dei gloriam.*

G. G. STOKES,

*President.*

## SPECIAL FUND IN 1890.

*Library.*

	£	s.	d.
Patteson, Rev. Canon J., M.A. ....	2	2	0

*People's Edition.*

Harries, G., Esq. ....	20	0	0
Dent, H. C., Esq., C.E., F.L.S. ....	1	1	0
Goode, Mrs. ....	1	1	0
Napier, J., Esq. ....	1	1	0
Harrison, Miss Grace ....	0	10	0
Johnson, Rev. G. H., M.A. ....	0	5	0
	<hr/>		
	£23	19	0
	<hr/>		

The following Balance Sheet was then read:—

# ANNUAL BALANCE SHEET.

RECEIPTS FOR 1890.				EXPENDITURE.			
	£	s.	d.		£	s.	d.
Balance .. .. .				Printing .. .. .			412 13 11
Subscriptions:—			10 17 8	Postage, &c. .. .. .			130 19 11
1 Life Member .. .. .	21	0	0	Binding .. .. .			17 18 7
1 Life Associate (renewed) .. .. .	10	10	0	Reporting .. .. .			29 8 0
6 Life Associates .. .. .	63	0	0	Stationery .. .. .			37 7 10
			*94 10 0	Advertising .. .. .			16 7 4
1 Member, 1883 .. .. .	2	2	0	Expenses of Meetings .. .. .			21 17 6
2 Members, 1884 .. .. .	4	4	0	Travelling Expenses .. .. .			13 19 8
2   "   1885 .. .. .	4	4	0	Salaries for Year:—			
1   "   1886 .. .. .	2	2	0	Late extra Clerk .. .. .	90	0	0
2   "   1887 .. .. .	4	4	0	The Clerk and Assistance .. .. .	62	1	0
2   "   1888 .. .. .	4	4	0				152 1 0
5   "   1889 .. .. .	10	10	0	Rent to Christmas, 1890 .. .. .			175 12 6
219   "   1890 .. .. .	459	18	0	Agreement .. .. .			2 6 0
11   "   1891 .. .. .	23	2	0	Housekeeper .. .. .			13 5 6
1   "   1892 .. .. .	2	2	0	Coals, Gas, and Oil .. .. .			5 10 10
25 Entrance Fees .. .. .	26	5	0	Insurance .. .. .			0 12 0
2 Associates, 1885 .. .. .	2	2	0	Sundry Office Expenses .. .. .			3 18 0
2   "   1886 .. .. .	2	2	0	Library, Books, Repairs, and Moving .. .. .			54 1 10
6   "   1887 .. .. .	6	6	0	Organising Expenses .. .. .			262 10 0
9   "   1888 .. .. .	9	9	0	Bankers' Charges .. .. .			1 0 6
38   "   1889 .. .. .	39	18	0	Balance, Cr. .. .. .			28 9 10
482   "   1890 .. .. .	506	2	0				
18   "   1891 .. .. .	18	18	0				
			1,127 14 0				
Div. on £1,365 18s. 9d. 2½ p.c. Consols .. .. .			36 12 4				
Literary Fund .. .. .			2 2 0				
Donations to Special Fund .. .. .			23 18 0				
Sale of Journals, &c. .. .. .			84 6 9				
			£1,380 0 9				£1,380 0 9

We have examined the Balance-Sheet with the Books and Vouchers, and find a Balance in hand of £28 9s. 10d.

JOHN ALLEN  
J. E. WAKEFIELD } *Auditors.*

\* To be invested.

W. N. WEST, *Hon. Treas.*

[The HON. SECRETARY (Captain FRANCIS PETRIE, F.G.S.) referred to the unavoidable absence of the Lord Chancellor and Dr. Naville, the author of the Address; the latter finding that his duties on his return from Egypt detained him in Geneva, asked that the Rev. Dr. W. Wright might be permitted to read the Address for him. As regards the Report, the Hon. Secretary alluded to only four "members" having retired during the year, a sufficient proof, if any were wanting, of the loyalty, he might say enthusiasm, which inspired the members for the cause of the Institute: an enthusiasm which, during the 21 years he had acted as the Institute's Honorary Secretary, had helped to raise the number of its adherents from 201 to its present total of 1,400 supporters; and of which there were found many other indications, among these being the translation and publication by various members, of Papers in the Journal into the language of the countries in which they resided; the foundation by members in the United States of a special Society, on the exact lines of the Institute, for their own vast and splendid land, whilst at the same time they continued their support as members of the Victoria Institute.]

Sir JOSEPH FAYREER, K.C.S.I., F.R.S.—I have been somewhat unexpectedly called upon to take the place that would have been much better occupied by the distinguished Vice-President, who is prevented, by important duties, from being present here to-night. I regret that it should be so, but am confident that even he cannot take more interest in the Institute than I do. I have that qualification at least for asking you to accept the Report just read. It is very gratifying and satisfactory to find the progress of the Institute so steady and sure, and its work spreading all over the world. Its principles are such as should be acceptable to reasonable persons. You will observe that the Scientific element predominates largely; the President at the present moment holding a most distinguished position in the scientific world, and until lately President of the Royal Society. That, at all events, goes far to show that the Scientific aim and tendencies of the Institution are not to be despised. You know the objects of the Institute so well that it is not necessary to restate them to you. I have heard of some who described one of its objects as the reconciliation of Science with Religion. Now, Religion and Science need no reconciliation. (Cheers.) It is perfectly probable—nay, more than probable that men frequently



misunderstand the one and misinterpret the other; it is only by patient investigation—by possessing our souls in patience, and not generalising and arriving at conclusions too early that we may hope to reach the truth. (Cheers.) One object of the Victoria Institute is to teach people this. The work done and the Papers read here are all calculated to serve that purpose. There is much in some of those that have been before the Institute lately of particular interest, and it seems to me that such is the case in that which we expect to listen to to-night. It is a matter for congratulation that the financial position of the Institute is also satisfactory—it might be more so, and let us hope it will.

It is a subject for great regret that we have had such severe losses by death during the past year—twice the average number. It is not only in the scientific progress of the Institute that we may be encouraged; it is well to know that its Journals are being spread over the world, whilst some of its Papers are being printed in a simple form for the less educated. There is so much to do this evening, whilst others have interesting matters to speak on, that I will not detain you any longer, but move “That the Report now read be received, and that the thanks of the members and associates be presented to the Council, Honorary Officers, and Auditors for their efficient conduct of the business of the Victoria Institute during the year.” I do not know whether I am out of order in doing so, but I would like to refer specially to the cordial thanks due to Captain Petrie for his long and constant service. (Cheers.) It is to his calm and dispassionate judgment in dealing with the difficult duties that are inseparable from his office and such work as comes before him, that the success of this Institute is in no small degree due.

The VEN. A. E. MOULE, B.D., Archdeacon of Mid China.—It is a very great privilege and pleasure to second this resolution—not that I have any special title to your attention this evening, but I may claim that my interest in the Victoria Institute is as old as the Institute itself. My dear father was one of its first members, and he inspired me with interest in it. I remember the eagerness with which he watched its birth and early years, and ever since that time I have been specially interested in its progress, and during the latter part of my stay in China, which now extends to 30 years, I have ever looked forward to the arrival of *The Transactions* with pleasure.

I need not inform this large meeting that there is not merely bold infidelity, but that which is far more pathetic, a manifest unrest, which falls on thousands in the present day—that a very decided unsettlement of thought extends to the furthest parts of the earth; and in the great city of Shanghai, with which I have been most concerned during the last eight years, it is not so much bold outspoken infidelity that I have in my humble efforts to contend with, as the free thought that pervades the place. We have not to meet so much the open and bold attacks on revealed truth and the inspiration of the Bible, as a spirit of Agnosticism; and I have turned to the Transactions of this Society as a treasure of untold wealth and importance, and have been quite sure that I should find there, not what is fallaciously called science, but what is rightly so-called—the result of a careful investigation of the economy of nature and science in all its branches; not a hasty, but a calm and impartial investigation accompanied by a serene faith and trust in the Bible. I suppose it is generally thought that the firmer your faith in the Bible may be the more cramped will be your interest in and investigation of science: that is a mistake. The more unwavering our belief in the truth of the Bible and the more firm our faith in the Bible, the more free will be our investigation of science. Surely that is the truth that we find in the investigations of the Victoria Institute.

We have been told that some of our Papers have been translated into different languages. Now a remarkable change is coming over China at the present time. A great system of competitive examinations has prevailed there for more than a thousand years, by which the lowest in the land can rise to the highest position. This is carried on within a certain definite circle of knowledge—we should consider it a very limited circle—divination, philosophy, poetry, history of China and subjects of that kind. Now this circle has been broken into and is being expanded before the investigations of western science; questions of geology, electricity, mining, and western history are being put, and the Chinese are now scrambling almost for books of science, and books of science are being translated largely by missionaries and largely also by members of official bodies, the customs service and the consular service in China, for the use of the learned Chinese. Now I ask what better books on science can we place in the hands of these eager and thirsty Chinese than the *Transactions*, or Extracts

from the *Transactions*, of our Institute? I have great hopes in this direction. My son in China is now studying for this special translation work, and when I go back I shall certainly suggest to him that one of the most valuable works to be translated into that language would be the Victoria Institute Papers. I thank God for this Institute—may it live long and prosper. I cordially second this resolution. [Resolution carried *nem. con.*]

Rev. Prebendary ROBINSON THORNTON, D.D.—I rise to return thanks on behalf of the Honorary Officers and Council, and do so with great pleasure. I can assure you that all who are connected with the working staff, so to speak, of the Institute, are very much obliged to you for your very kind appreciation of their work. It has been to us all a labour of love and we shall be inspired by your kind reception of our efforts.

The PRESIDENT.—It is a matter of much regret to Dr. Naville that he is not able to be with us to-night; but by his special wish the Address which he has been so kind as to prepare will be read by the Rev. W. WRIGHT, D.D.

The Rev. W. WRIGHT, D.D.—I consider it a very high honour, as it is to me a sincere pleasure, to be asked to read this extremely interesting Address.

## THE ROUTE OF THE EXODUS.

BY EDOUARD NAVILLE.

THE route which the Israelites followed when they were leaving Egypt is a topic on which travellers and commentators of Scripture have dwelt at great length, and on which they have put forth most divergent views. Though there are still many doubtful points on which we cannot pronounce with certainty, the excavations made recently have thrown much light on several points of the Exodus, especially on the first days of the journey. They have contributed to elucidate the passage of the Red Sea, the crowning episode, the historical character of which is not denied even by authors of well known rationalistic tendencies.\* This great event I consider also as the limit of my subject. I do not intend to follow the Israelites beyond the

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\* Wie indess auch der eigentliche Vorgang dieses in seiner geschichtlichen Gewissheit feststehenden Ereignisses gewesen sein mag. . . . Ewald, "Gesch. der Volkes Israel," II., p. 109.

borders of Egypt, but I should like to describe how the scriptural narrative of the Exodus seems to me to be explained in the light of the late discoveries in Egypt.

I shall recall only in a few words what concerns the arrival of the Israelites in Egypt. Most Egyptologists have adopted as correct the statement for which we are indebted to the Byzantine chronographer Syncellus, who says that it was under the king Apophis, in Egyptian *Apepi*, that Joseph attained the high dignity which is described in Scripture. Apepi is known to us as one of the last, perhaps even the very last, Hyksos king. The Hyksos were foreign invaders, and, in all probability, Mesopotamians, who had been driven out of their country by great events which took place in the valley of the Tigris and the Euphrates. They were a mixed race; the mass of the population seems to have been Semitic, while their rulers, judging from the type of their faces, such as they are seen on the monuments of Tanis and Bubastis, were of Turanian origin. Undoubtedly their invasion had been marked, as is related by Manetho, by destruction, plunder, and violence, as is usual in Eastern wars; but the Hyksos had soon yielded to the influence of the more cultivated race over which they reigned. The conquered had by degrees overcome the conquerors, who had adopted the customs, the language, the writing, the civilization of the Egyptians; all except the religion. For, notwithstanding several centuries of dominion, the religion still raised between the Hyksos and their subjects an insuperable barrier. "They reigned ignoring Ra," meaning in hostility against the Sun-god. Such is the way in which a native queen describes their rule two centuries after the first rebellion against them.

It is probable that the fact of the Hyksos kings being Mesopotamians, contributed to dispose them favourably towards the Hebrews who had the same origin. It is well known that for Abraham and his family, and especially Jacob, Mesopotamia, *Aram Naharaim*, was above all *their* country,\* whereas they considered themselves as strangers in Canaan. "An Aramean ready to perish, or, wandering, was my father," says the author of Deuteronomy.† The tradition lasted down to the time of Josephus. This Jewish writer relates the events of Genesis in a narrative which is parallel to that of Scripture, and which is based on the Holy text. When

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\* Genesis xxiv., 4, 11, etc.

† xxvi., 5, margin of the Revised Bible.

he reaches the point of the arrival of Jacob into Egypt, like Genesis also he interrupts his narrative in order to introduce the description of the family of the patriarch; but before beginning it he gives the following curious reason for quoting all the names: "I thought it necessary to record those names, in order to inform those who do not suspect it, that we are Mesopotamians and not Egyptians."\*

It is easy to notice in the narrative of Scripture that there is a difference between the king and his subjects. The native Egyptians could not look favourably on the establishment of strangers who belonged to the race whose rule they hated. "Every shepherd is an abomination unto the Egyptians." I believe that this passage must not be understood as referring to all shepherds in general. We must remember that in the Egyptian inscriptions the most usual name of the Hyksos is *the shepherds* or *the nomads of Asia*, and it is natural that the Egyptians should have felt towards the Hebrews the same antipathy as towards their rulers who had the same origin as the Hebrews, and who were hostile to the Egyptian gods. This is the origin of the ill will, the *δυσκολία*, which according to Josephus existed between the Egyptians and the Hebrew immigrants.

The Israelites were settled in the land of Goshen. The excavations which I made in 1885 at Saft el Henneh, six miles East of Zagazig, have enabled me to determine the exact site of the land of Goshen, at least of the territory which was originally assigned as residence to the family of Jacob; for we must admit that when the people increased in number, they extended beyond the limits of the land which had been allotted to them at the beginning. They spread in the south towards Heliopolis, in the north towards Tanis, and in the east in the direction of the Red Sea. I shall here briefly sum up the information which we derive from the hieroglyphical inscriptions and the ancient authors, apart from Scripture, in reference to the land of Goshen.†

The word Goshen, *גושן*, has been translated by the Septuagint *Γέσεν Ἀραβίας*, *Gesen of Arabia*. The name Arabia must be interpreted here as meaning the *nome* or province of Arabia mentioned by the geographer Ptolemy, and by Pliny, and the capital of which was called by the Greeks Phacusa. Let us go back not to the time when the Septuagint made their translation, viz., under Ptolemy Philadelphos, when great

\* Josephus, "Ant. Jud.," II., 177, ed. Niese.

† See "Goshen," p. 14-20.

changes had taken place in the division of the land; but as far as the XVIIIth or the XIXth dynasty, when the Israelites still occupied the land which they had received as allotment from the Hyksos king. At that time Egypt of the North, the Delta, was divided into 15 nomes or provinces, instead of 23, which existed under the Ptolemies and the Romans. One of the largest in extent had for its capital Heliopolis,\* called in Scripture *Aven* and *On*. It comprised the greatest part of the land which is crossed by travellers going from Cairo to Suez, and where are at present the cities and villages of Kalioub, Shibeen el-Kanater, Belbeis, Zagazig, and Tell el-Kebir. The great city of Bubastis, one of the chief residences of the Hyksos kings, was also included in this province, which was limited on the east by the nome of Pithom, called under the Ptolemies, Heroopolitan. The nome of Arabia and that of Bubastis, which later on were separated from the nome of Heliopolis, did not yet exist as distinct administrative divisions. About six miles east of Bubastis was the region called *Kesem* or *Kes*, which seems to have been also styled *the water of Ra*. A Dutch scholar, Van der Hardt, had already suggested in the last century that the root *Kes* of the name *Kesem* was to be found in the second syllable of the name *Phacusa* where it is preceded by the Coptic article *pa* or *pha*. Phacusa we know from Ptolemy to have been the capital of the nome of Arabia. As late as the 4th century of the Christian era, a woman coming from France and going to the Holy Land and to Egypt, Silvia Aquitana, mentions repeatedly in the narrative of her pilgrimage, that the land of Goshen was in her time the nome of Arabia, *civitas Arabia*.†

In the hieroglyphical inscriptions there seems to be an allusion to the presence of the Israelites in that region; for a text written at the time of Menepthah, the King of the Exodus, speaking of the neighbourhood of Pi-Bailos, the present Belbeis, says "that the country around was not cultivated, but left as pasture for cattle, because of the strangers. It was abandoned since the time of the ancestors." This proves that the land of Kes or Kesem was not inhabited;

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\* The fact that Goshen belonged to the nome of Heliopolis, explains the passage of Josephus, who says that Pharaoh allowed Jacob to live at Heliopolis, where his shepherds had their pastures: *συνεχώρησεν αὐτῷ ἦν . . . ἐν Ἡλίου πόλει ἐν ἐκείνῃ γὰρ καὶ οἱ ποιμένες αὐτοῦ τὰς νομὰς εἶχον*. Jos., "Ant. Jud.," ii., 188.

† Silvia Aquitanae, "Peregrinatio ad loca. Sancta." Ed. Gamurrini, 2 ed., p. 18—20.

it was a region of pastures, and could be given to strangers for grazing their cattle, without driving out the natives or depriving them of their land. A country of that kind was much more convenient for shepherds like the Hebrews, than other parts of Egypt, well cultivated, and where the population was very dense. In that sense Goshen was for them the "best of the land."\*

Moreover, as we know from the excavations at Bubastis, this city was one of the chief residences of the Hyksos kings, who raised there more important constructions even than those of Tanis, which was generally considered as having been their capital. It is quite possible that Joseph resided frequently at Bubastis, which was at the entrance of the land of Goshen. Therefore he had his family close by, and he could easily communicate with them. Thus Goshen, properly speaking, was the region situate east of Zagazig, towards Tell el-Kebir, and extending in the south beyond Belbeis in the direction of Heliopolis. It is a country which is familiar to the travellers who, as is the fashion now, take the road of Port Saïd for coming into Egypt or for leaving it. They pass through the land of Goshen in its whole length, and not only the original Goshen of the family of Jacob, but all the region to which this name was given, and which extended further in proportion as the people increased in number. It is probable that all the land occupied by the Israelites was called Goshen, and thus it became synonymous with another name which is purely Egyptian, and which dates only from the XIXth dynasty, I mean the name *land of Rameses*, which is found as late as the Septuagint, and even afterwards.

It is probable that this name dates from Rameses II., a vain and boastful king, who, as far as we can ascertain, was the persecutor of the Israelites, and whose chief desire seems to have been to cover the land with as many constructions as possible bearing his name, either by raising new ones or by usurping on a large scale the works of his predecessors. There were several *cities of Rameses* in Egypt; one of them was certainly in Goshen. In the same document which I quoted before, the narrative of the pilgrimage made by a woman in the 4th century, the author says that "going towards the city of Arabia she passed through the city of Rameses, the ruins of which were considerable; but the only monument to be seen was a stone on which were sculptured

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\* Genesis xlvii., 11.

two statues, said to be Moses and Aaron." If this tradition is to be trusted as to the site of the city, Rameses must have been in the vicinity of Saft el-Henneh (Goshen), east of Zagazig, not far from Tell el-Kebir.

According to historical synchronisms, Rameses II. must have been the persecutor of the Hebrews, whereas the Exodus took place under the reign of his son. Since the history of the reign of Rameses has become better known, his prestige and glory have declined considerably. It has been recognized that he was bent chiefly on dazzling his subjects and the future generations by his outward show and his magnificence, which concealed but imperfectly the rapid progress of decay in his weakened and exhausted kingdom. He saw near his residence of Bubastis a foreign race, which had never amalgamated with his subjects, and which at any time might become a danger to his kingdom. He knew by experience that the Asiatics in the East were troublesome neighbours; he could remember the difficulty he had found in beating the Khetas, to whom nevertheless he had been obliged to offer an honourable peace. The strangers, the Hebrews, were settled in a district which was the very gate of Egypt, and the key of the kingdom. Nothing is more natural than that Rameses should wish to make profit for his realm out of the presence of those strangers, instead of their being a constant threat to its safety. We should even say that it was good policy on his part. Why not turn them into useful workmen and labourers? Scripture says that Pharaoh employed the Israelites in building the store cities of Pithom and Raamses;\* in other words, he compelled them to be masons. He changed their manner of life, and instead of grazing their cattle, they had to make bricks and to raise walls. Josephus gives a more complete account of what they had to do: "they had to divide the river into many canals, to fortify cities, and to build dykes so that the river might not overflow and make lakes." Pharaoh treated the Israelites as if they had been prisoners. In a famous picture of the time of Thothmes III, which is found in a tomb at Thebes, we see prisoners of a Semitic type occupied in making bricks; some of them dig out the clay, others pour water over it, others knead the clay, others put it in moulds. The work is done under the eye of the overseer, who is sitting with a stick in his hand, and waiting patiently until he shall have to make use of his sign of office. These men are called war prisoners, therefore they are not Hebrews;

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\* Exodus i, 11.



but this picture gives a good idea of the manner of life which the oppressor enforced upon them. No doubt the yoke of Pharaoh was heavy; besides, a sudden and compulsory change of habits does not take place easily. It is not without pain and suffering that shepherds accustomed for generations to the free and easy-going life of driving their flocks in pastures, are tied down to the work of bricklayers and masons, under the eye of harsh and tyrannical overseers.


"The Israelites built for Pharaoh store cities, Pithom and Raamses." I mentioned before that the exact site of Raamses had not yet been discovered. It must have been in the neighbourhood of Phacusa, not far from the present Tell el-Kebir. As for Pithom, my first excavations determined its exact site, and even laid bare some ruins of the city and its temple.\* On the south side of the Freshwater Canal which runs from Cairo to Suez, through the Wadi Tumilat, about twelve miles from Ismaïlia, are the ruins of European houses now abandoned, but where a few years ago was a settlement of engineers and workmen who dug the canal. The French have called it *Ramsès*. The Arab name is *Tell el-Maskhutah*, which means "the mound of the statue," because of a monolith in red granite which stands there, and represents Rameses II. sitting between two gods. The existence of this statue and the fact that other monuments bearing the name of the same king were discovered in the garden of the chief engineer who resided there, induced Lepsius to consider Tell el-Maskhutah as being the site of Raamses. I settled there to begin excavations, in the hope of finding proofs that it was the city of Raamses. But the result of the work, the inscriptions discovered, showed that it was not Raamses but Pithom, and that the region around it had the name of *Thuket*, which the Israelites interpreted as *Sukkoth* (tents).

Pithom is the Egyptian *Pi* or *Pa Tum*, and means "the house," or "the sanctuary of Tum," the setting sun. Pithom was the religious name of the city, as *Pi Beseth* was the religious name of Bubastis, *Pa Amon*, or *No Amon*, that of Thebes, *Pa Neith* that of Saïs. The civil name of the city was *Thuku*, or *Thuket*, which was also that of the region around it, a region which the hieroglyphical inscriptions show to have been a border land. Brugsch has pointed out that the name of Thuket was the origin of the Hebrew *Succoth*; and I believe this interpretation to be perfectly in accordance

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\* See "The Store City of Pithom and the Route of the Exodus," 3rd edit.





with what we see not only in Egypt, but in all countries where two languages are spoken. In passing from one language to another, a proper name is generally not translated, it is only altered sufficiently to have a sense familiar to the people who have to use it. This new sense may be totally different from the original one. Examples of this fact are numerous in Egypt; it occurs also frequently in England where Norman words pronounced by Saxons took a sense absolutely different from their original meaning;\* and in my own country, in the cantons where German and French are spoken together. The Semitic form of Thuket was Succoth, a word familiar to the Hebrews, as it means tents.

Thuket, Succoth, was a district before being a city; its name is often mentioned in papyri of the XIXth dynasty. Its governor was an *aden*, evidently the same word as the Hebrew *adon*. There is a statue of one of those officials in the British Museum which was found at Pithom. From the papyri we get very important information concerning the district of Succoth. Its name is generally written with the determinative of foreign lands, although it was part of Egypt, thus showing that it was a border land. It contained what is called in Egyptian  *segair*, the same word as in Hebrew, סָגֵר. It means a wall or an enclosure of some kind, which was either a means of defence, a wall destined to prevent passing from the desert towards Egypt, or, perhaps, an enclosure for the cattle of the king, which were grazing in the neighbourhood.

Further information is given by a passage which I must quote in full,† following Brugsch's translation. It is a letter written by an official: "We have allowed the tribes of the Shasu, of the land of Atuma, to pass the stronghold of king Menephtah of the land of Succoth, towards the lakes of Pithom of king Menephtah of the land of Succoth, in order to feed themselves, and to feed their cattle in the great estate of Pharaoh . . . ." We learn from this passage that in the district of Succoth there were lakes or ponds of fresh water, near which there was good pasture land; and also a farm or estate belonging to the king, where the Bedouins of the desert asked to be allowed to feed their cattle.

\* I shall quote only one instance, the French *buffetier* became in English *Beefeater*.

† "The Store City of Pithom," p. 28.

These ponds or lakes are called by a Semitic word, *barokabuta* , the Hebrew , Arabic . The access to these lakes from the desert was possible only through a stronghold called by a Semitic name  *khetem*. A *khetem* is a kind of fortification which need not be considerable. It was specially destined to block a passage or a road; it might be translated more correctly a blockhouse. There were several *khetem* in Egypt. One of the most frequently mentioned is the *Khetem* of Zar, which was situate at the place now called Kantarah, on the Suez canal. There is a representation of it on a wall of the great temple of Karnak. It shows that the stronghold consisted of two gateways, with walls and towers placed on each side of a bridge, or possibly of a ford which crossed the Pelusiac branch of the Nile. It is natural to suppose that the *Khetem* of Succoth was of the same nature as that of Zar, and that it closed the place where, as we shall see further, the Red Sea could be crossed.

A very important fact concerning Succoth, for the knowledge of which we are indebted to the excavations made at Pithom, is the vicinity of the Red Sea, which extended much further north than it does now. Besides Pharaonic and Ptolemaic texts, there were found two stones with Latin inscriptions, giving us the Latin name of the city, *Ero*, or *Ero castra*, in Greek *Heroöpolis*. This city is often quoted by Greek and Latin authors, who are unanimous in stating that the city was built at the head of the Arabian Gulf, also called Heroöpolitan. Strabo and Pliny say it in the most distinct way. Agathemeros says that the Arabian Gulf began at Heroöpolis. Artemidoros, quoted by Strabo, states that the ships which went to the land of the Troglodytes sailed from Heroöpolis. Ptolemy fixes the latitude of the head of the Heroöpolitan gulf at one-sixth of a degree south of the city. If it was so as late as Ptolemy; if in his time the sea had not yet receded to its present limits, certainly it had not at the time of the Exodus. The extent of the Red Sea, at least as far as the northern end of the Bitter Lakes, is proved also by geological arguments. It is the opinion of Professor Edward Hull,\* and Sir William Dawson.† But I believe that at the time of the Exodus the Red Sea extended still further,

\* "Journal of the Victoria Institute," Vol. XXI., pp. 13 and 21.

† "Modern Science and Bible Lands," pp. 392, 397.

comprising also the present Lake Timsah. This view, the possibility of which is admitted by Sir William Dawson, has been expounded admirably by the French engineer Linant,\* who travelled in the country repeatedly between 1820 and 1830. According to his researches, the sea included Lake Timsah, covered the valleys now called Aboo Balah and Saba Biar, and reached as far as the village of Magfar.

Whether the sea extended only as far as the northern end of the Bitter Lakes, or whether, according to Linant, it went still further, the well established fact of the vicinity of the sea to the district of Succoth, and to its capital Pithom Heroöpolis, of which we know the site, is a very important element in determining the route of the Exodus. The identity of Pithom and Ero, which came out of the excavations in such a striking way, could already be concluded from the comparison of the translations of Gen. xlvi., 29, which reads thus: "And Joseph made ready his chariot, and went up to meet Israel his father, to Goshen." Here the Septuagint, instead of "Goshen," reads "near Heroöpolis," and the Coptic translator, who generally follows the Septuagint, has a variant, and reads "near the city of Pithom." Heroöpolis being a Greek name, it is natural that the Egyptian writer should replace it by the old native name. Many commentators have made use of this passage to disparage the value of the two versions, which now turn out to be quite correct.

A great Ptolemaic tablet, which was discovered in the excavations at Pithom, mentions another locality of the same name, *Pi Kerehet*, the house of the serpent. The inscription shows that it was a temple of Osiris, or what the Greeks called a Serapeum. The god was worshipped there under the form of a serpent. Considering as before in the case of Succoth, not the sense of the word, but its sound, it is certainly very like the *Pi Hahiroth* of Scripture, which is one of the places mentioned on the occasion of the Passage. Pi Hahiroth would thus be a locality in the district of Succoth. As it was a Serapeum, it is important to notice that the Itinerary of Antoninus mentions Serapiu as being eighteen miles from Ero. Standing on the pier of Ismailia, and looking over the Lake Timsah, the horizon is limited on the south by a flat ridge, a kind of table mountain, now called Gebel Mariam. Just at the foot of the mountain, on the

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\* "Mémoires sur les principaux Travaux d'utilité publique exécutés en Égypte," p. 195 et suiv.

south, and on the very bank of the Suez Canal, is an important Roman settlement, partly covered by the lagoons, but the ruins of which above the water cover an area of 500 yards square. This I believe to be Serapiu, Pi Kerehet. Its distance from Ero agrees nearly with the Itinerary, fourteen Roman miles instead of eighteen.

Let us now revert to the papyri, in order to get information about two other places mentioned as landmarks for the camp of the Israelites, *Baal-Zephon* and *Migdol*. As for the first, I quite agree with several scholars\* that it must not be considered as a city or even a village; it was a place of worship of a Semitic divinity in the form of a Baal. It was, as the Targum explains it, the sanctuary of an idol, the shape of which we do not know, but which may have been a mere stone. I believe it was something like the tombs of sheikhs, generally placed on hills, hundreds of which are met with in Egypt, and where people go for worship or to make pilgrimages, especially women. We might compare it also to the solitary shrines or chapels which are often seen in Roman Catholic countries. The word *ἐξεναντίας*, "over against," used by the Septuagint, seems to indicate that Baal Zephon was on the other side of the sea. It was the point towards which the camp of the Israelites was to make front, the direction in which they were to march. It is very like the name of *Baal-Zapuna*, which is read in one of the papyri of the British Museum; and if we adopt Philo's view, that Zaphon is the Northwind,† Baal Zephon was a divinity connected with the wind, and with the navigation on the Red Sea. We have no precise indication where it must be looked for; but as both Pi Hahiroth and Migdol were in the district of Succoth, I should place Baal Zephon on one of the heights between Lake Timsah and the Bitter Lakes, like Sheikh Ennedek.

We have more information about Migdol. It also is derived from a papyrus in the British Museum; a letter from a scribe who relates a journey very similar to that of the Israelites, in the following words: ‡ "I started from the great hall of the royal palace on the ninth day of the month of Epiphi, at the time of night, going after two slaves. When I arrived at the enclosure (segair) of Succoth, on the tenth of

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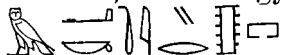
\* See Graetz, in Thayer, "The Hebrews and the Red Sea," p. 129. Dillmann, "Exodus," p. 142: "Der Ortsname—war ohne Frage hergenommen von einer dort befindlichen Cultusstätte einer Form des Baal . . ."

† Ebers, "Durch Gosen zum Sinai," p. 525.

‡ Brugsch, "Dict. Géog.," p. 51.

Epiphi, it was said to me, they spoke of the south, saying, let us cross over (to the desert) on . . . . . Epiphi. When I arrived at the stronghold (khetem) it was said to me, the two grooms going towards the mountain have crossed the wall north of the tower (Migdol) of Seti Merenphthah."

We do not know exactly the place where the scribe started from. The great hall of the royal palace is rather a vague expression; however, if we compare this letter with the others which are contained in the Anastasi papyri, we see that the city of Rameses is often mentioned. Several of the officers who write belong to the palace in the city of Rameses, so that we may fairly suppose that the same city was the starting point of our scribe. He starts in the evening, and after having probably travelled all the night, he reaches the next day the enclosure, the wall which protected Succoth on the east. His errand consists in getting information about two grooms, who have fled to the desert. At Succoth he is told that the fugitives have been heard to say that they were going to the south. Whether the report is true or not, it is natural that he should try to catch them in following the same direction. No doubt he goes south; and when he reaches the stronghold or block house, he hears that the two grooms have passed over the wall which is north of the tower, the *Migdol* of Seti Merenphthah. Evidently the tower and the wall must have been in the immediate vicinity, otherwise the people of the stronghold would not have been able to give him this piece of information. Their testimony is like that of eye-witnesses, therefore the fugitives could not have travelled a long distance before crossing the wall.

This blockhouse or stronghold we know already from the other text quoted before; it was the stronghold of King Menephtah which belonged to Succoth, and which closed the way to the lakes of Pithom and to the pastures of the royal farm. But the second text adds to our information two very important points—the stronghold was south of Pithom, which was first reached in coming from Egypt, and besides it contained a tower or migdol. South of Succoth there was a stronghold which, judging from analogy with the other of which we have a picture, closed a passage over the water. This stronghold had a tower, called in Egyptian by the same word as in Hebrew,  *Maktar* or *Maktal*, *Migdol*. From the aspect of the country, I should place Migdol on the height called by the French the Serapeum, and

where, until a few years ago, there was a bilingual tablet, Egyptian and cuneiform, dedicated by King Darius, which was most wantonly destroyed at the time of the digging of the canal. This Migdol was a watchtower, and it was also a protection against the raids of the nomads who, thanks to a phenomenon which took place occasionally, found the sea open, or could easily wade through, in order to pillage the royal domains on the Egyptian side. As it was, it proved to be a defence sufficiently effective to compel the nomads to ask permission from the officials stationed there, when they wished to pass for getting food for their cattle.

Knowing now the exact site of Pithom and of the region of Succoth, and the vicinity of the sea, which possibly extended as far as Magfar; having also determined conjecturally the sites of Pi Hahiroth, Baal Zephon, and Migdol, let us revert to the narrative of Scripture. The Israelites are dwelling in the Wadi Tumilat. From the original Goshen, the territory allotted to them near Bubastis, they have spread in the land of Rameses, on the east, as far as Pithom, which they have built, and on the south towards Heliopolis. The recent excavations made at Bubastis have shown that not only under the Hyksos kings, but also at the time of Rameses II., the city had a great importance: it probably was one of the chief resorts of the kings in the Delta, and the starting point of the expeditions to Syria and Palestine. I found there the statue of one of the sons of the king who was the fifth in the series, and who after the death of his elder brother became first cavalry officer of his father, and chief of the horse, meaning the chariots, which were an important part of the Egyptian armies, while there seems to have been only very little real cavalry. It was an officer of this rank who had the command of the chariots which perished in the Red Sea. Menepthah, the King of the Exodus, seems also to have resided at Bubastis; and it is quite possible that during the events which preceded the departure of the Israelites, the king was at Bubastis, very near the Israelites, and not at Tanis, as was generally supposed. This circumstance would considerably shorten the distances, and make the narrative more intelligible.

“And the children of Israel journeyed from Rameses to Succoth.”\* We have seen before that the site of the city of Rameses has not yet been determined; it must have been somewhere east of Saft el-Henneh, near Tell el-Kebir.

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\* Exodus xii., 37.

Rameses must not be taken here as meaning the store city of this name, it is the district around it, just as in the case of Succoth, their first station. The fortified city of Pithom did not open its gates to them; they encamped in the neighbourhood. At the time of the pilgrimage of Silvia Aquitana which I quoted before, Succoth is spoken of as being a slope of moderate size.\* The Israelites seem to have made the journey from Rameses to Succoth in one day, like the officer who followed the fugitives. Along their way they must have followed the canal dug by Rameses which watered the cities of the Wadi Tumilat, and which at the place where it emptied itself into the Red Sea formed those lakes to which the Bedouins asked for access.

In going to Canaan they had the choice between two different roads. There was one in the north which passing through Tanis and Daphnæ, reached the Mediterranean, and skirted its coast. It was decidedly shorter, but it passed at first through cultivated and well irrigated land, and also through important fortresses like Tanis, with large garrisons. It was the way of the great conquerors of the XVIIIth dynasty, and it is styled by Scripture "the way of the land of the Philistines." From the first, before any other indication is given as to the direction they were to follow, it is said that "God led them not by the way of the land of the Philistines, although that was near."† The other was the southern road which their ancestor Jacob had taken when he came to Egypt, since, according to the Septuagint, it was at Heroöpolis Pithom, that father and son had met after many years of separation. A few years ago the Bedouins coming from Syria frequently followed the same route, which was less convenient for an army, but well adapted for a people of nomads.

Leaving Succoth, its pastures, and its lakes, the Israelites had only to push straight forward; they skirted the northern end of the Red Sea; they had no river or sea to cross, and they could easily reach the desert. They began carrying out this plan, for "they took their journey from Succoth, and encamped in Etham, in the edge of the wilderness."‡ Etham is a name which has not yet been satisfactorily explained. From it the desert was named in which the Israelites journeyed during three days. At Etham the


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\* "Socoth autem est clivus modicus in media valle, justa quem collium fixerunt castra filii Israel."

† Exodus xiii., 17.

‡ Exodus xiii., 20.



Israelites received a command which at first must have seemed to them most extraordinary.\* “And the Lord spake unto Moses, saying, Speak unto the children of Israel, that they turn back and encamp before Pi-hahiroth, between Migdol and the sea, before Baal-zephon: over against it shall ye encamp by the sea. And Pharaoh will say of the children of Israel, They are entangled in the land, the wilderness hath shut them in.” Certainly this command was of a nature to shake the confidence of the Israelites in their leader. They had reached the desert, they had nothing in front of them, and instead of hastening towards the wilderness so as to be as soon as possible out of the reach of their oppressors, they were told to change entirely their route, to retrace their steps so as to remain on Egyptian soil, and even to put the sea between themselves and the desert. Surely it would encourage Pharaoh in his pursuit. For the king, the reason of this sudden change and of this extraordinary move was obvious. The Israelites were afraid of crossing the desert. They were entangled and wandering in the land of Egypt, because the desert was for them an insuperable barrier. This is in my opinion the right explanation of the words, “the wilderness hath shut them in;” viz., the desert which is in front of them prevents them from going out. Curiously the word translated here *shut in* is the Hebrew פָּקַד; the same word met with before, in Egyptian, in the description of the district of Succoth. The desert is for the Israelites a *segair*,  a wall closing the passage, as we saw there was one in Succoth.

It is to be noticed that whereas in other parts of Scripture, and especially in the description of the route in the wilderness, the geographical data are sometimes vague and always very concise, here they are given with a remarkable precision. It is not said to the Israelites merely that they are to stop near the sea in the most favourable camping ground, or something of the like. They are to reach a definite spot, the landmarks of which are given; on the north, Pi-hahiroth, the sanctuary of Osiris; on the south, Migdol, the watch tower on the hill, now called the Serapeum; in front, the sea; and on the opposite side, the shrine or the stone of Baal Zephon. The reason of this description seems to be the following: at that particular spot a phenomenon occurred which was to be the means of escape for the Israelites—the sea

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\* Exodus xiv., 1.

receded under the influence of the wind. "The Lord caused the sea to go back by a strong east wind all the night, and made the sea dry land, and the waters were divided."\* It has often been noticed by travellers in Egypt, that under the influence of a strong wind the sea recedes sometimes for a great distance, and comes back again to its former bed when the wind ceases or changes its direction. This phenomenon is not rare in Lake Menzaleh, which communicates with the sea; in Lake Bourlos, or even along the track of sand which lines the Mediterranean on the east of the Suez Canal towards the Syrian coast. There is nothing extraordinary in this taking place in the part of the sea between Lake Timsah and the Bitter Lakes; there the slow rising of the ground, which in later times cut off Lake Timsah from the Bitter Lakes, was already being felt; the sea must have been shallow and probably not very wide. I should even go further, and say that it had been known before that this phenomenon occurred at that particular spot, and that this is the reason why the spot is pointed out so exactly to Moses; that is also, in my opinion, the reason why the Pharaohs built there a khetem, or stronghold. I imagine that the result of the action of a strong wind was, in most cases, to cause the water to recede, and to create there a temporary and occasional ford, which people could easily wade through, as was seen north of Suez, at the end of the present Red Sea, before the canal was dug. As the wind in lowering the depth of the water could sometimes create a passage, it was necessary to close it; and, for this purpose the Pharaohs built there a watch tower, a *Migdol*, in order that the nomads coming from the desert, and who might be attracted by the rich pastures of Pithom, could not pass without being seen.

To the action of the wind we must add that of the tide, which is now felt in the Bitter Lakes. As for the wind alone, its effects on the sea are known in Egypt to the present day. That it should affect shallow water in a flat country is easily intelligible. Much more striking instances of the power of the wind compelling even a strong current to stop for a certain time have occurred elsewhere, and especially in my native country. On the title page of a book printed at Geneva in 1495, and which is called "Le Fardelet hystorial" (litt., the historical bundle), one reads the following

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\* Exodus xiv., 21.

words :\* “Printed in Geneva, in the year 1495, in which year there was such a very strong wind, on the ninth day of January, that it drove back the Rhone into the lake as much as one-fourth of a league above Geneva, and it looked like a wall of water, and it lasted nearly an hour before the water could flow.” This extraordinary event could take place when the river was much wider than it is now. The southern part of the city not being built, the river expanded into ponds and marshes; its depth and the strength of the current were much less than now, since its bed has been restricted everywhere by houses and embankments. A clergyman, Des Gallars, in Latin Gallasius, who wrote a latin commentary on Exodus, in the middle of the following century, alludes to this fact as proof of the opening of the Red Sea, and he adds that in his time there were still some ocular witnesses of this extraordinary event. †

The same thing happened again in 1645, and is related by several Genevese historians. ‡ On the 19th of January, during a very strong wind, between seven and ten in the morning, the inhabitants could go down on dry ground between the bridges, and pass from one bank to the other. Instances of the same kind might be quoted from several other countries.

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\* The passage reads thus in its picturesque old French : “Imprimé à Genève, l’an 1495, au quel an fit si très grand vent, le IXe jour de janvier, qu’il fit remonter le Rhône dedans le lac bien ung quart de lieue au dessus de Genève, et semblait être une montagne d’eau, et dura bien l’espace d’une heure que l’eau ne pouvait descendre.”

† Nunc ad dividendas aquas et patefaciendam per invia suo populo viam ventum immisit: idque ab Oriente, quoniam ab ea parte vehementior in illis regionibus esse solet. Quum igitur ventorum vi operatur Deus, in auctorem ipsum potius quam in organa quibus utitur aut effectus ipsos, oculos ac mentes defigamus. Novum autem videri non debet, absistere maria ac findi impetu venti, quum ordinario naturæ cursu ipsa impelli ac veluti in cumulos et montes efferi, atque interdum longe a litoribus summovei videamus. Intellexi a viris fide dignis, se paulo ante hæc tempora hic Genevæ in eo loco ubi Rhodanus lacu xiens alveum suum ingreditur, vidisse aquas Austri violentia ita repressas ut iis velut in acervum cumulatis, alveus siccus ferè per horæ spatium manserit. Atque eius rei superstites adhuc sunt oculati testes nonnulli. Nam eò ferè universa plebs concurrit. (In Exodum commentarii Nicolao Gallasio auctore, p. 88.)

‡ I shall quote only one authority, Calandrini, in a note on a Latin poem: Anno 1645, die Dominica Januarii decimâ nonâ, horis inter octavam decimamque Genevæ tam terribilis extitit impetus, ut celerem Rhodani fluxum retrogeret usque in Lemanium lacum, undæque muri instar coactæ cursum suum sisterent, adeo ut vado sub binis pontibus locisque vicinis facto, novitate rei numerosa commota plebs deambulaverit quasi in sicco, et pisciculos, etiamque majusculos manu collegerit quam plurimos.

In the case of the Israelites, Scripture relates in the plainest words what occurred: in a place where, as I said, the water was shallow, a strong east wind opened the sea and made a way through which the people passed. The mountains of water which are mentioned seem to indicate that there was a current of some kind which must have been produced by the tide. It has been objected that an east wind would have driven the water towards the Israelites, and not opened the sea, as the wind never acts as a wedge.\* It may be answered that here we must not take the word east as meaning east sharp; it is much more likely south-east, the well known *Khamseen*, which blows frequently at that time of the year, and often changes direction in the course of the day from east to south-west.† The Septuagint translate ἀνέμος νότος, and the Vulgate *ventus urens*. In my opinion, which I express only as a conjecture, the *Khamseen* acted on the tide as the wind did on the Rhone, it stopped the current, I should say the ebb, and prevented the water from flowing. In the morning, the wind ceasing suddenly, the water took its level violently, and swept off everything which was on its way.

In the description given above of what to my mind seems to have been the route chosen by the Israelites, there is a point which I consider as very important: it shortens considerably the distances over which they had to travel. We have seen that it was quite possible that Pharaoh was at Bubastis when he received the visits of Moses and Aaron. The city of Rameses was in the Wady Tumulat, not far from Tell el-Kebir; Succoth was the district around Tell el-Maskhutah; and the place where they crossed the Red Sea was about fifteen or seventeen miles south of Succoth. The whole covers a space which in width was not more than forty miles. I consider the distance to be one of the chief objections to the place proposed by Prof. Ebers for the crossing of the sea, immediately north of Suez. Besides, this opinion is open to the same objection as the place advocated by Sir William Dawson, the southern part of the Bitter Lakes, viz., the Israelites would have had to pass over the ridge of Gebel Geneffe, of no inconsiderable height, and no easy access. In travelling by railway from Ismailia to Suez, before reaching the Bitter Lakes, the way seems entirely closed by the Gebel Geneffe and its highest summit the Peak Chebrewet. The

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\* Dillmann, "Exodus," p. 150.

† Linant, l.l., p. 207.

Israelites in their flight towards the south would have had either to climb over the mountains, or to follow a narrow track, if there was any, between the sea and the mountains, where they would have been easily destroyed.\* Josephus alludes twice to the fact that they had in front steep mountains projecting into the sea, and that they were shut up between the sea and mountains. Seen from a distance of a few miles, the mountains would produce on the Israelites the same effect as they do now on travellers; they would appear as entirely barring the way, even if there was an open path along the sea, which is doubtful, and it explains the despair of the Israelites described by Scripture and Josephus.

The site which I assign for the passage of the Red Sea agrees with the views of Linant, Lieblein, and Lesseps. These three authorities admit that the passage took place north of the Bitter Lakes, in the space which divides them from Lake Timsah, not far from the present Serapeum. I believe this is in accordance not only with the monuments, but also with the aspect of the country; and I advise the numerous travellers who go through that region, so full of glorious remembrances, to look at it in that light.†

The Right Rev. Bishop STALEY, D.D.—I have the pleasure given me to-night of asking you to express your obligation to the powerful writer of the Address, whose absence we all regret, and also to all the authors of the Papers read this session. Dr. Naville's is a remarkable Paper, in that it seems to lessen so completely the difficulties which the author himself feels cannot be entirely eluded in the story which we have, without any elucidation, given in its naked form, in the book of Exodus. He endeavours to show from evidence which I think is irrefutable, that the relative position of Pharaoh and the Israelites was somewhat different from that asserted by some previous writers who have endeavoured to illustrate the subject, and thus deprives it of certain difficulties which have been put forward. It seems clear that the wind and sea were instruments in God's providence for accomplishing the

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\* Linant, l.l., p. 205.

† "Journal of the Victoria Institute," Vol. xxi., p. 21.

deliverance; operating together on the side of the Israelites. This does not in the slightest particular lessen our faith in the supernatural, but it raises us to a higher platform, if I may say so, in our contemplation of natural laws. God is pleased for His purposes to use natural laws as He wills, and as seemeth to Him good, in the accomplishment of His ends. It has been a great treat to me to be present to-night, and to hear this most interesting Paper. I have followed for years past the work of the Institute, though it has not been my privilege, owing to my residence being so far away from the metropolis, often to be present at its meetings. It was founded through the instrumentality of some who have been gathered to their rest. One of these was a Cambridge friend of mine, the Rev. Walter Mitchell—a man of thorough scientific bent and culture himself. I may say an original investigator in one province, viz., that of crystallography, and I remember the deep interest which he felt in this Society. I trust it will continue to prosper. (Cheers.)

Sir JOHN COODE, K.C.M.G. (the late).—I, like the Bishop who has preceded me, feel honoured in being called upon to second the resolution “That our best thanks be presented to Doctor Naville for the Annual Address now delivered, to Dr. Wright for reading it, and to those who have given Papers during the Session.” Perhaps without simply contenting myself with barely seconding the resolution, I may be permitted to say a word or two.

In 1884 I happened to be nominated by Her Majesty’s Government as one of the representatives of Great Britain to inquire into the question of the enlargement of the Suez Canal, that is to say, whether there should be an enlargement of the Canal or whether there should be an entirely separate canal from the Isthmus. At that time Professor Hull, who is referred to in this Paper, had written an essay on this very subject, and had given expression to the opinion that the Red Sea, at the time of the crossing by the Israelites, did extend very much further to the north than in our day. My mind was very full of that subject, because I had not very long before been to Suez and been told, like all other travellers at that time, that it was just about south of Suez where the Israelites crossed the Red Sea. That, I must say, was rather difficult to believe. I was prepared to believe in miracles, but when Professor Hull’s Paper was brought out indicating that the scripture narrative would be made much clearer

to us if it were found that the passage of the Israelites through the Red Sea was nearer to Ismailia and to Rameses than was previously supposed, a great point was gained. I was very anxious before we reported on the subject of the enlargement of the Canal that we should clearly ascertain what was the nature of the strata that would have to be dealt with and passed through. In the district referred to near the Bitter Lakes, I went on one occasion up the slope of the Canal and saw something rather striking at a level of about 12 or 15 feet above the level of the Canal, which, for all practical purposes, was within an inch or so of the level of the sea. I saw something attracting attention and I took a pick out of a workman's hand and struck it into the earth and then I dug up this very sample which I hold in my hand. At first sight I thought it was gypsum, and so I said, but standing by my side was an Italian gentleman, who had been one of the engineers employed on that section of the Canal. I said, "This is gypsum." "Oh no," he said, "it is salt." "What makes you say salt?" said I. "Why," he said, "the whole district here is covered with salt." Now here I will hand to our President, Sir George Stokes, that very piece which I dug up, and I think by its taste and form of crystals, which are cubical, he will be much inclined to say that it is salt? [The President assented.] Now, it being admitted that it is salt, here we have the fact that at some time or other this land must have been submerged—or the sea has receded. I think this piece of salt is a proof of the truth of Professor Hull's theory, and it is also a proof of the accuracy of the writer of this Paper when he refers to the slow rising of the ground which at a later time cut off Lake Timsah from the Bitter Lakes. If this specimen does not prove that at this time this land must have been under the influence of the sea at a level of 12 feet, I do not know what proof is. (Cheers.) I have great pleasure in seconding the resolution.

The resolution was then carried unanimously.

Rev. W. WRIGHT, D.D.—Whilst thanking you for the kind mention of my name in the vote of thanks, I do not feel that I deserve any thanks whatever. It has been to me a great pleasure to have read this Paper to-night for my valued friend Dr. Naville. I have studied the country he describes, and while not expressing any opinion as to several minor suggestions towards the end of the Paper, I agree with its substance throughout. In regard to this crossing of the Red Sea I think we sometimes hamper ourselves

with our definition of a miracle. When we say it is a result brought about by something contrary to the laws of nature, we give the infidel his argument. (Hear, hear.) For me to say a miracle is something contrary to the laws of nature implies that I know what the laws of nature are. Now I do not think I can say so. God may use and does use His laws as He wills, and in doing so He may for anything I know be acting according to the laws of nature. When we speak of a miracle as something "so far removed from common experience as to show the hand of God especially at work among His own forces," we are on safer ground.

Mr. ALEXANDER McARTHUR, M.P.—I have to move a resolution which I am sure will meet with hearty approval. It is that the thanks of the meeting be presented to the President. Considering the great and important duties he has to perform, our indebtedness to him for his constant care of our interests calls for very special acknowledgment. (Cheers.)

Sir THEODORE FORD.—I have great pleasure in seconding the resolution which has just been proposed. I entirely concur in what the last speaker has said, but at this late hour, I am desirous of saying as little as possible, and I can only trust that the shortness of the language I use will not convey the idea that the thanks of this meeting should be less hearty than they ought to be. (Cheers.)

The resolution having been passed,

The PRESIDENT.—I rise to return my sincere thanks for the honour you have done me in passing this resolution. I confess I have not been as active a President as I could have wished, because, as mentioned, my time is so very much occupied by many duties both in London and at Cambridge, but I need not say that among the duties which afford me great pleasure are those in connection with the Victoria Institute.

[The Members, Associates, and their guests then adjourned to the Museum, where refreshments were served.]



## ORDINARY MEETING.\*

THE PRESIDENT, SIR G. G. STOKES, BART., LL.D., Sc.D.,  
IN THE CHAIR.

The Minutes of the last Meeting were read and confirmed, and the following Elections were announced :—

**LIFE MEMBERS:**—The Rt. Hon. Lord Masham ; Sir S. Shippard, G.C.M.G., S. Africa ; The Very Rev. T. W. Jex-Blake, Dean of Wells.

**MEMBERS:**—The Rt. Hon. Lord Lilford, Oundle ; The Rt. Rev. Dr. Stanton, Bishop of Newcastle, N.S.W. ; Rev. Alex. Alison, D.D., U.S.A. ; The Rev. Professor J. H. Bernard, B.D., Dublin Univ. ; T. Hunter Boyd, Esq., London ; The Rev. A. Carr, M.A., Berks ; J. Cory, Esq., J.P., Wales ; Rev. C. M. Cobern, U.S.A. ; T. H. Davies, Esq., Lancashire ; W. W. Felts, Esq., U.S.A. ; A. Wilford Hall, Esq., LL.D., Ph.D., U.S.A. ; Rev. Canon E. Huntingford, D.C.L., Hampshire ; W. H. Hudleston, Esq., F.R.S., &c., London ; Miss G. L. Leonard, U.S.A. ; Lt. H. G. Lyons, R.E., F.G.S., Egypt ; Rev. J. E. Rogers, Kent.

**LIFE ASSOCIATE:**—Rev. E. Atkinson, D.D., Master Clare Coll., Cambridge.

**ASSOCIATES:**—Rt. Rev. Bishop C. H. Fowler, D.D., U.S.A. ; Rt. Rev. Bishop C. G. Barlow, D.D., N. Queensland ; Kali Charan Banurji, Esq., M.A., B.L., India ; Allan McDougall Clark, Esq., India ; G. W. Cuming, Esq., Kent ; J. Leyland Feilden, Esq., Sussex ; Sir Theodore Ford, C.M.G., London ; T. W. E. Higgs, Esq., A.M.I.C.E., London ; Colonel W. R. Lewis, R.A., Sussex ; Colonel P. D. Marrett, R.A., Guernsey ; Captain H. A. Lowe, London ; R. Little, Esq., Cheshire ; Professor H. Langhorne Orchard, M.A., B.Sc., London ; H. Oldham, Esq., Oldham ; J. H. Phillips, Esq., Yorks ; E. J. Reddie, Esq., Yorks ; A. T. Schofield, Esq., M.D., London ; General W. Stirling, C.B., Kent ; R. Ashley Smith, Esq., M.D., Staffordshire ; The Hon. C. Abercrombie Smith, M.A., Fell. St. Peter's C., S. Africa ; W. Williams, Esq., C.E., Somerset ; R. Huyshe Walkey, Esq., Warwickshire ; Rev. Professor Canon J. D. O'Meara, Canada ; Rev. Professor A. Baumgartner, Switzerland ; Rev. A. W. Chapman, B.D., Ireland ; Rev. A. K. Cherrill, London ; Rev. J. Duncan Craig, D.D., Ireland ; Rev. M. J. Cramer, A.M., D.D., U.S.A. ; Rev. B. Davis, B.A., India ; Rev. J. Eckersley, M.A., Staffordshire ; Rev. H. J.

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\* Jan. 4, 1892, since which date the notes of the Proceedings have been carefully revised by those who took part therein.

Spencer Gray, M.A., India; Rev. J. R. Longley Hall, Palestine; Rev. R. C. Hallows, Ireland; Rev. J. W. Hodgkinson, Glasgow; Rev. J. Hudson, M.A., Chillingham; Rev. Canon Courtenay Moore, Ireland; Rev. S. D. McConnell, M.A., D.D., U.S.A.; Rev. G. H. Rouse, LL.B., India; Rev. J. Redman, India; Rev. T. L. F. Stack, B.D., Ireland; Rev. R. Shann, M.A., Camb., Hertfordshire; Rev. E. J. Tyser, M.A., Yorks; Rev. W. Wace, M.A., Camb., India; Rev. A. B. W. Whatton, M.A., London; Rev. J. Wilkie, India; Rev. E. W. Wilson, London; Mrs. E. M. Hutchinson, Surrey; St. John's, New Brunswick Public Library, Canada.

Also the presentation of the following works to the Library:—

"Proceedings of the Royal Society"	<i>From the same.</i>
"Proceedings of the Royal Asiatic Society"	"
"Proceedings of the Royal Dublin Society"	"
"Proceedings of the Royal Geographical Society"	"
"Proceedings of the Royal Colonial Institute"	"
"Proceedings of the Royal Institution"	"
"Proceedings of the Royal Irish Academy"	"
"Proceedings of the Royal United Service Institution"	"
"Proceedings of the Royal Society of Canada"	"
"Proceedings of the Royal Society of New South Wales"	"
"Proceedings of the Geological Society"	"
"Proceedings of the Society of Arts"	"
Also the Proceedings of many Colonial and Foreign Societies.	

The following Paper was then read by the Author:—

*FROM REFLEX ACTION TO VOLITION.* By Dr. ALEX HILL, Master of Downing College, Cambridge.

**A**N anatomist as such has no affair with the analysis of mental processes. My professional work consists in attempts to understand the structural relations of the several parts of the central nervous system; to give, what is very far from complete as yet, an account of the mechanism by which thought is produced; not to explain its mode of working. Still it is impossible for any one who is constantly occupied in the study of the Brain to avoid speculating as to the way in which it does its work. Feeling, emotion, thought, all the manifold phenomena of mind-life are but the products of the brain's activity; how far does a study of the apparatus in which they are produced help us to classify these phenomena? How far does a subjective analysis of the phenomena help us to understand the Construction of the

Machine? Wide as is the gap between the tissue which lies upon the stage of the microscope and the functions of which, when alive, that tissue was the seat, the anatomist who is constantly occupied in the study of the nervous system cannot refrain from attempting to answer the question: "How does it do its work?" Fastening facts as steps upon the face of the knowable, we attempt to climb as far as they will carry us in order that we may take a wider survey of the unknowable. No two thinkers assign to their data the same value, or use them in the same way; nor have we any means of deciding who has climbed the highest or sees the farthest. The views which I wish to lay before you to-night are merely the reflections of an anatomist as to the functions of the tissue at which he works, not the analysis by the psychologist or metaphysician of the phenomena of mental action. Nevertheless, this paper must to a certain extent treat of its subject from both sides. Looking up from the microscope the anatomist asks himself: "What is the work which the brain has to do?" Attempting to picture it in action he turns from the fabric to the machine, from the machine to the fabric, in his efforts to realise the relation between the two.

May I use this illustration of the fabric and its manufacture, to explain the objects of this paper? Let us imagine that we find ourselves placed before a curtain-making machine, and its wonderfully intricate product a lace curtain, and that we try by contemplating the curtain on the one hand, and the machine on the other, to realise the machine in action, the curtain in process of formation. Look first at the curtain, a labyrinth of threads, crossing and recrossing, looping and knotting, in apparent confusion, but each thread holding its proper place in the pattern, taking part in the realization of a design, which when seen as a whole, conveys what seems to be a simple idea, a picture it may be of fruit or flowers. The machine in which this intricate texture is woven must, to some extent, resemble its product in complexity and delicacy; and it is not without an object that I choose the lace curtain for my illustration. It suggests the fabric, the formation of which we wish to investigate. The thought which—preceding volition—flashes, as we say, into the mind, presenting to our consciousness what we take to be an integral idea, is like the curtain woven of many threads, the ends of which reach far backwards in our lives. It is made up of the experiences of a lifetime, and each experience of countless filaments of sensation.

How dull a paper this promises to be is the half-formed thought of most minds in this room! But what a complex product of experience is this simple thought: experience of other papers, judgment of character, recollections of efforts made to struggle with metaphysical subjects.

The metaphysical side of the problem is the aspect which I wish to avoid as far as possible. It is the thoughts of an anatomist that I have to offer. I must therefore ask you to follow me in tracing the evolution of the nervous system from the beginning. Which of all the mass of facts accumulated by the patient research for the most part of very recent years shall we select as steps on which to climb? First we must ask what is the ground from which we start—where in the animal kingdom does the nervous system first appear? There are animals which have none; but it is not possible to trace its genesis as clearly as we could wish. Some points, however, are absolutely clear. It is certain, for instance, that in all animals the whole nervous system is formed from the layer which, in the rest of its extent, becomes the skin. It belongs to the surface of the body, the layer, that is to say, which alone is exposed to the impact of impressions from the outer world. It is also clear that before a nervous system is formed, certain regions of the surface are, owing to their favourable situation, marked off as outposts for observation, as sense-organs. From the epithelial cells of these sense-organs, thin processes, or nerves, are prolonged to the muscle fibres contraction of which enables the animal to make the appropriate movement for escaping the danger, or seizing the food, of the presence of which the sense organ conveyed information. It is also clear that the nervous system, properly so called, is first formed by the deposition from the surface of certain of these sensory cells, the union of whose processes into a network or plexus, provides for the transmission of the impulses received by their more fortunate sisters not only to the muscle fibres, with which the cells on the surface may be supposed to have been in primitive connection, but also with a variety of muscles, the contraction of each of which is appropriate to the particular *kind* of impression received. Examine now the way in which this primitive nervous system works. It may be said in the jelly-fish to have reached the stage of evolution just described, and no animal (as Dr. Romanes was the first to recognize) is more accessible to experiment. It is not necessary for the purposes of this paper to follow Dr. Romanes and others through the

elaborate investigations which they have made into the working of the nervous system in this animal. We have probably all of us made such observations as are necessary to determine its fundamental properties. Watch the jelly-fish as it feeds on a rocky coast. The rhythmic pulsations of its pellucid bell bear it gently to and fro within a closely circumscribed area. At one moment the wave washes it close to the rock; at the next it has turned on to its side and a few pulsations carry it safely out of danger. However difficult it may be to compare its sensations with our own, they are undoubtedly of such a nature as to give it warning of danger, or rather to produce the movements by which the danger is escaped. What a contrast there is between the beautiful sentient and motile organism at work in the sea and the helpless lump of jelly cast up on the sand. As long, however, as it has life the function by which its movements are determined can be shown to exist. Stroke the stranded jelly-fish with your finger, and if it still lives each stroke is answered by an attempted contraction of the bell, a movement which follows the stimulus with unvarying regularity, after an easily-determined interval of time. The only evidence which we can obtain of the functions of the nervous system in the jelly-fish, relates to this unerring mechanical response to stimulus; Reflex Action.

To reflex action there are progressively added other functions, of the nature of which we can only form an imperfect picture by a process of subjective analysis, but the existence of which we infer from their effects in the exhibition of what we may term without staying to define or limit the expression "the power of choice."

Compare the frog with the jelly fish. The healthy, uninjured frog displays in its behaviour evidences of a power of selecting its actions. Frighten it and it may jump away, or it may under apparently exactly similar conditions refuse to move. Remove the brain of the frog (an operation which it bears with remarkable impunity), and carefully keep it moist and fed, and for the rest of its life, which may easily be prolonged for a year or eighteen months, we have in our hands a machine which responds infallibly to every stimulus, but never makes a move in the absence of an easily recognised provoking cause.

It is easy to show, however, that there are many actions over which the uninjured frog has no control—which are purely reflex—and my object is to trace the relation to one

another of actions which, in their pronounced forms, we distinguish as reflex and voluntary respectively.

Much light is thrown upon this subject by a study of the tissue, the "grey matter" of the central nervous system, in which these actions are carried out.

It belongs to two great divisions:—(1) The grey matter of the spinal cord and of the central canal of the brain; and (2) the grey matter which covers the surface of the brain. The former is the earlier or more primitive formation, the latter appears much later in the animal series, and only reaches a considerable development in its higher members. The structure of the two is however fundamentally similar. Each consists of a network into which afferent or sensory nerves pour their impulses, and out of which efferent or motor nerves take their start. The filaments of the network are the ramifying processes of cells. Most of the cells are small, but those which support motor nerve-fibres are large; the reason being, apparently, that the long nerve fibre, which runs perhaps to a distant muscle, derives its nourishment from the cell out of which it grows. Time would fail me to give an account of the constitution of this network, nor am I for another reason sorry to pass it by with a hasty notice, for it happens that during the last year or two entirely new views as to its constitution in detail have been advanced, and are still *sub judice*.

The only marked difference between the grey matter of the brain and that of the spinal cord which we can point out is that whereas the spinal cord receives sensory nerves and gives origin to motor ones, the cortex of the brain is, so far as we know, connected with the periphery only through the mediation of the central grey matter, or plexus of the spinal cord.

These are the only points with regard to the evolution and anatomy of the nervous system of which I wish to make use for the purposes of my argument. 1st. It is formed from the surface of the body. 2nd. It is in the first instance intimately connected with the sense organs. 3rd. That it consists of a plexus by means of which the cells of the sense organs are placed in connection with muscle fibres. 4th. That in the lowest animals it is the function of this network to transfer sensory impressions to the appropriate motor mechanism. 5th. That in higher animals the direct and inevitable transfer of impulses gives way to a greater or less extent to the power of choice.

In vertebrate animals, a second field of plexus, the cortex of the brain, is superadded to the primitive grey matter of the spinal cord. Several questions must however be answered before we can correlate this with any distinct advance in the performance of the nervous apparatus. We do not know whether the elements contained in this cortical formation are already present in invertebrate animals; nor do we know what relation the great masses at the base of the cerebrum, the corpora striata (which constitute the bulk of the birds' brain), bear to the mantle or cortex proper, which does not really make its appearance amongst vertebrates until the cartilaginous fishes are reached. Much as we should like to regard the first appearance of the cortex as a great step in advance in the evolution of the central nervous system, we must at present refrain from drawing wide conclusions, and merely note the fact that the nervous system of higher animals is divisible into two parts; that it consists of a vast aggregation of elements which, so far as we can see, are not marked by any differences in character or even in arrangement. The fibres which connect the inner grey matter with the surface of the body, pour their impulses into a network from which motor fibres take origin. The grey matter presents therefore a variety of routes through which sensory impulses may flow over into motor paths, each sensory-motor path constituting an arc.

But on the arcs which collectively make up the lower system are superadded arcs, the loops of which lie in the higher grey matter. At the same time, therefore, that an impulse flows across the spinal cord, as a simple direct reflex action, a certain part of this impulse is also diverted to the brain along fibres which ascend in the outer part of the spinal cord; and from the brain descending fibres carry the impulse back again to the lower arc. Let us try to avoid expressions which convey a more concrete idea of the relations of these two sets of arcs to one another than we are at present justified in formulating; and above all let us avoid terms derived from commerce, military organizations, and other developments of social activity which would be but coarse symbols to apply to this relation, of the exact nature of which we are able to form but a dim picture, and let us say that we see in the superposed arcs of which there may be, for aught we know, not two only, but many, opportunities for the control, the reinforcement, the restraint of reflex action. Accurate measurements of the time taken by impulses in travelling through

the grey matter have done much to throw light upon the route they follow; but we do not yet know whether we ought to speak of the conversion of a sensory into a motor impulse, as its passage through the lower network under the direction of nerve currents which originate in the higher; or whether the impulse when it reaches the lower grey matter takes in some cases a direct cross path, while in others it makes its transit through a longer loop. One thing is quite certain, namely, that the routes which are most frequently used are the most open, and therefore the most easily traversed.

Of all the impulses reflected through the grey matter, the most frequent are those upon which the position of the body depends. As we stand upright the body sways ceaselessly backwards and forwards. Swaying backwards, muscles on the front of the thigh are stretched. The stretching gives rise to an impulse which travels up to the spinal cord and down again to the muscles leading to their contraction. As our weight falls forwards the muscles of the calf are stretched, contract, and restore the body to the erect position. These adaptive movements are made unconsciously many times in every minute. The reflex route is always open. Afferent and efferent nerves are, as it were, constantly "switched on" through the grey matter. The flexor and extensor muscles which play over every joint, bear this seesawing relation to one another. But peculiar interest attaches to the contraction of the extensor muscles of the thigh, for the reflex action by which this is brought about can be very easily demonstrated in almost all healthy people, and can be used as a very sensitive test of the condition of the spinal cord when this is diseased. A person sits in an ordinary upright chair, with one leg crossed over the other, the muscles lax, and the foot hanging free. If then the tendon below the knee-cap is gently tapped, the slight sudden stretching of the muscles of the thigh so produced, leads to their contraction and the sudden jerking forward of the foot. This "knee-jerk," as it is termed, occurs with such rapidity, that physiologists have had difficulty in understanding how it could really be a reflex action. The jerk follows the tap in .03 second. If, therefore, we deduct the time taken by the impulse in ascending to the spinal cord and descending to the muscle, we find that the passage of the grey matter occupies no more than .01 to .015 second. This is only a type of a large number of similar adaptive movements, but I dwell



upon it particularly because it is a pure reflex allying us to the jelly fish, and sweeping away, if properly understood, several fancied distinctions between the mode of action of the nervous system in ourselves as compared with lower animals. It is a pure reflex. It is carried out in the complete absence of consciousness. The movement has no representation in consciousness. It occurs with equal readiness in waking and sleeping, in persons upon whom it has often been tried before, and in those who have no idea of the result likely to follow upon the tap.

Let us use the knee-jerk as the starting point for an analysis of action. The knee-jerk is an unconscious reflex. From this, as zero, in the scale of consciousness, it is easy to construct a table of actions in which the power of control plays an increasingly important part, although all rest equally upon a reflex basis. The foot when trodden upon is inevitably withdrawn. In a choleric person the impulse travels along longer routes, overflowing perhaps into violent action which is equally irresistible. It is easy to picture to oneself the paths in the network as lines of varying resistance, and to imagine the nerve current as choosing the route which offers least opposition to its flow. In its first origin the nervous system is like an open moor, equally easy and equally difficult of passage in all directions. The nervous system as we inherit it is a labyrinth of paths, the depth and breadth of each of which is a measure of the number of impulses which have in our ancestors', as well as in our own lives, passed that way. Practice means the beating down of paths. The facility which comes with practice depends upon the ease with which impulses pass, and this is true not only of simple and obvious reflex actions, but also of such movements as often appear at first sight voluntary rather than reflex; the deftness with which an engraver reproduces a picture on his box-wood block; the astonishing rapidity with which the musician translates certain black spots upon the paper into movements of the fingers; the naturalness with which a cultivated and kindly man answers the needs of his friend with sympathetic glance and courteous action. There is no epoch in life's history at which path-making ends. And here, although not necessary for my argument, I must assert my belief that the nerve network inherited by the individual is a labyrinth of paths which his ancestors have beaten down. Training and circumstance modify the ancestral pattern, so that the network transmitted to our offspring has the form which choice, self-

control, and self-development, whether upward or downward in the ethical scale, have impressed upon it. This is a large question, which would carry me far beyond the province of this paper. At present evolutionists are divided into two camps. Some hold to the older doctrine that under the influence of the environment the organism acquires and transmits tendencies favourable for its existence, while others form the newer school, headed by Weismann, who believes that acquired characteristics are not transmitted, but that the "chance" differences between a number of individuals born under the same conditions tend, when favourable, to improve the individual's prospects in life, while unfavourable modifications diminish the likelihood of his holding his own in the struggle for existence. This doctrine does not carry my judgment with it; it throws back the variations to chance as an ultimate cause. Chance! an expression which in science can only stand for a cause not yet discovered. Weismann proceeds a step further in his reasoning, and draws a wide distinction between the reproductive cells and the rest of the body in which they happen to reside. The body at large, he says, plays no part in determining the structure of the offspring, and therefore no characters which it may acquire can be transmitted. There is no logical difficulty in looking at the central nervous system from this point of view, but it appears to me that observation of human nature demonstrates beyond a doubt that mental characteristics, peculiarities in the form of the network which have been acquired by individual occupation and effort, are handed on to offspring. Theories of evolution are based as a rule upon the study of external form, but it is undeniably true that if the disposition acquired during life by the least of the strands of this inconceivably complicated network is transmitted to a descendant, Weismann's theory falls to the ground.

Hypotheses with regard to its origin aside, the network undoubtedly exists. It is first formed in connection with sense organs, and probably never presents what can be properly called a motor or "kinetogenetic" part. Its function is the distribution of the sensory impulses which it receives into their appropriate nerve fibres, which have their starting points in the larger motor cells.

Up to the present we have spoken of the transmission of impulses as if it always occurred only after such delay as was necessary for the transit through the nerve network. Another step in evolution must now be recognized. It is impossible

to say at what level in the animal kingdom this advance occurs, although it is easy to point out animals in which MEMORY is clearly present, and others like the jelly fish in which such a function, if it exist, is hardly to be shown. In the great majority of animals, although in varying degree, the passage of the impulse is accompanied by molecular changes, which result in its being stored. It is not merely reflected as in a glass, but fixed as in a photograph. Infinite possibilities are opened up by this capacity of the system for storing impulses; possibilities of the combination and comparison of the impulse received from the outer world and about to be discharged in movement, not only with other impressions concurrently received, but also with all the accumulated experiences of the past. Reverting to our former illustration of the effects produced when one person treads upon another person's toe, we see how the impulse may not only flow over into the muscles which withdraw the foot, or return it in a kick, but may be combined with impressions received through the eye which awaken memories of former accidents of the same kind, and the troubles which resulted from a hasty resentment, so that the movement made is not the reflection of one impulse only, but of several; nor of current impulses only, but of these combined with others long ago received. Now we begin to feel as if we had reached the outskirts of thought. If we could watch the machinery in motion, we should see not the shuttle flying backwards and forwards, weaving, twisting, intertwining threads, but nerve currents starting in different, perhaps distant parts of the central system, hastening, checking, combining, departing, to form the pattern which we recognize as a thought.

My survey of the subject must be confusingly hasty, but there is one other feature of the network, a consideration of which throws a brilliant light upon its mode of working. Hitherto we have spoken of reflex action only. This is the web upon which is woven the many-coloured fabric of intelligence and emotion. From what peculiarities in its quality and amount do the less easily defined phenomena of mental action acquire their colour? The cortex of the brain is, we know, the seat of these higher processes. But twenty years ago it was thought that although the elements of which the cortex is composed are simple, although there are no local peculiarities in their arrangement, although the cortex is in a word remarkably uniform in constitution, its mode of action must be complicated to a degree which baffled investigation.

Now we know that it, like the spinal cord, is a network which receives afferent or sensory fibres, and gives origin (from large cells which look after their nutrition) to efferent or motor ones. A vast aggregation of sensory-motor arcs. Recent investigations have further shown that the fibres which reach the cortex do not come promiscuously from all parts of the lower network, but that it is divided into areas, each of which is in connection, through its lower centre in the spinal cord and the rest of the central grey matter, with a particular sense organ. We owe this discovery entirely to experiment, but the information obtained by stimulating or excising particular regions of the brain can be checked by appeal to other classes of evidence.

Among animals we can easily pick out certain ones whose sensory endowments differ conspicuously from those possessed by most other members of their group. For example, aquatic mammals are deficient in the sense of smell; whether it be because the mammalian organs of olfaction are only adapted for the recognition of bodies suspended in a gaseous medium and cannot be used for the recognition of substances in solution; or whether it be that the respiratory apparatus is closed when the animal is under water, and so there is no opportunity for the renewal of the fluid which fills the nostril, it is a notable fact that however far the groups to which particular aquatic animals respectively belong are removed from each other, the sense of smell is in abeyance in every case. Whales, dugongs, seals, &c., resemble one another in having but little sense of smell or none at all. And with the dwindling of the sense of smell is associated deficiency of the region of the brain known as the temporo-sphenoidal lobe. Dogs and other cursorial carnivora on the contrary possess this sense highly developed, and show a corresponding development of this region of the brain. The herbivora are remarkable for their acuteness of vision, with which is associated a fulness of the occipital lobe. Cats and other crouching carnivora hunt largely by the sense of hearing. Otters find their way among the snags and roots which overhang the dark pools in which they seek their prey, with the aid of the sensitive bristles of the cheek. This list might be almost indefinitely multiplied, but we find from the result of observations upon the brains of animals, as well as from the results of experiment and the observations of disease, that the cortex of the brain is mapped out into areas of separate occupation. There is no division into regions allocated to

separate mental faculties; but we have reason to think, although we have not yet succeeded in accounting for a small portion of the front of the brain, that the whole of the cortex is occupied in the reception and elaboration of sensations.

This brings us a step nearer to understanding the mode of working of the brain. The network of which it consists belongs to the several senses. The kind of work which it does must depend upon the amount of territory in which each sense is represented.

Hitherto we have described the organism as if it were simply a machine for transforming external force into movement. Our actions, our thoughts, our characters are the products of certain forces transmitted, some at once, and others after lying latent, through a network which happens to have a certain inherited form modified by the circumstances in which our lots are cast. Thus is undoubtedly formed the substratum of mental action, the machine with which the mind works. But there is a danger of dwelling upon this aspect of the problem to the exclusion of other phenomena which are equally demonstrable, but which can only be studied by different methods of research. To be perfectly candid I must admit that it was from this *other* side that I wished especially to treat the subject in this paper, but I found that even a hasty statement of the data with which I wished to work, required all the time which I could demand, and more than I ought to ask, of your patience. I must be content with indicating in a few sentences the lines along which my own thought travels in attempting to reach from these data conclusions with regard to the subjects which must ever carry the greatest human interest.

In the description of the principles of construction of the thought-producing apparatus which I have submitted to you, I have carefully avoided the use of any term which implies a knowledge of the processes which go on within ourselves. I have spoken of reflex action, of memory, but not of consciousness. Consciousness is a by-phenomenon which accompanies the reception and transmission of sensory impressions. It cannot be imagined as preceding sensation, it accompanies it. In the evolution of the animal kingdom, it makes its appearance at some point which we can never determine, for we can only judge of its existence in animals by its effects. It cannot be defined, for we can only express it in terms of itself, or in descriptions of the circumstances under which it

is manifest. All that is or ever can be known about it must be deduced from introspection. It is impossible to suppose (and this is really the consideration which removes it altogether from the sphere of the phenomena which we have been studying) that any of the force received by a sense-organ from the outer world, and which travels up sensory nerves into the nerve network, is used up in its production. It is impossible to imagine a balance sheet of force which shall include on the debit side the forces which act upon our bodies from without *plus* the force generated by metabolism within the body and on the credit side:—to the production of consciousness *so much!* The anatomist must let consciousness alone.

But just as movement presupposes sensation so consciousness predicates choice.

It is necessary to be very modest in asserting of a given action that it is a voluntary one. All actions may be performed in the absence of volition, but when there is consciousness of sensation there is also a power of selecting sensations, and therefore of determining the combination of nerve-currents which shall flow out in action. [Considerable applause followed the conclusion of the paper.]

The President (Sir GEORGE G. STOKES, Bart., M.P., V.P.R.S.).—You have already anticipated me in returning your thanks, by your applause, to Dr. Hill for this very interesting and suggestive paper on a most difficult and mysterious subject. I will now invite remarks especially from those who have given particular attention to this subject or to subjects allied to it. Perhaps Sir Joseph Fayrer will open the discussion.

Sir JOSEPH FAYRER, K.C.S.I., M.D., F.R.S.—I think, sir, that there may be others present who would be better able to speak upon this subject than I am; but as you have called upon me I will make one or two remarks—not in the way of criticism, for there is nothing to criticise.

First, I wish to express my admiration of the paper. Dr. Hill has brought before us an exceedingly difficult, and, as you have said, mysterious question, and has done so with judgment and

discretion. The whole of the vast subject of the nervous system he has dealt with in this short paper. He has taken it from a low form of life beginning with the Medusa, but he might have gone even lower, for though in them you find no threads or filaments to indicate a nervous system, there is other evidence of its existence. Dr. Romanes and Dr. Hill himself have shown that first demonstrable evidence of a nervous system. From that rudimentary condition in the lowest organised creature you progress to the brain that wrote the plays of Shakespeare, or that discovered the laws of gravitation. There is remarkable continuity and evolution. As to the brain, if you put a jelly fish at one end and Sir Isaac Newton at the other, the transition is gradual from the one standard of intelligence to the other.

There is much in the paper that is very interesting, and, as the President has said, very suggestive; and where the author speaks of heredity I am very much struck with the value of his remarks. I cannot conceive how any one could have advanced the theory that these things are not hereditary; that they are so transmitted there can be little doubt. I was in the north of Scotland shooting, and in the course of my walks found a dead grouse lying under the telegraph wires, and drew the keeper's attention to it: in the course of conversation he said: "Where we used to get ten of these dead birds, we now get one." I asked him why that was, and he said: "They have got to know it—the knowledge seems to have been transmitted to them. Formerly they used to fly against the wires and kill themselves, while now they avoid them."

The author has spoken of the origin of sensation, and has referred to the otter, the seal, the whale, and so on, the latter being destitute, I believe, of smell, and what would be the use of it? The creature keeps its head under water, and the sense of smell is not transmitted through the water—but the dog has large organs of smell—his mission seems to be perpetually in an atmosphere of it. If you watch a sky-terrier, or other dog, his whole time is occupied in the pursuit of his sense of smell. All this is very interesting, but Dr. Hill has not spoken of anything that we do not share in common (though we have it in a higher degree) with our lower neighbours.

He has not gone into the psychical aspect of the matter. I think if he were to do so it would be interesting, but I quite

sympathise with him in his treatment of the subject, and congratulate him on having done it in an admirable manner. I have great pleasure in endorsing the vote of thanks offered to Dr. Hill for his very interesting paper.

Mr. D. HOWARD, F.C.S.—I have pleasure in offering my thanks to Dr. Hill for his paper, and I hope it will be studied by those who do not know much of the subject, as well as by those who do, because it gives a clear idea of a very interesting and difficult subject, and I hope Dr. Hill will follow it up further with a paper such as Sir Joseph Fayrer has suggested. It is a great advantage to have some knowledge of anatomy, and it is the anatomy of the brain alone that can throw any light on the problems treated of in this paper.

I cannot imagine anything more unwise than to blame such investigations as tending to materialism: we might as well object that the study of the vibrations of a fiddle detracted from the skill of Joachim.

As to the knee-jerk that has been referred to, I can only say it is best tried by somebody who does not know the meaning of it. I have a vivid recollection of it in my boyish days, and it was irresistibly ludicrous, but the other reflex action of the toe is also likely to follow where it is practised on an unconscious school-boy.

Lt.-Colonel T. A. FREEMAN, M.A., Oxon.—“This doctrine does not carry my judgment with it; it throws back the variations to chance, as an ultimate cause. Chance! an expression which in science can only stand for a cause not yet discovered.” When I read this in the paper I was reminded of an exceedingly fine expression in Napier’s “Peninsular War.” Any who have read that work know how he constantly refers to fortune, and I have heard this objected to on religious grounds; but he ends by describing fortune as being “the unknown combinations of infinite power,” and he points out that fortune was against Napoleon; and because fortune was against him his immense power in the Peninsula was shattered. If you take that definition I think you will see at once what a magnificent idea it is. Now to pass to one other thing in the paper, as to reflex action, which was spoken of as the “knee-jerk.” It brought to my mind something that happened in India some years ago, when an officer was severely injured by a wild boar. He would certainly have died if he had not had a strong consti-



tution. He survived the injury, but for ever after he had a certain spot in one of his arms the touching of which instantly produced the most violent motion of his arm—it was perfectly uncontrollable and independent of his will at all. Of course that was an exceptional movement, whereas the knee-action is normal; but it was an example of action which was purely without volition.

Dr. ROBERT JONES (of Earlswood Asylum), a Visitor.—Mr. President, ladies and gentlemen: I am in charge of an Asylum where mentally afflicted children and imbeciles are received, and I was much struck with that portion of the paper in which Dr. Hill says that faculties, as such, have no special location on the surface of the brain. It struck me very much that from analogy, faculties should have a very distinct location.

Amongst our patients we have those who would strike you as being very peculiar from one special faculty being developed practically at the expense of all the rest—for instance, one knows Gibbon's "Decline and Fall of the Roman Empire" from cover to cover, and another, the History of Achilles to the Siege of Troy without much correction; but they are absolutely devoid of judgment. Another patient can transpose Bach's Fugues at sight, and is also absolutely devoid of judgment. Another patient can construct models. He went to see the *Great Eastern* steamship and came back and constructed an accurate model from memory. These persons would seem to have a portion of the brain abnormally developed. I speak with little authority from an anatomical point on this subject, for Dr. Hill has doubtless examined many portions of the brain, and perhaps he would give me a reason for his belief that faculties, as such, have no special *locus in quo* in the brain. I beg to thank Dr. Hill for his most able paper, which he has given us in very simple diction.

Dr. A. T. SCHOFIELD.—This being my first opportunity of coming here, and being one of the most recent members of the Institute, my ignorance, combined with my intense interest in the subject, must be my apology for rising. With your permission, sir, I would ask one or two questions connected with the paper that Dr. Hill has read. I may say how exceedingly pleased I was to find in his remarks that he established the fact that the paths most frequently used by impulses, become those that are most open and most easily traversed; and thereby endorsed, with all

the weight of his authority, the enormous power of habits, and the great value of training children in all parts of their character by the definite formation of habits. Dr. Hill does not go so far as to say that these impulses or habits actually form in the brain connecting fibres between cell and cell. I believe that is not demonstrated. I do not know whether he would say it is possible, if not probable. Then, further, with regard to impulses, Dr. Hill spoke of the lengthened or higher arcs:—"The grey matter presents therefore a variety of routes, through which sensory impulses may flow over into motor paths, each sensori-motor path constituting an arc." Is sensation the only origin he recognises for impulses? Is he bound by the statement in his paper, that movement presupposes sensation, or does he admit the idea of the presence of cells specially in the frontal regions, which can absolutely start motion in the body apart from all ascertained and demonstrable sensations received from any part of the body? Does he look upon the mind as simply controlling these lengthened arcs, or does he look on the higher nerve centres themselves as having power to initiate action? I would also ask him whether he considers, with regard to volition, that reflex or automatic action comes first in the history of development? He has begun with the jelly fish, but I would ask him if he regards the action of such individual separate cells as compose some of the amœbæ and others as purely reflex, or being, as far as he can judge, automatic? What one wants to get at is whether action, in its first initiation, is really thoroughly ascertained to be reflex in these elementary creatures, which are far lower in the scale than the jelly fish; and whether what we see under the microscope are reflex or automatic movements.

Lastly, as to fixing a locality for the different actions of the brain. Are we to understand that the brain acts as a whole, or that certain regions of it (not of course in any way resembling the "bumps" of phrenologists) are devoted to certain ideas and classes of thought, and may be actively engaged, other parts being at the time at rest? I am afraid my questions are rather crude, but I have great interest in the matter, and I should be glad to have them answered.

Rev. A. I. McCaul, M.A.—I should like to make one remark as to faculties having no location, for it is a point of some interest, and brings to my mind a passage in Aristotle, which I daresay is familiar to the author of the paper, in which he seems to urge that there are

certain portions of the mind, or soul, as he calls it, suited to various classes of knowledge. The passage I refer to is in the sixth book—(Ethics vi, 2).

“πρὸς γὰρ τὰ τῷ γένοι ἕτερα καὶ τῶν τῆς  
 ψυχῆς μορίων, ἕτερον τῷ γένοι τὸ πρὸς  
 ἑκάτερον πεφυκὸς, εἴπερ καθ’ ὁμοιότητά τινα  
 καὶ οἰκειότητα ἢ γνῶσις ὑπάρχει αὐτοῖς.”

In other words; that in reference to the objects of knowledge which are generically different, there are portions of the mind, also generically different, adapted to them.

The AUTHOR.—I must apologize, Mr. President, for not having begun at the commencement of this discussion to take notes of the questions which were asked me, since I did not expect so great a number.

While in the first place I am very grateful for the expressions of approbation which Sir Joseph Fayrer has bestowed upon the paper, I must nevertheless respectfully decline his invitation to follow up the question, from a philosophical standpoint, in a second paper. In this paper I have endeavoured to be as modest as possible, restricting my observations to those branches of the subject in which I have, as I think, information to offer, in the hope that it might serve the purpose of members of the Institute, who are far more competent than myself to apply these data to speculations of the kind which Sir Joseph Fayrer suggests.

Major Freeman’s reminiscences are very interesting as affording a striking illustration of the fact that while the greater number of actions are controlled by the will, actions may in certain cases be purely reflex. The power of control is an ever-increasing power, not only in the individual, but also in the race. Our actions however are, as I think, at bottom reflex, although the power of control is so greatly developed that we do not recognize their reflex basis. Many of the things which we do in the day appear to us to be pre-eminently voluntary actions, because we do not recognize that we did the same things under the same circumstances yesterday and the day before. We do not recognize that some sound heard or sight seen is the suggestion which leads to the liberation of nervous impulses which result in appropriate speech or act. No one who has observed the habits of old persons

whose minds are failing, owing to atrophy of the brain, can fail to have noticed the regularity with which given conditions produce given results. Actions which in a robust person would be judged as strictly voluntary, can be induced over and over again by placing the person in the same circumstances which have been observed to evoke them.

Dr. Jones, from his large experience at Earlswood Asylum, criticised my account of location in the cortex of the brain. I think he misunderstood me in the bearing that my remarks may have on phrenology; what I meant was that the faculties, as phrenologists classify them, are not to be localised as tabulated by them. As to the location of faculties in a much larger sense, about that I have nothing to say. I had merely pointed out that the study of the cortex of the brain drives us further and further back, and the more we know the more simple do we find its arrangement to be.

Flourens thought, twenty years ago, that the brain acted as a whole; but we know now that if you stimulate or exert one part of the brain you get a movement of the arm, another part governs the leg, and another part movement of the eye-balls; the whole has been mapped out into areas which are connected with the muscles and certain sense organs controlling their actions.

The instances that Dr. Jones gave of certain persons lacking common judgment and excelling in other things, recalling the form of the *Great Eastern*, for instance, and so on, do not need any special explanation, for it would be an absurdity to regard judgment as a "faculty" capable of localization in the brain. It is most probable on the other hand that the power of recalling the image of things which have been seen depends upon the relative development of certain portions of the brain.

I have been asked a certain number of questions, to answer which would require a much greater knowledge of Plato and Aristotle than I possess. I must therefore restrict myself to the anatomical and physiological sides of the question before us—I was asked, I think, whether the cells in the brain were capable of discharging "ideal impulses," I believe that was the expression. Now the curious thing about the study of the anatomy of the cortex is that the more we go into it, the more we are inclined to give up the notion that the cells have anything to do with the mental processes, except in so far as they serve for the connection

of filaments of the network and transmission of impulses. The function of the cells seems to be to look after the nutrition of the filaments. We cannot find any cell that has any such use as that suggested, viz. :—that it is a kind of little office in which an ideal impression is originated and from which it is discharged.

As to the distinction between reflex and automatic action, again I should say that research into the lower organisms indicates that reflex action is the phenomenon which we can study, and that automatism is a thing which physiology cannot understand. [Applause.]

The PRESIDENT.—I congratulate the Institute on having had a most interesting evening—interesting both as regards the paper as well as the discussion to which it has given rise, and I am sure you will agree with me that our thanks are due both for the paper and to those who have taken part in considering it. [Applause.]

The Meeting was then adjourned.

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## ORDINARY MEETING.\*

CAPTAIN FRANCIS W. H. PETRIE, F.G.S., HON. SEC., IN THE  
CHAIR.

In consequence of the sad news of the death of His Royal Highness the Duke of Clarence and Avondale, the Chairman moved the following resolutions :—

- a. That the members of the Victoria Institute tender most respectfully to the Queen, the expression of their deep sympathy with Her Majesty in Her present bereavement through the death of His Royal Highness the Duke of Clarence and Avondale.
- b. That they also tender to Their Royal Highnesses the Prince and Princess of Wales special sympathy in their present heavy affliction, which has caused such widespread and universally profound regret, especially the painful circumstances which have intensified a national calamity.

These resolutions having been seconded, were carried unanimously.

It was arranged that Addresses in conformity with these resolutions should be forwarded to Her Majesty the Queen, and to Their Royal Highnesses the Prince and Princess of Wales.

Mr. J. W. Slater, F.C.S., F.L.S. (the Author of the Paper to be read at this meeting) then moved : "That as a mark of respect to the Royal Family no other business be transacted."

This resolution having been passed, the meeting was then adjourned.

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\* Jan. 18, 1892.

## ORDINARY MEETING.\*

THE PRESIDENT, SIR GEORGE G. STOKES, BART., V.P.R.S.,  
IN THE CHAIR.

The minutes of the last meeting were read and confirmed, after which the following letters were read :—

“ *Whitehall,*  
*1st February, 1892.*”

‘ Sir,

“ I have had the honour to lay before the Queen the loyal and dutiful Resolution which has been adopted at a meeting of the Victoria Institute on the occasion of the death of His Royal Highness the Duke of Clarence and Avondale, K.G. ; and I have to inform you that Her Majesty was pleased to receive the Resolution very graciously.

“ I have the honour to be,

“ Sir,

“ Your obedient Servant,

“ HENRY MATTHEWS.”

“ *Windsor Castle,*  
*23rd January, 1892.*”

“ General Sir Dighton Probyn, Comptroller and Treasurer of the Household, is directed to convey to the members of the Victoria Institute, the best thanks of the Prince and Princess of Wales for the kind Resolution, condoling with Their Royal Highnesses in their great bereavement.”

The following elections were then announced :—

ASSOCIATES :—Rev. F. E. Hoskins, A.B., A.M., Syria ; J. Kennedy, Esq., B.C.S., M.R.A.S., London ; Jas. D. Logan, Esq., J.P., Africa ; W. Mackintosh, Esq., Morocco ; A. McGillewie, Esq., South Africa ; H. E. Millard, Esq., Vienna ; Warren Upham, Esq., Assistant Geologist of the United States Survey.

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\* Feb. 1, 1892.

The following works were presented to the Library :—

- “Natural Theology, being the Gifford Lectures for 1891.” By Sir George  
G. Stokes, Bart., V.P.R.S. *From the Author.*
- “The Hebrew Bible and Science.” By the Rev. W. C. Badger, M.A.  
*From the Author.*
- “Eclipses, Past and Future.” By Rev. S. J. Johnson, M.A. Oxon.  
F.R.A.S. *From the Author*
- “Hindu Literature.” E. A. Reed. ”
- “Life and Times of Joseph.” By Rev. H. G. Tomkins. ”
- “The London Quarterly Review.” *A. McArthur, Esq.*
- “Natural Theology and Modern Thought.” Rev. J. H. Kennedy, B.D.  
*From the Author.*
- “The Testimony of History to the Truth of Scripture.” By Rev.  
H. L. Hastings. *From the Author.*
- “Bulletins of the Museum of Comparative Zoology at Harvard College.”  
*From Professor Alexander Agassiz.*

The following Paper was then read by the Author :—

### THE WEAK SIDES OF NATURAL SELECTION.

By J. W. SLATER, Esq., F.C.S., F.E.S.

IT may seem, perhaps, strange that in these days any sober-minded naturalist, especially if a believer in Organic Evolution, should venture to call in question the theory of “Natural Selection.” Yet it may be worth while to look closely into this process and to ask whether it can really do all that has been so freely ascribed to it. I shall not attempt to describe or to define Natural Selection, since that task has been performed in several works which are easily accessible. I will merely say that it amounts to nearly the same thing, though seen from another point of view, as the “struggle for existence,” or the “survival of the fittest,” and that it is the very essence of that form of Evolutionism which is mainly due to Charles Darwin. Now I am by no means seeking to deny that creatures out of harmony with their surroundings are ill-calculated to survive. Still less can I doubt that there is a struggle for existence raging in the world around us. But I ask if this struggle is not more likely to blot out existing forms of life than to bring new forms into being, or to raise them to a higher stage of existence?



In the first place we find that among those who accept Natural Selection as the main agent in the Genesis of Species there have sprung up wide differences of opinion both as to its scope and its modes of operation. Whilst some naturalists regard it as the main, if not the sole factor in phyllogeny, Charles Darwin himself in his later writings owns that in the earlier editions of his "Origin of Species," he "probably attributed too much to the action of Natural Selection and the Survival of the Fittest." More decisive is the language of Dr. A. R. Wallace: "Natural Selection is not the all-powerful, all-sufficient and only cause of the development of organic forms." Candour, however, compels me to admit that Dr. Wallace now appears to have swung round to a belief in Natural Selection more sweeping than that at first entertained by Darwin. Professor St. George Mivart also considers that the Survival of the Fittest "plays merely a subordinate part." Very similar is the contention of Mr. Herbert Spencer. Mr. S. Butler rejects Natural Selection entirely.

Mr. J. Huddart cannot realize that such haphazard means as Natural Selection can have wrought out such marvels as are exhibited throughout creation. He insists that "were Natural Selection permitted to mould the forms of life around us, uncontrolled and undirected by any Supreme power, shapes the most grotesque and monstrous would inevitably inhabit the globe."

That Natural Selection has not been thus uncontrolled may be gathered from the limits which seem to have been set to the development and the modification of species. Why do we never see in any vertebrate animal more than two pairs of limbs or their rudiments? Why are parts which have lost their function, such as the external ear in mankind, or the vermiform appendage to the cæcum, still produced in generation after generation? Why is the secretion of silk confined to invertebrate animals, and the production of physiological venoms to cold-blooded groups? To such questions and to many more the believer in Natural Selection is so far less able to reply than is the naturalist of the Old School. The latter could solve all problems by an appeal to the *sic volo, sic jubeo* of the Creator. The Natural Selectionist refers us instead, substantially to chance. Can such an exchange satisfy our reason?

We may thus venture to say that there prevails a very wide-spread feeling of the insufficiency of the Darwinian

explanation of the Origin of Species. Authorities are by no means agreed as to its bearings and its efficiency.

But there is further divergence of opinion. One of the authorised expounders of Darwinism tells us that most people misunderstand the meaning of the phrase, "struggle for existence." "They imagine that the struggle is chiefly waged between different species, whilst it is chiefly conducted between members of the same species." But what says the co-discoverer of the theory of Natural Selection? In his "Island Life" he admits that "The most effective agent in the extinction of species is the pressure of other species, whether as enemies, or simply as competitors," a distinction, I must remark, without a difference. We cannot, indeed, conceive of a species extinguishing itself, the case of the Kilkenny cats, of course, being always excepted. But let us turn from authorities, even the most eminent, to actual facts.

The native flora and fauna of St. Helena have been practically extirpated by the goat. The young seedlings were browsed down as fast as they sprung up, and when the old giants of the forest decayed there were no successors to take their place. As a necessary consequence the insects and the birds disappeared in turn. The same "horned wretch"—fit type of evil—which as Sir Joseph Hooker shows, has ravaged the earth even to a greater extent than man has done by war, is now, in the very same manner, laying waste South Africa; to such an extent has the mischief already been carried that a troop of the Colonial cavalry on the march actually gave three cheers on meeting with a tree.

Vile European weeds, devoid alike of use and beauty, are fast extirpating the lovely and interesting flora of West Australia and of California. To give a catalogue of the instances where some plant or animal is being extinguished, or has already disappeared under the pressure of some other species, would fill a goodly volume. But almost every observer or even reader will himself have met with such instances. We may, therefore, I think, venture to reject Mr. Grant Allen's contention, and to conclude that though much suffering has been occasioned to individuals by struggles within the boundaries of the same species, for the causes of the great changes in either the animal or the vegetable world, we must look elsewhere, *i.e.*, to attacks from without.

I fear it must be owned that Natural Selection supplies too easy a solution for many difficulties. Thus we are asked

why are many insects, known to be venomous or offensive, clad with remarkably gay colours. Mr. Wallace and my late friend Thomas Belt held that these colours are a danger-signal, and have observed that such creatures are in fact shunned. I have found that some of the most strikingly coloured caterpillars feed on deadly plants and retain poisonous principles in their bodies. Yet Mr. Grant Allen, on the other hand, does not believe in warning colours, but asserts that poisonous plants, such as the arum, have, by a process of Natural Selection, developed intensely brilliant colours so as to allure birds to eat them. The seeds are then supposed to vegetate more luxuriantly in the decaying body of the poisoned bird. I cannot learn that Mr. Allen has ever met with a dead bird with arum-berries in its crop.

It would surely be a boon to the scientific world if the leading Darwinians would come to some understanding concerning natural selection and tell us what we are expected to believe.

But I must now ask if this process can produce new species? It has been admitted that before natural selection can come into play, variation must have already set in. Suppose a pair of animals existing in the primeval world had produced a hundred fertile ova. There are then only two possible cases: the young animals springing from these ova must either be one and all exactly alike, or they must exhibit certain differences. In the former alternative there is absolutely no ground for natural selection to work upon; the very idea of selection implying differences in the objects among which a selection is to be made. In the second alternative the varieties being, by hypothesis, antecedent to the action of natural selection, cannot be its effects. Hence in either case we have something which the Darwinian theory is quite unable to account for. We want a law which shall go deeper than Natural Selection, before we can understand the origin of species. At present we are merely offered, as it were, a rope ladder with no point from which it may be suspended.

We may ask how can Natural Selection have developed in any animal a power far beyond its utmost need? There is a small black spider in Southern Russia, that lurks among grass. Horses and cattle are often bitten upon the lips whilst grazing, and sometimes die in consequence. How can such a poison have been developed? What benefit can it confer upon the spider? It is, of course, unable to eat the dead

horse or cow. It cannot act defensively, since any animal which might crush the spider will not even be aware of its presence. And in order to overpower the creatures upon which the spider feeds, a venom incomparably less intense would suffice. A spider of similar properties is found in Queensland, and its bite, if not fatal to man, causes intense suffering. This species is black, with a red spot.

There is another consideration which seems to me not devoid of weight. Believers in Organic Evolution consider that all the species of mammalia found, *e.g.*, in Asia and Africa, have been derived from one—or a few—pristine placental forms. They suppose that in a similar manner the mammalia of Australia have been derived from one—or a few—pristine marsupial forms. But if we examine the Australian species we find them analogues, or it might perhaps be said parodies of the placental mammalian forms existing in the rest of the world. Thus the extinct *Thylacoleo carnifex* was in habits, form, and size, a lion, to be distinguished from the true lion merely by its marsupial bones. *Diprotodon* and *Nototherium*, also extinct, seem to have approximated to the elephant. The tiger wolf, or zebra wolf of Tasmania, is always, excepting its marsupial features, an excellent imitation of a wolf. In like manner various other Australian forms mimic the species of the rest of the world. This seems to show that Natural Selection is not supreme, but that its operation is over-ruled by some unknown agency which keeps it within certain limits.

We come now to another consideration. It is admitted that most animals and plants produce so numerous a progeny that were all to survive they could not find food. Hence the destruction of a large portion is imperative. But this process is not, as Darwinism supposes, a methodical weeding out of the unfit, whilst the healthiest and strongest are selected for preservation. As far as we can see it is a perfectly random operation. Mr. Wallace admits that the "weeding out" takes place among insects to a great extent in the egg and larva states, to which we may safely add in the pupa state. Of the eggs laid by a female butterfly many perish as such without ever seeing the light at all. But how is this effected? Every egg of the whole brood is equally helpless on the approach of a devourer or a parasite. For one that escapes in virtue of any superiority on its own part ten will owe their survival to what—humanly speaking—must be pronounced mere chance. One egg, without any peculiar fitness on its

part, may survive, because it has been deposited by the mother in a less conspicuous place than the rest. One egg may have perished, not from want of fitness, but because some ovivorous or parasitical insect visited the particular leaf to which it was attached. Other causes might be mentioned—accidental as far as man can judge—upon which the quickening, or the death of an egg, may depend. Here, then, there is no selection, no weeding out, but a destruction of a number of individuals with as little reference to their properties as if the question had been decided by lot.

From the egg we pass to the larva. Here there are doubtless greater individual differences. It may be at once admitted that one caterpillar may have keener senses to perceive the approach of danger, greater agility in escaping, more cunning in concealment, or an odour less attractive to enemies than have others, and that it may thus have a greater prospect of survival. But every observer knows that a vast number of cases must occur in which chance alone can decide. The quite accidental matter of position at some moment may be of far greater consequence for the life of a larva than a slight variation in any of the points just mentioned.

No small proportion of the premature deaths occur also in the pupa state, and here we have a return to the conditions of the egg. Without any reference to attributes of their own some pupæ may have been discovered by birds, by moles, hedgehogs, or the like, while others may by pure accident have escaped. The condition of a lepidopterous insect from the egg to its emergence from the chrysalis seems very much like that of the inmates of a town under the infliction of a heavy bombardment. It may perish or it may survive, neither alternative depending so much on its peculiar attributes as on the position which it occupies at some given moment.

From butterflies we pass to birds. In a work containing much with which I am unable to agree, the author argues that it is not the weaker and slower grouse on the Scottish moors which chiefly fall victims to the falcon. The swiftness of this destroyer is so vastly in excess of that of the fleetest grouse, that all relative differences in speed among the latter birds utterly vanish. The strongest winged and most vigorous moorcock, if once espied on the wing by the enemy, has practically no greater chance of escape than a feeble, sickly bird. On the very contrary, the boldest and most energetic grouse, which will be as a rule the healthiest, will fall victims

more frequently than their weaker brethren, from the mere fact that they are more venturesome, and hence more likely to be on the wing. The effects of the co-existence of grouse and falcons in any country, will, therefore, not be so much the development of a strain of the former better adapted for rapid flight, ultimately in the course of many generations endowed with longer or more pointed wings, but merely a thinning of numbers which will tell equally upon the strong and upon the weak, and which in some instances may even give an advantage to the latter.

The argument of the influence of the falcon upon the development of the grouse seems applicable not merely to this individual instance, but to every case where a bird or a beast has to struggle for existence against enemies greatly its superiors in speed, in strength, or in cunning. Slight increments in swiftness or force, trifling improvements in offensive or defensive arms or in means of concealment must, under many circumstances, be absolutely thrown away. Thus there are numbers of cases where preservation and destruction are not necessarily selective.

Nor can we admit that existing species are universally and necessarily ennobled by the "Struggle for Existence." It is well known that when a man is seeking to improve any cultivated plant or domestic animal, his first step is to suppress all struggle for existence, whether with other species or among co-existing individuals of the same species. The gardener plants on a given plot of ground only so many trees, etc., as may find a superabundance of nutrient matter, of air and light. As far as it lies in his power he eliminates all struggle with weeds, or animal competitors. And his results, gathered not by theory, but purely by experience, prove that he is right. Imagine a competitive turnip-field where the plants are left, in vulgar phrase, to "fight it out." The experience of slovenly farmers has proved that such a field will produce neither any fine roots, nor a total average crop equal to that of a field where the struggle for existence has been suppressed. If the weaker individuals finally go to the wall in this struggle, it has first called them into existence.

There is yet a further general consideration to be weighed. Mr. A. R. Wallace in his "Island Life" (p. 55) admits that "new species can only be formed when and where there is room for them." Hence the less severe the struggle for existence, or in other words the less Natural Selection is

brought into play, the more likely are new forms of plants and animals to be evolved.

Dr. H. Behr, speaking of the aboriginal vegetation of California, says:—"Its very variation (*i.e.*, its richness in species) is a proof of a certain want of vitality, for any more vigorous organism by superseding the weaker ones would have produced originally the monotony developed at present by the immigration of alien plants." Here an intensified struggle for existence is held up not as a multiplier but as a reducer of the number of species, as a cause of monotony. If such is its function in our time we may surely demand very good evidence before we admit that it can ever have played the opposite part, and been chiefly or even largely instrumental in producing the present multitude of organic forms from a few original types. We often forget that out of the almost infinite array of animal and vegetable species, a multitude, perhaps the majority, are rare. Now, if it be true that a rare species is one that is verging towards extinction, what are we to infer?

Passing from these general considerations to more specific objections, we often find in animals organs removed from their normal position and placed elsewhere. We generally find the organs of hearing, like those of the other special senses, placed in the head. But in insects the ears, or what stands in their stead, are located differently in different groups. Thus the Orthoptera (locusts, cockroaches, etc.) seem to have ears on their fore-legs. In other groups these organs are supposed to be attached to the subcostal vein of the wings. In the two-winged flies, on the contrary, the power of hearing has been traced to the two little knobs, called by some "balancers" or poisers, which take the place of the hindwings.

We may therefore ask how can the organs of so important a sense have been gradually transported, by Natural Selection, from one of these positions to another? What could be the advantage gained at each successive step? For we must remember that the advocates of Natural Selection tell us that only advantageous changes are likely to be preserved or handed down to posterity.

A most familiar fact in the life-history of insects is the change which most of the so-called orders undergo. On being hatched out from the egg they appear in forms for the most part quite unlike their parents, and it is only by a series of metamorphoses (as they are usually termed), that they

assume the form of maturity. But in one of the most primitive groups, the Orthoptera, there are no such changes. The young cockroach issues from the egg not as a grub or a maggot, but a miniature of the adult insect, from which, indeed, it differs mainly by the absence of wings. But the Orthoptera, and in particular the cockroach group, seem to be among the most ancient forms of insects—indeed, according to some authorities the most ancient of all true insects. It is further supposed that all insects are ultimately descended from the Thysanura. These creatures also do not undergo a metamorphosis. Thus there arises the question how, on the principle of Natural Selection, the metamorphic character in the higher and more recent orders of insects can have arisen? Where has been the advantage, or in other words, how has this change contributed to the preservation and multiplication of the species? We all know that the caterpillar, the grub, or the maggot is more helpless than the insect in its mature form. Its organs of sensation are less developed and its locomotory apparatus is less efficient. We are then almost forced to conclude that insects cannot have become metamorphic by a process of Natural Selection.

Another difficulty is the disappearance of the hind-wings in the Diptera, such as the gnat, the house-fly and their kindred. We find the other orders, both earlier and more recent, provided with the normal four wings, and we do not readily see how, on the principle of Natural Selection, the Diptera should have lost the hinder pair.

Among the vertebrate animals we find similar questions suggested. We take the fore-leg of the lizard and the wing of the bird, and we find each of these limbs useful. But if Natural Selection has gradually modified the one into the other it is hard to conceive how the earliest steps towards developing the leg into the wing could have been of the slightest use to the creature in question. And unless useful, such variations should not, on Darwin's hypothesis, have been reproduced and continued.

Perhaps the most decisive case of the inability of Natural Selection to account for some particular structure is the position of the mouth of the shark. Everyone knows that in fishes or reptiles generally the mouth opens at or very near to the foremost extremity of the body. Take up a herring, a frog, a serpent, or a lizard and imagine how strangely the animal would be inconvenienced in attempting to seize its food, and at what a disadvantage it would be placed in



defending itself against any enemy, if the mouth were made to open not at the front of the head. This, of course, would be especially the case in fishes which do not possess any limbs capable of assisting the mouth. Yet such is the position of the mouth in the shark which, by the way, ranks among the most ancient fishes.

Now can the mouth have conceivably been brought into its present position by Natural Selection? This peculiarity of the mouth, and every step by which it can have been reached, must be and has been a constant disadvantage to the shark. By it he often loses an expected prey, as many a diver and many a sailor who has fallen overboard can testify. Any shark which should have its mouth in the normal position would have the advantage over its rivals in the struggle for existence. Surely, then, we may safely conclude that the peculiar position of the shark's mouth has been reached and is now maintained not in virtue of, but rather in defiance, of Natural Selection.

From the above considerations, and from many more which might be brought forward if time permitted, we may, I submit, venture to conclude that Natural Selection or the struggle for existence is by no means the prime agent in genesis of species. That it may have a subordinate and limited efficacy I am not prepared to deny.

The more we reflect on the subject the more shall we become convinced that the origin of species is a far more difficult and complicated question than it may seem on skimming the writings of Darwin and Wallace, or indulging in the whipped cream of their popular expounders.

We have certainly no proof that Natural Selection is at present multiplying species, or that in existing species it is leading to any higher development. Often, indeed, it seems to work rather in the opposite direction.

That it seems to furnish in many cases a happy explanation we must admit. But in others it leaves us so completely in the lurch that it must be supplemented if not over-ruled by some higher agency.

We must also remember that supposing all the above mentioned difficulties explained away, and the objections set aside, Natural Selection furnishes merely a final cause for the properties of animals and plants. But science is in general more concerned with the efficient causes. Natural Selection may tell us that the colours of an animal approximate to the colours of the objects by which it is surrounded ;

that the exquisite designs of the wings of a male butterfly are elaborated for the sake of attracting the female, etc. This is very well as far as it can be demonstrated. But we rather seek to know how, when, and where, and from what materials the colouring-matters are produced, and how they are conveyed to the parts where we find them deposited?

This, I submit, the doctrine of Natural Selection does not do—does not even attempt to do. Bacon told us that the study of final and ultimate causes corrupted philosophy. How much more must this be the case if everything in the organic world is substantially referred, not to the Divine will, but to accident!

The PRESIDENT (Sir G. G. STOKES, Bart., LL.D., D.Sc., V.P.R.S).—I will now ask you to accord your thanks to Mr. Slater who has favoured us with this Paper and invite your remarks upon it.

Professor E. HULL, LL.D., F.R.S.—I think we are very much indebted to Mr. Slater for the able manner in which he has handled this question. He is one who is thoroughly competent to do so as an authority, and the facts and statements that he has placed before us speak for themselves. I do not pretend to be an authority on the subject; but as a geologist, it is one which I have been obliged to some extent to deal with; and even before this Society I have ventured to bring forward some arguments of a kind analogous to Mr. Slater's, although perhaps less formidable to the development theory than those he has produced this evening. There are many points in the paper which one would like to take up. For instance, I was much impressed with the question of the position of the shark's mouth, to which Mr. Slater has alluded. We might take that as a case of design in position, if so disposed; but I think, perhaps, a Darwinian advocate would reply to Mr. Slater on that point, that the shark is one of the oldest fishes, coming down from the Old Red Sandstone and Upper Silurian. They are heterocercal, and belong to an old type undoubtedly, and the position of the mouth may be due to descent from an ancient type. This only occurs to me by the way, and of course I am not

able to verify it.\* Perhaps Mr. Slater will say if I am right or not. If I am right in this view, the Darwinian advocate would perhaps say that the position of the mouth in the shark was only a survival of its progenitors of the Old Red Sandstone order. Well, that is just one point out of many, but I confess I do not see how it is possible to answer some of the arguments that Mr. Slater has adduced.

Now, as regards the survival of the fittest—that is to say, the fittest for its environment;—it will occur to one at once, that one cannot see, on that hypothesis, why there should have been any inhabitant of the ocean of a higher type than, say, the sharks or Placoid or Ganoid fishes. What is the difference in the environment in the ocean of the present day and that of the Tertiary time; or in the character of the ocean now and in the Silurian time? I think it would be very difficult for geologists to assert that there was any difference whatever in the oceanic waters of those ancient geological periods and those of the present day, and we may say of those ancient times that the creatures of those periods were fully adapted to their environment, and there is no cause, as far as I can see, why they should have been modified into other forms in consequence of any change in the environment. The same argument might be adduced in reference to many land animals. Why should there have been any animal higher than, say, the primitive earliest marsupial? To all intents and purposes the surface of the ocean, the air, climate and productions, were as suitable to the animals of those days as they are now. What I mean to assert is, there is no physical reason, as far as one can see, why there should have been any modification in the animal structures to suit any altered conditions of the surface of the land or the atmosphere or waters of the ocean. We might take up many points of this inquiry, and I think we should probably find that we were just as much in the dark as regards the higher races of animals and plants, as time went on, as we were at the beginning.

It seems to me to be almost unreasonable for anyone to assert that the present races of animals and plants can have come into existence by any natural process without the superintending,

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\* Professor Seeley refers to this subject in his *Manual of Geology* part I, p. 501.

guiding, and controlling intervention of an Almighty Creator; that is a conclusion to which I came a good many years ago, and one which I have never seen any reason to alter to the present day

PROFESSOR H. LANGHORNE ORCHARD, M.A., B.Sc.—I think Mr. Slater has abundantly proved that natural selection, according to the Darwinian theory, is subordinate to what Mr. Darwin would call chance, *i.e.*, undesigned coincidence. It appears that natural selection could not work at all until variation is produced; but this variation is not supposed to owe its origin at all to natural or any other selection, but to chance. Then again, after this natural selection has worked, the results of its working will or will not endure according as chance (*i.e.*, according to the Darwinian hypothesis, undesigned coincidence) shall go on. That, I think, is shown fully on pages 62–3; so that really the whole fabric and emphasis of Darwinism reposes upon chance. I think Mr. Slater rather dwelt upon this, that natural selection, even according to its advocates, is subordinate to chance, and would never originate but for *chance* having set up variation, and, having originated, it will or will not endure according as *chance* determines the matter. That, I think, is well shown here. It is very interesting to see that the goat is such a good natural selector, and no one, I suppose, not even the most thorough-going Darwinian, would deny that the goat has a certain amount of will and purpose; now if, in its action of natural selection, the goat works by will, purpose, and intelligence, why should not natural selection work on *other* occasions and through other agents also by will and purpose? Here, in one case at all events, it has done so, and in no case can it be shown that it does not do so, if it exist at all. If, in the case of the goat, there is will, purpose, and intelligence, why should it not appear in other cases? In every case in which we trace the cause or origin it is found to be in design, and it is not philosophical or scientific to assume that in other cases the cause can be unconnected with design.

REV. A. K. CHERILL, M.A.—Mr. Slater has brought a formidable attack against the theory of natural selection, and it would require a very careful consideration of all the points he has raised, one by one, to see if any answer can be found to them from the point of view of the advocate of natural selection. Perhaps one of his difficulties might be answered: I refer to the

one about the hearing apparatus of insects. It is no doubt difficult to suppose that the hearing apparatus has been turned from place to place in the course of development; but the hearing of insects is in a very rudimentary condition, and I imagine that when the sense of hearing first began to show itself, it would be by some parts of the body becoming more sensitive than other parts to sound waves; and it might be naturally supposed that this beginning would take place in various parts of the body, and that, afterwards, those rudimentary organs of hearing would be improved by natural selection which were in the most convenient place. But with regard to development generally, as has been pointed out this evening, the theory of natural selection seems to found itself on chance—the idea that chance variations take place and that then these chance variations are improved and confirmed when found to be useful. I think a very strong argument might be brought against this. A chance variation would be single, and there is no reason why, when a variation takes place by chance, two or three variations should take place at the same time, having a definite reference to each other. But it can almost always be shown that a mere single chance variation would be no advantage to the creature. Take such a case as the development of the power of flight in a bird. Supposing that a bird was born with a longer wing than the species usually had; if this were merely the result of chance there would be no reason why both wings should be longer—why not one only? But passing this by, and supposing they were both longer, still that would be of no advantage to the bird unless the muscles were also stronger in proportion, and then they would require a stronger attachment, so that it not only requires longer wings, but also a modification in the muscles and breast bone and possibly also in the breathing apparatus, all corresponding together in order that the bird may gain any advantage by it, and it is not credible that all these variations should occur together by mere chance. That opens up a further question upon which I should be glad if anyone would throw any light. It is often said by the advocates of natural selection that acquired variations are not perpetuated. I always ask everyone who knows anything about development, whenever I get the opportunity, what their opinion on that point is; I have asked a good many doctors and men who are expected to know something about it, and have very often received the answer from them that

they do not see any reason why an acquired peculiarity should not be inherited just as much as one that is accidental. Now if we could suppose that acquired faculties are inherited, it would throw a very different light on the theory of development. For example, in the development of the wings of birds; a bird that was very active in its habits would strengthen its own wings, and one might suppose that in the process of reproduction it would be likely that the nourishment should be specially directed towards that part which in the parent bird had been developed by practice, and that the wings of the young should be not only stronger, but also a little bigger. If anything of that kind could be maintained you would get something like a moral law brought into the theory of development—that when the parent exerts itself, and does its best, there might be some improvement in its offspring; and that seems to me to remove a good deal of the objection which is sometimes felt to Evolution as a theory which excludes moral government from the world, showing, perhaps, that it rather lays some sort of foundation for it.

The AUTHOR.—I was glad to hear what Professor Hull said in regard to the peculiar position of the shark's mouth. I have frequently heard it asserted that it was a providential provision to restrain its ravages, but to such assertions I have simply given Darwin's declaration:—"If one instance can be found that any property or peculiarity of an animal is not for its own advantage but for the advantage of its species, I throw my theory up entirely."

A remark was made by Mr. Cherrill which agrees very much with a point that was raised by Professor Fleeming Jenkin, I think it was in the *North British Quarterly*. The article made a considerable sensation and rather staggered Darwin himself. The point was to this effect:—"Suppose a male bird of any species possesses a rather better power of flight than the average of its contemporaries, the probability is that unless it mate with a female bird which always possesses some exceptional advantage, the advantage of the male will, in the course of a couple of generations, be bred out." We must have at least two individuals possessing a variation in some favourable direction, if we are to have a new and improved breed. Permit me in conclusion to express my thanks for the kind manner in which my paper has been received.

The meeting then adjourned.

## REMARKS ON THE FOREGOING PAPER.

Dr. D. BIDDLE, M.R.C.S., writes :—

In regard to the compatibility of (what looks like) chance with design, some very able remarks have been made by the author of *Ednor Whitlock*, Mr. Hugh MacColl. He shows by a mathematically conducted process of dotting paper within prescribed limits, that pre-ordained patterns can be produced with unerring precision, and even the shading be arranged, although the utmost licence be allowed to "chance"—within those limits.

But it has always appeared to me that the weak point in the theory of evolution is the making time a cause of change. Natural selection is admitted to be unavailing to produce new species within the period allowed to any single observer, and some go so far as to admit that the formation of new species by evolutionary methods must be regarded as pre-historical. But everything is possible, say they, if time be given. This is a delusion. It has lately been asserted that Sir G. B. Airy tossed pennies with a friend for a week, in order to find the longest run of heads (or tails) obtainable in that period, and 28 was the longest. But by the generally-accepted laws of probability, if time were allowed, a run of a million would occur, and there is nothing to prevent its occurrence early in the tossing. Common-sense, however, avers that a run of one hundred would make us doubt whether a fair penny was being fairly tossed. The law is said to be that, however often one face has turned up in succession, the chance is half, or absolutely equal, for the next toss; and yet it is affirmed that there is a constant tendency to equalisation, which should make the chance favour the other face, after a run on the former. This only shows how careful we should be in accepting the dicta of theorisers.

A theory which depends on chance-variations, occurring at stupendous intervals of time, and of which no trustworthy instance can be produced before our eyes, is doomed to failure, and must ere long be laughed out of court. It is eminently unscientific, for it believes in the production of an effect without the prior action of any proper cause.

Dr. H. B. GUPPY writes :—

Mr. Slater makes several very good points in his criticism of the theory of natural selection, and I think most people nowadays

are beginning to perceive that this is too difficult and complicated a question to be decided either in this generation, or, in fact, in any future generation without a far greater use of the methods of observation and experiment than has hitherto been made. As he remarks, the theory does not touch some of the simplest of phenomena in the world around us. Granted for the moment that in natural selection we have an explanation of the origin of a species, I cannot see that that lands us very much on our way; the practical knowledge of the cattle-breeder, the pigeon-fancier, and the horticulturist, in pre-Darwinian times carried them nearly as far, only that they did not formulate a theory of the universe on those grounds.

Some reference is made by Mr. Slater to the extent in which indigenous plants have been often exterminated by introduced species, but we must also remember the destruction, far more extensive, as I think, of would-be intruders into the domains of previously established species, and the modification of others. For nearly two years I have been making observations on the dispersal of water-plants and marsh-plants, and for a long time I imagined that the problem to be solved might be briefly thus stated:—"Given the distribution and capacity for dispersal of a plant, to explain its distribution," but I gradually came to see that another postulate was required. Take, for instance, the case of our common marsh-plant, *Bidens cernua*. It is rarely that one finds in the same plant to the same degree equal capacities for dispersal by the different agencies of the currents, birds, etc., etc. The achenes can float for months in sea-water and yet germinate; they float all the winter through in our rivers, such as the Lea, and must be transported in great numbers annually to the sea, when they commence their ocean voyage. The reflexed prickles of the achenes eminently fit them also for transportal in birds' plumage, for which they are as well adapted as the fruits of *Galium aparine*, and I cannot doubt but that birds such as duck, teal, &c., are very important agents in the dispersal of this plant. Yet with all these means of dispersal, this plant, though diffused widely in the temperate and northern regions of Europe, Asia, and America, is not to be found in the tropics. I do not doubt for a moment but that the achenes of this plant have been transported to almost every corner of the globe a thousand times over, and yet the species is not to be found in the tropics. And why not? Either the conditions there are antagonistic, or else it has sported in its new home into varieties that owe their permanence to their surroundings, and so we call them "species." The genus, as we learn from Bentham's and Hooker's handbook, is not very numerous in species and is diffused over the whole globe, occurring even in the Arctic Circle.

We thus perceive that the absence of a plant in a particular region may be by no means due to its inability to get there. We



must know "how it behaves under its new conditions" when it is there, and this is the postulate required for the complete statement of the problem. In this manner we open up *an illimitable field for experiment and observation*. A *Bidens cernua* cultivated for a series of generations in the tropics might tell a rather strange story concerning the antecedents of the species already established in that region.

Mr. JOSEPH JOHN MURPHY writes:—

I wish to offer a few observations on Mr. Slater's paper on "The weak sides of Natural Selection."

I agree with his main conclusion, which I understand to be that although natural selection is an agent in the origin of species, it is by no means the sole or the chief agent; but in some ways he seems unjust to the theory of natural selection, by demanding that it should explain what in the nature of things it cannot explain.

He says:—"Suppose a pair of animals in the primeval world had produced a hundred fertile ova. The young animals springing from these ova must either be one and all exactly alike, or they must exhibit certain differences. In the former alternative there is no ground for natural selection to work upon; the very idea of selection implying differences in the objects among which a selection is to be made. In the second alternative, the varieties (he means *variations*) being, by hypothesis, antecedent to selection, cannot be its effects. Hence, in either case, we have something which the Darwinian theory is quite unable to account for." This is perfectly true, and perfectly irrelevant. It is like objecting to the Newtonian theory of the planetary motions that it does not account for gravitation; an objection which, I believe, was actually made in Newton's time. Every theory, except in pure logic and mathematics (and I am not sure that geometry ought to be excepted) must postulate facts—and not only particular facts but general truths—without being able to account for them. The next observation, that "before we can understand the origin of species, we want a law which shall go deeper than natural selection," is as true and as luminous as if he had said "we want a law which shall go deeper than gravitation before we can understand the motions of the planets." To such objections it is enough to reply that gravitation is ultimate in astronomy, and spontaneous variation ultimate in morphology and evolution.

In another passage, Mr. Slater appears to have not only mistaken the logic, but the meaning, of the question under discussion. He says Mr. Wallace "admits that the most effective agent in the extinction of species is the pressure of other species, whether as enemies or simply as competitors—a distinction, I must remark, without a difference." No difference between enemies and competitors! If sheep were exterminated in one country through

being devoured by wolves, and in another through being deprived of pasture by the competition of goats, the difference would be important from a naturalist's point of view.

When the Darwinian (I say this without being myself a Darwinian) is assailed with a volley of questions, "Can your theory account for this, or for that?" it is generally wisest to reply "No, we are human, and do not profess to account for everything." When Mr. Slater asks "why do we never see in any vertebrate animal more than two pairs of limbs, or their rudiments? Why are parts that have lost their function, such as the external ear in mankind, or the vermiform appendage to the cæcum, still produced in generation after generation?" it is a sufficient answer to say that we have no means of measuring the force of heredity, which tends to the preservation of such organs, against the forces which tend to their disappearance; but the Darwinian, or any other, theory of evolution must take account of the existence of both. And when he goes on to ask "why is the secretion of silk confined to invertebrate animals, and the production of physiological venoms to cold-blooded ones?" he is propounding questions far more difficult than if he were to ask why certain crystalline forms are correlated with certain chemical properties; yet, so far as I am aware, the first step has not yet been taken in the explanation of such correlations in the inorganic world. The same applies to his concluding difficulty. "Natural selection may tell us that the colours of an animal approximate to the colours of the objects by which it is surrounded. This is very well as far as it can be demonstrated, but we rather seek to know how, when, where, and from what materials the colouring matters are produced." This is as reasonable as if he were to see a shipyard with machines of magnificent power and precision for forging steel, and then complain because he was informed by his guide that the chemistry of steel is very imperfectly understood.

I will conclude my reply to Mr. Slater with the consideration of what appears to me a purely imaginary difficulty, though I am aware that it has been strongly insisted on. I mean the position of the mouth in the shark, which is on the under side of the fish, some way back from the snout, instead of at the snout, as in some allied fishes. Mr. Slater says, "This peculiarity of the mouth must be a constant disadvantage to the shark. By it he often loses an expected prey, as many a diver and many a sailor who has fallen overboard can testify." This has been constantly repeated, and yet a little reflection will show its untenability. If a diver or a half-drowned sailor seriously asserts that he was saved from being devoured by a shark because the shark lost the imperceptible fraction of a second which he required to swim through the distance between his snout and his mouth, I cannot credit it.\*

\* The position of the mouth may be fitted for his usual prey, but the shark having to turn to seize *a man*, gives the latter an advantage.—Ed.

Another form of the statement is, if possible, yet more untenable. It has been said that the shark loses time in seizing his prey through the necessity of turning on his side. I think this must depend on the position of the prey; but if it is always necessary, what fraction of a second will this movement require? and cannot one of the swiftest swimmers in the sea turn his body half way round while swimming, so as to lose no time at all?

I have endeavoured to reply to Mr. Slater where I think him wrong, and especially to demolish his shark; but in many things I agree with him, especially as to the inadequacy of natural selection to account for the metamorphoses of insects, which appear to point to some unexplained law of life; and also its inadequacy to explain the very remarkable fact of the existence of closely parallel, though but distantly related, forms in the placental and the marsupial sub-classes of the mammalia. I believe that no theory of evolution can explain away the necessity of a Guiding Intelligence. My work on *Habit and Intelligence* contains my detailed views on this subject.

Mr. F. P. PASCOE, F.L.S., ex-President of the Entomological Society, writes:—

Many thanks for the proof copy of Mr. Slater's paper.

"Natural selection" is such a convenient phrase for our real ignorance that it will probably be long before it is discarded.

A power "picking out with unerring skill" seems to me to be utterly inadequate to account for the formation of new organs—some apparently useless as, for example, the comb-like organs of the scorpions. It makes no attempt to account for the numerous forms of the Protozoa—perhaps the most extraordinary beings in all organic nature.

"The proof that there is a selective agency at work is," Mr. Wallace thinks, "to be found in the stability of species." (*Nature*, Oct. 1, 1891.)

I have elsewhere remarked (in my *Summary of the Darwinian Theory*) that Darwin, with the conspicuous candour that distinguished him, was ever ready to admit—and in the strongest terms—what he considered were objections to his theory. Some he thought at first were "insuperable," such as the absence of the infinitely many fine transitional forms which must have existed; others—as the neuter ants—"fatal to the whole theory." That the eye could have been formed by natural selection "seemed absurd in the highest degree." Instincts, too, were so wonderful that they might appear sufficient "to overthrow the whole theory."

Some of these difficulties were "so serious that to this day he could hardly reflect on them without being in some degree staggered." But he says the more important of the objections to his theory "relate to questions on which we are confessedly ignorant; nor do we know how ignorant we are."

Darwin adds that the absence of "the infinitely many fine gradations between past and present species required on the 'theory,' is the most obvious of the many objections which may be urged against it." This he attributes to the imperfection of the geological record. Perhaps he has relied too much on the dogma, *natura non facit saltum*.

Dr. GERARD SMITH, M.R.C.S., writes:—

The Paper is a very important one, it is very desirable that biologists should be very accurate in their formulation of the facts of "natural selection," at present it is spoken of as if it were a *cause*, whereas the expression "natural selection" only really forms a convenient heading under which to group the results of observations upon the gradual modification of organisms; the way in which *variation* is used. So far as we have gone, variation is creation, for the power of variation must either be a production *de novo* of organs; or it must be the results of originally implanted potentialities in the protoplasm; I have heard and seen much of *rudimentary*, *i.e.*, degenerated organs, but fail to learn much about *nascent* organs; everywhere there are structures which must be *complete*, or *nothing*, that is, if the theory of natural selection as a *cause* is to hold; a nascent and as yet useless organ has a meaning if one believes in an *implanted* potentiality towards a certain grade of perfection or differentiation. So far as I can learn, on the purely materialistic conception, I am expected to put my faith in a process which is the result of a *previously existing* (but not *foreseen* or *implanted*) potentiality for variation in a useful direction producing variations having at first no relation to their environment, but subsequently made *useful*, though *useless* at first, by *use*; in preparation for a future more complete utility, which is not foreseen or expected! This is rather a hard creed I find.

The Rev. F. A. WALKER, D.D., F.L.S., writes:—

All arguments respecting the "struggle for existence" should, I venture to think, be stated in reference to some particular climate and country, and to its Fauna, which, whether consisting of insects, or other forms of organic life as well, happen to serve as the subject under discussion. It is obviously impossible to arrive at any world-wide generalization on this topic, because climatic influences which in the steaming tropics act with astonishing rapidity and productive power on all forms of living beings, prove actually the retarding, not to say destructive agents in respect of all except the very hardiest species in the frozen north. While *vice versâ*, arctic regions are singularly free from, and in many instances, altogether without the noxious creatures, and animals of prey that are constantly occupied in diminishing the numbers of their weaker and more defenceless brethren. By far the most manifold forms of life

have their beginning in regions of tropical heat which generate alike multitudes of harmless creatures, and at the same time give birth and development to their numerous natural destroyers, a counterpoise to the too rapid or excessive preponderance of any individual species being thereby effected. The parasitic lianas, and other creepers which surround with their deadly embrace the towering forest tree, and by degrees strangling all vitality in their supporter, hasten on its decay, and ultimately themselves come to an end together with the fall of the dead trunk, giant serpents, huge and venomous spiders, centipedes, and scorpions, etc. These are altogether wanting in temperate regions of our globe; and in Iceland no reptile of any description is to be met with, the most common of our small British centipedes occurs very rarely, while the circumstance of the Arachnida only comprising ground spiders, and very few (and I am not certain that there are any at all there) that construct webs, tends to numbers of flies and moths that would otherwise come to an end, being preserved.

Climate and isolation are the two factors we have to take account of in a review of the "struggle for existence" in "Ultima Thule." To take the second of these two circumstances first, its isolation at a distance of 500 miles from the north coast of Scotland, renders the chance of any new species of insect visiting its lonely wastes, almost, if not altogether, an impossibility. Supposing, for argument's sake that during the short island summer of 10 or 12 weeks an insect was imported by the periodical voyage of the Danish steamer, having settled on the vessel before it left the port of Copenhagen (as a solitary Painted Lady *V. Cardui*) was reported on reliable authority to have been seen in Shore Street, Reyhjavik, in the summer of 1888) the chances of its perpetuation and continuance are even more infinitesimal than those of its arrival. The food plants of the larvæ of most of our common butterflies either do not occur at all in Iceland, as for example the oak and the elm, or are very rare and local as the nettle and thistle, or are very scantily cultivated, as the cabbage and turnip. Any English species of butterfly moreover would be seriously, if not altogether handicapped in the struggle for existence in consequence of the fact that as all the so-called Icelandic forest consists of dwarf scrub, willow and birch, there is no hollow tree trunk wherein the imago can safely hibernate, or sheltered place whereon the pupa can hang up during the inclement weather. The actual severity of the climate, which, by the way, varies considerably in different parts of the Island, is not the only enemy to be reckoned with, but the fact of unavoidable exposure to its storms of wind, rain, and snow as well. The larvæ of several moths on the contrary, which occur in Iceland seek a refuge under ground preparatory to undergoing their change into the pupa state, and are thereby preserved from any ill-effects consequent on

"The dreadful pother o'er their heads."

The isolation above mentioned has not, as might at first be supposed, tended to produce a genesis of species peculiar to the island, so far as I have been able to ascertain after a thrice repeated visit to the greater number of the Icelandic fjords. Interesting local types and varieties of certain of the Icelandic moths beyond all question do exist, but they are either such as are also found at Rannoch or elsewhere in Scotland, or where not occurring in Scotland, are at all events represented by precisely the same forms in other parts of Scandinavia, in Finland, for example. Whether the Flora and Fauna of Iceland be compared with those of the Faroes and of Scotland on the one hand, or with those of Norway, Sweden, Lapland etc., on the other, Iceland in either case will be found to possess quite the lowest number of species of any of the aforesaid regions. The great scarcity of land birds as contrasted with aquatic ditto in Iceland *may* serve to account for the astonishing number of individuals of certain species of geometridæ which are thus marvellously aided in their struggle for existence. The vast quantities of offal and refuse of fish that lie scattered on the shores of every fjord beyond all doubt tend to the perpetuation in portentous numbers of such species of Diptera as habitually derive their subsistence from garbage, while the prevalence of the Arctic Tern, as delighting in similar food, is referable to the same cause. On the contrary, how are we to account for the fact that Ichneumonidæ are very few and far between, except by the circumstance that Diurnal Lepidoptera being wholly wanting, there are no chrysalids there for them to deposit their eggs in, as with ourselves? Or again, why is there only one species of humble bee in the whole of Iceland, and why is that so rare (for I believe I was the first to report it at all from the N. and E. sides of the island) except that some of the flowers in which the insect delights, as the blossom of the lime, are incapable of being cultivated in Iceland, and no pains whatever has been taken to plant others, as the broad bean and the clover, which last plant shows a straggling blossom here and there of both red and white varieties, solely from its seed having been accidentally introduced along with grass seed from another land. Here in the struggle for existence the perpetuation of the particular insect and plant is maintained indeed, but with difficulty, and in scanty proportions, and very locally. The utility of bees in hybridising clover is so well-known, that if a live batch were introduced into Iceland just as several have ere this into New Zealand, fragrant plant and winged bee might act and react on each other beneficially were it not for the utter want of enterprise and industry displayed by the Icelander. In conclusion, with regard to Diptera once more, genus *Eristalis* occurs in the Faroes but not in Iceland, and I was told by a noted British entomologist, that if I wanted to find *Eristalis* in Iceland, I had only to run a drain there, but for all that one species of genus *Helophilus* is found in Iceland, and that genus both there and at home delights fully as much in the neighbourhood of drains

on a sunshiny day as that of *Eristalis* itself. May we not rather regard the absence of *Eristalis* to be consequent on the scarcity or total want in Iceland of the cabbage, on which plant I have captured that tribe in the Faroes, as well as of the thistle, on which blossom I ordinarily take it at home ?

“The colours of an animal approximate to the colours of the objects by which it is surrounded.” P. 66.

This is most true in respect of all organic nature and is corroborated by sundry instances in beasts, birds, reptiles, insects, etc. But when the lecturer proceeds to state “But we rather seek to know how, when, and where, and from what materials the colouring-matters are produced, and how they are conveyed to the parts where we find them deposited ?” It is difficult to give a satisfactory answer, various are the hypotheses adduced. The green of many kinds of caterpillar from its similarity to that of the stalk or leaf that holds the insect affords one of the commonest and best known examples of the approximation above mentioned. Some have it that the insect is indebted for its colour owing to its constantly eyeing the verdure by which it is surrounded. (1) But effects produced by eyeing external objects are confined to creatures in a state of parturition. (2) Also all insects in the larva stage are incapable of reproduction. (3) And the green colour is common to both sexes of the larva. Others hold that the green of the caterpillar is occasioned by the creature’s absorption of the colouring matter through its pores, and others that it is caused by the creature’s constantly devouring the “chlorophyll” or colouring matter of the leaf, which forms its habitual food. But these are serious, if not fatal objections to both of these last two theories. In the first place perfect insects (moths for example) as closely resemble surrounding rocks in many cases as caterpillars do leaves. Now the rocks obviously cannot afford them nutriment, and further, a moth could not possibly imbibe by means of the pores, nor is its proboscis capable of the same work as the jaws of the caterpillar. Also the effect produced by feeding different individuals of the same kind of caterpillar on two different kinds of leaves or on two differently coloured leaves (dark green and light green for example) respectively does not appear to have any result as regards the next generation of *caterpillars*, while on the other hand a corresponding variation has been noted in the *moths* which are yellowish or white respectively after an indefinite period—say three or four seasons of the larvæ being so fed. Then again, leaves such as the larvæ feed on, are not the only objects that the larvæ resemble. Other larvæ of a dark brown tint are quite as undistinguishable from a crooked stick or twig. It is hardly possible to tell the difference between a common oak moth (*Tortrix Viridana*) when settled on a tree trunk from a small patch of pale green lichen, or again, another common *Tortrix* in a similar situation, from bird lime. Probably no one besides myself has collected the mountain geometra (*larentia*

*caesiata*) alike in the western highlands, and also in the S.W. of Iceland. It is worth while to compare the two series in question. The marbled appearance of the Scotch specimens so closely resembling their limestone or schistose rocks, and the dingy or grimy appearance of the Icelandic ditto enabling them to lie *perdu* on their native lava. While fully conceding that these instances of similarity in insects (of which a hundred more examples might be given) to the vegetable and the mineral world are ordered by Providence as a safeguard against total or partial destruction by their natural foes, I think we must be content to suspend our judgment as to the particular agency by which this wondrous similarity is effected.

“Genesis of Species,” p. 58. I am not personally quite certain whether I thoroughly understand this term. By exposure of larva or pupa to greater heat or more cold, or by feeding the larva on a different food plant from that which it frequents in a state of nature, we may obtain moths of different colour and markings, and by breeding again from these and repeating the same experiments through several successive seasons, we may perpetuate these superficial distinctions, but can we so permanently perpetuate them during the time we keep and register our observations of each successive brood or during our own lifetime as to render it certain that the insects, if restored to liberty and to their original food plants, would not shortly or at any rate by degrees hark back to their former type. The ultimate test of two true species is inability to pair with one another, or at least of reproduction in a third generation, just as the ultimate test of two genera is diversity of structure. Difference of colour, size, markings, may frequently be noticed in the case of two really different species, but these are not invariable nor final tests, either of two different species, or two different genera of butterflies, and even the two sexes of the same insect are often far from presenting the same striking difference to those of another tribe.



## THE AUTHOR'S FURTHER REPLY.

Dr. BIDDLE's remarks on the compatibility of chance (apparent) with design deserves serious attention. It may be mentioned that in the opinion of some authorities—undemonstrated, I must admit, and probably undemonstrable—the formation of new species or even varieties is at an end. We no longer witness the origin of new well-marked varieties of mankind, save by the mixture of races which already exist. Perhaps the isolation needed for this end is no longer existent. But the development of the European, the negro, and the Mongol from the original human stock—intermixture being impossible—seems to present a problem of the same nature as the origin of the tiger, the leopard and the jaguar from one common feline stock.

Mr. GUPPY's studies on the distribution of aquatic and marsh plants are of very high value as the type of a class of researches which ought to be extensively followed up. They are likely to throw useful cross-lights on all theories concerning the origin of species.

An interesting fact is the career of the Canadian water-weed (*Anacharis*, or *Elodea*?). Some years back it was spreading with alarming speed in our rivers and inland navigations. Suddenly it has ceased to multiply and has even died out in very many cases. No known cause has been ascertained.

Another interesting fact is the spread of the periwinkle. It is asserted by horticulturists and botanists not to ripen its seed in England. Yet we find it growing and spreading in woods where it cannot have straggled away from gardens, and where certainly no one can have taken the trouble to plant it.

Mr. J. J. MURPHY's remarks call for some reply, in fact they make me fear that I have not explained my views with sufficient distinctness.

The objection that Natural Selection cannot be accepted as the prime cause of the genesis of species seems to me, as to not a few abler men, simply fatal to Darwinism, and is not to be disposed of by the scarcely relevant illustration drawn from the Newtonian theory of planetary movements. I do not reject Natural Selection because I do not know its origin, but because it fails to account for the phenomena. Now, Mr. Murphy's objection to Newton merely raises the question of the *origin* of gravitation, not urging that it fails to account for the planetary movements. Hence between the cases there is no parallelism and Mr. Murphy's illustration does not apply.

As regards the "volley of questions" with which the Darwinian is assailed we all know that any theory must stand or fall according to the questions it can solve. If Nature—I dislike the term—has been for millions of years ever striving to improve plants and animals, preserving only modifications which are useful to them and cancelling every step in a different direction, it might be expected that the peculiarities which I pointed out would have been on the way to extinction. Darwinism says that every useless feature in an animal is a drain on its resources by which it must be *pro tanto* handicapped in the "struggle for existence." The illustration from the ship-building yard seems to me singularly unhappy. The uses to which iron and steel are there put are purely *mechanical*, and the question raised by the supposed visitor as to its *chemical* constitution is therefore irrelevant. But the difficulties which I have ventured to point out are of the very essence of the question.

The position of the shark's mouth is undeniable, and all evidence agrees to show that it is an inconvenience. Granted that the loss of time to the shark is small, yet the delay of a second may turn the scale between life or death. Unless the peculiarity of the shark's mouth is a gain to this fish it ought not, on the principle of Natural Selection, to have been preserved.

After careful inquiry made both before drawing up my paper and subsequently, all the observations I have been able to meet with agree with the view that the position of the shark's mouth is and must be a disadvantage.

The distinction between an enemy and a competitor is, in the instance given, purely *nil*, as far as the species attacked is concerned.

Mr. Murphy, in contending that this difference is something real, forgets that to the sheep it makes no ultimate difference whether it is devoured by wolves or starved from want of food. Nor does it differ substantially from the naturalist's point of view, since one and the same end is effected though in another manner.

The Rev. Dr. WALKER'S critique is most valuable. He regards the question—or questions—with the eye of a practical observant naturalist and points out some of the many difficulties to be encountered in explaining, *e.g.*, the colouration of insects.

It is, indeed, possible that the chlorophyll of green vegetables may take a part in the colouration of butterflies. But we have to ask why are green colours wanting in other species which select the same diet?

I have succeeded in detecting tannin in many insects—all plant-feeders—and I think this fact may explain the frequent occurrence of browns, russets, tans, &c., both in Lepidoptera and Coleoptera. But before we can generalise we must acquire a

much better acquaintance with the processes of digestion and assimilation in insects.

I may venture to question one remark of Dr. Walker's on the scarcity of life in arctic regions. According to various observers (Mr. Seebohm, *Siberia in Asia*) mosquitoes are as abundant in the Tundras of Siberia as in any part of the globe. But though insect *individuals* are present in such numbers, insect *species* are few.

### ORDINARY MEETING.\*

THE PRESIDENT, SIR GEORGE G. STOKES, BART., V.P.R.S.,  
IN THE CHAIR.

The Minutes of the last Meeting were read and confirmed, and the following Elections were announced :—

ASSOCIATES :—The Right Reverend the Bishop of Singapore and Sarawak ;  
The Rev. E. A. Davies, F.R.G.S., Rochdale ; E. W. Gurney Masterman,  
Esq., F.R.C.S.E., Lond.

The Proceedings at this Meeting will shortly be ready for publication.

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\* Feb. 15, 1892.

## ORDINARY MEETING.\*

H. CADMAN JONES, ESQ., M.A., IN THE CHAIR.

The Minutes of the last Meeting were read and confirmed, and the following Elections were announced :—

MEMBERS :—Major-General W. L. Geary, C.B., R.A. ; The Rev. Townsend Storrs, M.A., Head Master, the Grammar School, Doncaster.

ASSOCIATES :—The Rev. Canon Baker, F.L.S., F.S.Sc., J.P., Cape of Good Hope ; The Rev. E. Herbruck, A.B., A.M., Ph.D., Ed. *Christian World*, United States ; The Rev. Alexander Macpherson, Scotland ; J. Whitehead, Esq., F.G.S., Guernsey.

The following Paper was then read by the Author :—

*ON SERPENT-WORSHIP AND ON THE VENOMOUS  
SNAKES OF INDIA AND THE MORTALITY  
CAUSED BY THEM.* By SIR JOSEPH FAYRER,  
K.C.S.I., LL.D., M.D., F.R.S.

THE serpent is the ancient enemy of the human race, and it is still held in antipathy, not only by man, but by the lower animals. In man, this is probably due as much or more to the lethal properties of some forms, as to the repulsiveness of their aspect generally ; while animals seem to be instinctively imbued with the dread of them. The destructive qualities, albeit the property of but few members of this large order, have come to be attributed so universally to all, that the innocent are classed with the guilty, and the harmless creature which undulates so gracefully through the grass, is popularly associated with the deadly cobra or rattlesnake.

But although dread of their baneful properties may lie at the root of the repugnance in which they are held, yet with this feeling, no doubt, has been mingled a sentiment of veneration for their supposed wisdom and supernatural power, which, combined with fear, originated one of the earliest forms of worship, in which superstition and religious feeling have found expression, for coeval with the worship of trees, the heavenly bodies, and other natural objects, we find that ophiolatry has been general throughout the world from the remotest antiquity.

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\* March 7, 1892.

Serpent-worship, according to Fergusson,\* is characteristic of the Turanian races, and is rarely to be found among Aryan or Semitic peoples. There is no mention of it in the Old Testament from the formation of the Jewish nation, unless the raising of the Brazen Serpent be so considered, but six centuries later, Hezekiah "broke in pieces the brazen serpent that Moses had made; for unto those days the children of Israel did burn incense to it; and he called it Nehushtan," 2 Kings xviii, 4 and 5. Between these periods there is no other mention of it in the Old Testament, but in the book of the Wisdom of Solomon, xi, 15, we read, "They worshipped serpents void of reason"; nevertheless its revival among the Gnostic sect of the Ophites points to the fact that the notion was not extinct. "A wondrous blending of the ancient rites of Ophiolatry with mystic conceptions of Gnosticism appears in the cultus which tradition (in truth or slander) declares the semi-Christian sect of Ophites to have rendered to their tame snake, enticing it out of its chest, to coil round the sacramental bread, and worshipping it as representing the great king from heaven, who in the beginning gave to the man and woman the knowledge of the mysteries" ("Primitive Culture," Tylor).

Serpent-worship, according to Fergusson, has prevailed to a greater or less extent nearly all over the world.

In America it was known in Peru, Mexico, and among the Red Indians, according to ancient records of the United States.

Its prevalence in Western Asia seems doubtful, except in Judæa, to a slight extent in Phœnicia, and in the Troad, among the so-called Ophiogones.

As regards Europe, there are next to no traces of its prevalence among the Germans, though Tylor refers to the "Prussian serpent-worship and offering of food to the household snakes," nor among the Gauls nor Britons. Ophiolatry is said to have been practised by the Druids; according to Fergusson there is not much evidence of this, but other authorities state that the serpent's egg was the Druids' crest, and that the serpent was entwined at the foot of their altars. At Avebury in Wiltshire, there existed the figure of a serpent in stones extending for two and a half miles, of which the head and tail are still obvious. There are traces of it in

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\* To Fergusson's "Tree and Serpent-Worship," and Tylor's "Primitive Culture," I am indebted for much information.

Scandinavia and on the east coast of Scotland, north of the Forth, where sculptured stone monuments have been found on which the serpent appears frequently, and as a prominent figure.

In Greece the temple of Æsculapius was a centre of serpent-worship, whilst the Æsculapian rod symbolises wisdom. In this, as in other cases, the serpent was the symbol of the god, rather than itself the god.\*

In Italy the serpent was often represented as the *genius loci*, but there is no direct evidence that beyond this the Romans ever worshipped it. Dante, in his "Inferno," ascribes to the serpent supernatural power, his bite causing a man to be reduced to ashes:—

"Ed ecco ad un, ch'era da nostra proda,  
S'avventò un serpente, che'l trafisse  
Là, dove'l collo alle spalle s'annoda.  
Nè O si tosto mai, nè I si scrisse,  
Com' ei s'accese, ed arse, e cener tutto  
Convenne che cascando divenisse."

(Inferno, xxiv, 97.)

It probably prevailed in Eastern Europe during the Middle Ages, and in Esthonia and Finland up to a comparatively recent period.

In Africa, Fergusson says that Sheikh Haredi in Upper Egypt is one of the best known sites of modern serpent-worship, but there are very slight traces of its prevalence in ancient Egypt. The Egyptians worshipped many animals, but there is nothing to show that the serpent was honoured above the rest.

In Abyssinia it was worshipped before the introduction of Christianity in the fourth century, and on the Guinea coast serpent-worship flourishes at the present day, and possibly has done so for the last 4,000 years.

In Eastern Asia, Persia affords but slight traces of it. The Iranians were Aryans, and brought with them fire-worship. It may have existed among their predecessors.

Cashmere was one of the principal centres of it. There is no direct testimony of its existence there till a century before the Christian era, and the latest authoritative notice of its practice was in the reign of Akbar (fourteenth century).

In Cambodia and the adjacent countries, serpent-worship reached its fullest development. The country was conquered

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\* For instance, when a pestilence was raging in Rome in 291 B.C., the god was brought in the form of a serpent from Epidaurus. A sanctuary was built for him on the Tiber Island.

by the Siamese in the middle of the fourteenth century, and since then it has given place, to a great extent, to Buddhism.

It prevailed also in Ceylon till the island was converted to Buddhism, in the third or perhaps the sixth century, and there are traces of it there still.

In China there are only slight traces, but the repetition of the dragon-like forms in connection with temples, pagodas, &c., in China and Burma, is suggestive of something akin to the ophidian worship.

In India it was not noticed before the Mahabhrata, but in that is mention of the Nagas, the great serpent-worshipping race, who, taking the serpent as their emblem or cognizance, came to consider themselves the descendants of serpents. There are tribes in India called Nagas at the present day.

Ophiolatry in a modified form still prevails in many parts of India. It is met with in Manipur, Cashmere, Sumbulpore, Nepaul, in many parts of the Deccan and Southern India. On the festival of Nag-Panchmee, snakes are worshipped by most of the lower tribes of the Deccan.

Serpent-worship has no place in Brahminism, but the Hindus of the present day, if they do not directly worship the snake, will neither injure nor kill, but rather propitiate it. This feeling may be as much due to fear of any bodily harm it may do them, as to the idea of its possessing supernatural powers. Tylor says the serpent has been taken as the symbol of the world, of the Tauut, or heaven-god of the Phœnicians, and as the emblem of eternity; in the latter case it is depicted with its tail in its mouth. It may have been the personification of evil in the Apophis serpent of the Egyptian Hades, and it was so in the wicked serpent of the Zoroastrians, Aji Dahaka; Ajdaha is still applied to the larger constricting snakes. Sir George Birdwood tells me that besides abstract evil, Aji Dahaka symbolised death, destruction, the storm cloud, &c. "There Ingromaniyus (Ahriman) the deadly created a mighty serpent, and snow, the work of Deva." *Cyclopædia of India* (Balfour). He also reminds me that the deadly serpent is the symbol of evil in all Eastern countries, though there, as in Greece and Rome, it may have had also a creative symbolism.

But time does not permit that I should dwell longer on this exceedingly interesting subject; I must rather describe to you those forms of the serpent in which the lethal attributes exist in their most marked conditions, producing fear and repugnance, if not the worship of olden times.

The cobra, as I have said, is an object of veneration and superstitious awe to the natives of Hindustan, for in a religion that deprecates the wrath of a cruel and relentless power which it desires to propitiate, the symbol of evil represented by this reptile is naturally regarded with peculiar deference. The rapidity and deadliness of its poison, and the large death-rate due to its bite, explain these feelings, which need cause no surprise when it is remembered that upwards of 20,000 people die yearly of snake-bite alone.

I shall give you a brief account of the most interesting forms of venomous snakes, confining myself to those that are found in our Indian Empire. It would be barely possible even to enumerate in the time at my disposal, the deadly snakes of other countries, or the innumerable innocent forms.

Wherever climate and other conditions are favourable, snakes are likely to be found, the most venomous as well as the greatest numbers in hot and tropical regions. In our own island, as well as in most parts of Europe, the common adder is the only venomous snake, and its power is feeble compared with that of the snakes of India, the West Indies, Tropical America, Africa, and Australia.

The order Ophidia is divided into Colubriform and Viperiform; the first are both venomous and innocuous, the second are all venomous. Both are numerous represented in India; the colubriform has five genera of *Elapidae*, and four of *Hydrophiidae*, the viperiform has two genera of *Viperidae*, and four of *Crotalidae*, making fifteen poisonous genera, which comprise a large number of species, but this is small compared with the number of innocent colubrine snakes.

The most widely distributed venomous snakes are the viperiform; America and Africa abound in them; the *Crotalidae* are most numerous in America, the *Viperidae* in Africa, whilst poisonous colubrine snakes are most numerous in Asia.

The Ophidia are cold-blooded vertebrata, destitute of external skeleton, pectoral arch, sternum, or limbs. In a few there is a rudimentary pelvis and hinder extremities.

The body is covered by a deciduous epidermis and scales. These, with some exceptions, assume on the head and abdomen the condition of scutæ or plates. The bones of the mouth are connected by ligaments, which allow of great distension, thus enabling the creature to swallow prey larger in diameter than itself.

In snakes, one lung is much larger than the other. Their



circulation of mixed arterial and venous blood is regulated by a heart, consisting of one ventricle, and two auricles. Locomotion is effected in the terrestrial forms by the motion of the numerous ribs, which are connected indirectly with the abdominal scutæ. These act as feet, and aided by the undulations of the body, grasp the surface, thus effecting the rapid movements of which a snake is capable. The pelagic serpents swim like fish, motion being effected by the undulations of the body and of the fin-like tail.

Snakes have neither external ears nor eyelids; the eye is protected by a transparent capsule, which is shed with the epidermis. The approach of moulting is indicated by diminution in the brilliancy of the colouration, and a pearly opacity of the eye; the creature itself becoming more or less apathetic until the process is completed.

The scales and scutæ form the basis of classification. Those on the head are named as follows:—

Rostral.		Præ } Orbitals.
Anterior } Frontals.		Post } Orbitals.
Posterior } Frontals.		Upper } Labials.
Vertical.		Lower } Labials.
Supra-ciliary.		Temporals.
Occipital.		Mental.
Nasals.		Chin-shields.
Loreal.		

The form and arrangement of the scales vary. In some snakes they are plain and lie side by side, more or less lanceolate in form. In others they are imbricated, that is they overlap each other. On the head, in some snakes, they are arranged as large plates or shields. On the abdomen in the land snakes, they are in transverse plates for the purpose of locomotion. In the *Hydrophidae* and burrowing land snakes these are absent.

Snakes are oviparous and viviparous; the colubrine, except the pelagic forms, for the most part belong to the first class, the viperine to the second. The cobra lays twenty to thirty white, leathery eggs, which are hatched in some warm place by natural heat. Some are said to incubate; the python is said to coil itself round the eggs until they are hatched. The female of all snakes is said to be larger than the male; there are slight differences in colour and form, but no other external distinction.

Snakes hibernate in the cold, but returning warmth rouses them into activity. They generally eat living creatures, but

some will eat eggs—the cobra robs the hen roosts, or devours insects, molluscs, and even, it is said, vegetable matter; and some are cannibals—the ophiophagus and callophis live on snakes. In captivity they will, it is said, drink milk.

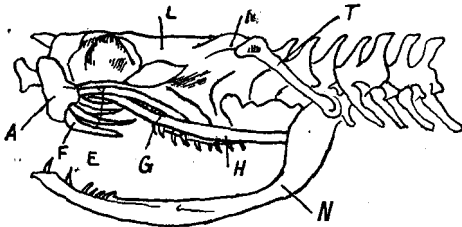
Snakes differ in their habits and modes of life, and are grouped accordingly. Tree and grass snakes live in the trees, bushes, and grass, and are often coloured like the vegetation they frequent; their tails are prehensile. When slender, they are called whip snakes; innocent and poisonous forms are found among these. Ground snakes are found in all three sub-orders; the great proportion belong to this group.

Burrowing snakes live much under ground, have a rigid, cylindrical body, short tail, narrow mouth, small teeth, and are all innocent.

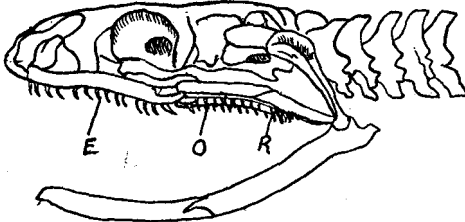
There are fresh-and salt-water snakes. The salt-water snakes are adapted for an aquatic life, and are venomous; the fresh-water snakes have not the same characters as the *Hydrophidae*, and are innocent—a curious fact! The *Hydrophidae* are viviparous.

I.

VIPERIFORM (*Daboia Russellii*).



INNOCENT (*Ptyas Mucosus*).



COLUBRIFORM (*Naja tripudians*).



- A Maxillary bone.
- B Intermaxillary bone.
- E Maxillary teeth.
- E' Ecto-pterygoid bone.
- F Poison fang.
- G Palatine bone.
- H Pterygoid bone.
- L Frontal bone.
- M Mastoid bone.
- N Mandible.
- T Tympanic.
- O Palatine teeth.
- P Parietal bone.
- R Pterygoid teeth.

Deglutition is effected in a peculiar way; the prey being seized, the mouth gapes laterally and vertically, each side of the jaws is called separately into action; the sharp and recurved teeth hold the prey firmly, as each side of the jaw alternately advances or relaxes its grasp, and it is thus gradually but inevitably engulfed.

The maxillary bones in the venomous snakes are much shorter, and provided with fewer teeth than in the innocent. In the latter, they are elongated slips of bone set with small recurved teeth. In the poisonous colubrine snakes they are less elongated and have a fixed, large poison fang, several loose, reserve fangs, and one, two, or more fixed smaller teeth, not directly connected with the poison apparatus. In the *Viperidae* the maxillary bone is a short, triangular, movable wedge, furnished with a poison fang lying hidden in the mucous sheath. The movements of the fang are due to the rotation of the maxillary bone. This mobility is great in vipers, whilst it is slight in the colubrines.

When the fang is reclined or erected, the maxillary bone into which it is inserted is pushed by the external pterygoid bone, a movement which is effected by muscular action. The muscular arrangement for opening and closing the mouth at the same time compresses the poison gland, thereby injecting the venom through the tubular fang. The fangs are shed at intervals, and to supply the loss, the reserves are provided. These lie in the capsule of mucous membrane which ensheathes the fang. The fang is, during development

## II.

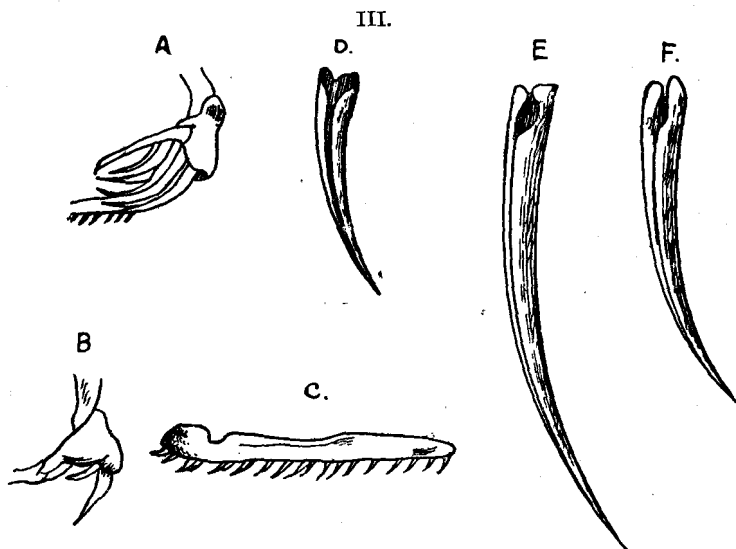
COBRA DI CAPELLO (*Naja Tripudians*).

- A Poison gland.
- B Duct.
- C Fang.

folded on itself so as to form a tube. It is along this channel that the poison passes; when the fang is deeply imbedded the quantity of virus injected is considerable, and its effects are rapidly manifested.

The poison glands are situated between the orbit and the tympanic bone; they are composed of lobes and lobules, which having secreted the virus, transmit it under muscular pressure through a duct which communicates with a triangular opening at the base of the fang. They are of various forms and sizes; in *callophis* they are much elongated; in the cobra they are of the size and something of the shape of a small almond.

The virus is a transparent, slightly viscid fluid, faintly acid in reaction, of a straw colour—in the ophiophagus, of a yellow colour; when dried it forms a semi-crystalline substance, like gum arabic. It is secreted in considerable quantities, and if a fresh, vigorous cobra be made to bite a leaf stretched across a tea-spoon—or as the natives do it, a mussel-shell, several drops may be obtained. The poison



- A Maxillary bone and fangs of *Daboia Russellii* (viperiform).  
 B " " " " *Naja tripudians* (colubriform).  
 C " " " teeth of *Ptyas mucosus* (innocent).  
 D Fang of *Hydrophis*.  
 E " *Daboia*.  
 F " *Naja tripudians*.

is exhausted when the snake has bitten frequently, but is rapidly reformed; in the interval the reptile is comparatively harmless, but soon becomes dangerous again. A vigorous cobra can kill several creatures before its bite becomes impotent. Removal of the fangs renders the snake temporarily harmless.

Some animals, especially the pig and the mongoose, are supposed to have immunity from snake-bite; fat sometimes protects the former, the latter is so wiry and active that it frequently escapes with only a scratch; but, if either of them be fairly bitten in a vascular part, it succumbs like any other animal.

The chemistry of snake-poison has been studied by Fontana, by Prince L. Bonaparte, Armstrong, Gautier, and others, and recently by Drs. Weir, Mitchell, and Reichert, of the United States. It is a most virulent poison, and may neither be sucked from a bite nor swallowed with impunity. It acts most rapidly on warm-blooded, but is also deadly to cold-blooded creatures, and to the lowest forms of invertebrate life. Strange to say, a snake cannot poison itself, or one of its own species, scarcely its own congeners, and only slightly any other genus of venomous snake; but it kills innocent snakes quickly. Snake-poison kills by extinguishing the source of nerve energy. It is also a blood poison and irritant, and causes great local disturbance as well as blood change. If it enter by a large vein, life may be destroyed in a few seconds. The chief effect is on the respiratory apparatus, and death occurs by asphyxia; but general paralysis is also a result. The phenomena of poisoning vary according to the nature of the snake and the individual peculiarities of the creature injured, the chief difference being observed in viperine, as contrasted with colubrine poison. The latter is a nerve-poison of great deadliness; as a blood poison its results are less marked. Viperine poison, on the other hand, is a more potent blood-poison.

Adder poison is of the viperine character, and though its immediate effects as a nerve-poison are feeble, yet those on the blood and locally on the tissues may be productive of serious symptoms.

It is impossible to enumerate all the antidotes that have been reported beneficial; but amongst those that have the greatest repute may be mentioned arsenic, ammonia, alcohol, quinine, strychnine, acids, snake poison itself, snake-bile, and the snake-stone, so much relied on in India.

These stones are said to attach themselves closely to the bitten part, the blood that oozes out being rapidly absorbed, and when it drops off the bitten person is thought to be out of danger. Faraday said that these are pieces of charred bone. There may be a fragment of truth in the supposition that they are of use, because in absorbing the blood, they must also absorb some of the poison, though so little that their efficacy must be a mere delusion.

Experience shows that so far no physiological antidote to snake-virus is known, and that, when the full effect is produced, remedies are of little avail; but when the poison has entered in smaller quantities, medical treatment may be of service.

The entry of the poison into the system should be arrested, if possible, by a ligature above the injured part; next the poison in the wound should be destroyed or removed by excision or by burning, and the application of potassium permanganate. The subsequent treatment is conducted on ordinary medical principles, of which further details would be out of place here.

I must now describe the principal venomous snakes of India. The *Elapidae* are subdivided into *Najadae* or hooded snakes, and *Elapidae* proper, which are not hooded. *Najadae* has two genera, *Naja* and *Ophiophagus*; *Elapidae* has three, *Bungarus*, *Xenurelaps*, *Callophis*.

*Naja* includes the several varieties of cobra, which are all of one species, though differing considerably in external appearance.

The cobra di capello (*Naja tripudians*) has numerous synonyms in different parts of India. A common general native term is *kala nag* or *kala samp*. There are many varieties, and they are considered by natives to be of different degrees of activity or deadliness; but the probability is that any difference is due to temporary or individual causes.

The cobras are all hooded, bearing on the hood a spectacle mark, or a single ocellus, or no mark at all; this hood is caused by the expansion of a certain number of elongated ribs. The body and tail are relatively of moderate length, seldom together exceeding five or six feet, more frequently three or four feet. The scales are smooth and imbricated; there is no loreal shield, the nostrils are lateral and the pupil is round. The colour generally is from a light chocolate, speckled, to a dark brown or even black. The head is short, and not very distinctly separated from the neck; the fangs

are of moderate size and but slightly movable; there are one or two small teeth behind them in the maxillary bone.

Cobras are most active in the night, though often seen in the day. They will live weeks, even months in captivity, without touching food or water. They go into water readily, but are essentially terrestrial snakes. They occasionally ascend trees in search of food, and are not infrequently found in holes in walls, old ruins, fowl-houses, and among stacks of wood, cellars, old brick-kilns, old masonry of brick, or stone, or mud, among the grass or low jungle: such are the common resorts, and during the rains and inundations they collect in such places of refuge, where men, stepping on, or unintentionally disturbing them, mostly in the dark, are bitten.

The cobra sheds the epidermis with the outer layer of the cornea frequently, the fangs also are shed. The entire slough is often marked by a single rent, through which the creature has emerged, brightly coloured and glistening in its new epidermis. It aids the process of exfoliation by friction against some hard substance, such as the branches of a tree, a stone, or the like, the cast off epidermis being often found in fragments. It is oviparous, the eggs are about the size of those of a pigeon, and the shell is white, tough, and leathery.

The cobra is found all over Hindustan, up to a height of 8,000 feet. It is equally dreaded and fatal wherever met with; fortunately it is not naturally aggressive unless provoked, then raising the anterior third or more of its body, and expanding its hood, with a loud hissing it draws back its head prepared to strike, darts forward and scratches, or imbeds its fangs in the object of attack. In the latter case, the results are often dangerous and fatal, but if the fangs only inflict a scratch, or if the snake be exhausted, the same danger is not incurred. If the poison enter a large vein and be quickly carried into the circulation, death is very rapid. Men have been known to perish from a cobra bite within half an hour. The largest and strongest, as well as the smallest and weakest creatures succumb. Fortunately all who are bitten do not die. In the first place some human beings as well as lower animals have greater tolerance than others; or a wound may have been inflicted and yet but little of the poison inoculated; or in the third place, the snake may be weak or sickly, or it may have been exhausted by recent biting, and thus have become temporarily incapable of inflicting a fatal wound, though it may still poison. But when a cobra in the full possession of its power bites and injects the

poison into man or beast, it is almost surely fatal, and all the vaunted antidotes are futile.

Cobras are frequently exhibited by the so-called snake-charmers. Their graceful attitudes, with raised heads and distended necks, as they sway from side to side watching the movements of their keeper, and frequently striking at him, and the ease with which they are handled, make them general favourites. I may here remark that the cobra depicted in Hindoo legends or old paintings is the gokurrah, or spectaclèd snake. They are generally deprived of their fangs (which is done by cutting them out with a coarse knife), but the snake-charmers know the habits of the creature so well that they handle them without fear, even when armed, though with great caution, always grasping them tightly below the head with one hand and holding the tail with the other. They know that a new fang is soon produced, and to prevent this they sometimes remove the capsule and reserve fangs, thus making the snake permanently harmless. The sole secret of these men lies in their dexterity and fearlessness. Their *muntras*, their antidotes, and the pipes with which they pretend to charm are as devoid of real power over the snake as are the snake-stones, roots, and other nostrums over its poison. They know that dexterity is their real security.

The snake-charmers occasionally exhibit the ophiophagus, —which, like the cobra, dilates the hood when excited—also the bungarus, daboia, and some of the innocent snakes, such as *Chrysopelia*, *Passerita*, *Ptyas*, and *Erix*, which are remarkable for the beauty of their colours, their activity, or their peculiarity of form. These exhibitions are always accompanied by the music of the pipes.

The cobra is an object of superstitious awe to the Hindus. Should fear or the death of some inmate of the house in which the cobra has taken up its abode prove stronger than superstition, it may be caught and deported in an earthen jar to some field, where it is allowed to escape, but not destroyed. Still the cobra has many enemies. Besides by its natural foes, such as the mongoose (*Herpestes*), pigs, rapacious birds, and other creatures, numbers are destroyed by low caste people for the sake of reward. But still the loss of human life is great.

The *Ophiophagus elaps* (Hamadryad, Sunkerchor) is one of the largest venomous snakes. It attains a length of ten or twelve feet, is very powerful and active, and is said to be aggressive; it is hooded like the cobra, and resembles it in



general configuration. The adult is some shade of olive green or brown; the shields of the head, the scales of the neck, hinder part of the body and tail are edged with black; the body and hood are marked with black oblique bands. There are several varieties with modifications of colouration, but the general characters are essentially the same. The young differ considerably from the old, and might be mistaken for another genus; they are black, with numerous white, equidistant, narrow cross bands. The shields surrounding the occipital are large, and give a distinctive character to the adult snake. This snake, though widely distributed throughout India and in the Andaman Islands, is not common and probably does not destroy many human lives; but it is very deadly, and its virus seems to have similar effects to that of the cobra. It is found in the forest and grass jungle, and is said to live in hollow trees, and to climb them, being frequently found resting in the branches; it also takes to the water very readily. As its name implies it feeds on snakes, though probably when they are not forthcoming, it is contented with other small creatures. Its hood is smaller than the cobra's; it is even more graceful in its movements and turns more rapidly. The snake-charmers, who prize it highly, say it is very difficult to catch and handle. A fine specimen of the ophiophagus, about nine or ten feet in length, lived for some ten years in the Zoological Society's gardens, and died two or three years ago; it consumed numbers of the common English snake, and, I believe, would eat nothing else. It seemed a quiet, unaggressive creature until roused, when it would raise its head, dilate its hood, and strike at any object brought near it.

*Bungarus* has two Indian species. The *Bungarus cœruleus* or krait, is probably next to the cobra, the most destructive snake to human life. The other species, *B. fasciatus*, sankni, or raj-samp, is probably equally poisonous; but it is not much brought in contact with men, and therefore is less destructive to human life than *cœruleus*. The krait is of a dark, almost steel-blue black to a chocolate brown, with narrow white cross-streaks, rings, or bars of white; the ventral surface is of a dark, livid colour, or white or yellow tinge; but there are varieties in the form of colouration. This species is common all over India. The fangs are smaller than those of the cobra, and the poison is not so rapid in its action, but it is very dangerous and destructive. It is found in the fields, in grassy plains, rice fields, low, scrubby jungle, and among

débris of wood and buildings. It insinuates itself into houses, into the bath-rooms, verandahs, on the ledges of doors, in book-cases and cupboards: in such situations it not infrequently causes fatal accidents. *Lycodon aulicus* is sometimes mistaken for it, but the least examination detects the difference. The scales along the dorsal region are hexagonal and very characteristic. The krait rarely attains the length of four feet.

*Bungurus fasciatus*, is larger than *cæruleus*, and is beautifully marked with rings of yellow on a dark steel-blue ground. The metallic lustre of the skin is very beautiful; its body is of a triangular shape, and it has hexagonal scales along the dorsal ridge. It is tolerably common in Bengal, Burmah, and Southern India, and is known in the north-west. It is found in the open country, in grass, in low jungle, and in the fields in holes in the ground, sometimes deep down among the roots of trees; it sometimes finds its way into a native hut. It feeds, like the krait, on small animals, mice, birds, frogs, lizards, probably on small snakes, and even insects. It is not very aggressive, but when attacked, retaliates fiercely. It lies coiled up, and when disturbed, jerks itself out like a spring, but does not extend its whole length of body.

*Xenurelaps* has only one species, which is closely allied to *Bungarus*. It is very rare, and consequently not destructive to human life.

The genus *Callophis* has several species in different parts of India, which are all more or less brilliantly coloured. They are not aggressive, and bite reluctantly, so it is sufficient to enumerate some of the species: *Callophis intestinalis*, *C. Maclellandi*, *C. anularis*, *C. trimaculatus*, *C. nigrescens*, *C. cerasinus*, and probably others.

The viperiform sub-order has two families, *Viperidae*, or vipers, and *Crotalidae*, or pit-vipers. The former is represented in India by two genera, *Daboia* and *Echis*, each of which has one Indian species, viz., *Daboia Russellii* and *Echis carinata* or kuppur. *Crotalidae* has several genera; *Trimeresurus*, with seven species; *Peltopelor*, one species; *Halyis*, two species; *Hypnale*, one species. These snakes are all venomous, but cause few deaths.

The *Daboia Russellii*, sometimes called cobra-monil and chain viper, is a very beautiful snake; it is of a light chocolate colour, with large, black, white-edged rings; a yellow line is on each side of the upper surface of the head, converging on the snout; rostral and labial shields yellow

with brown margin, a triangular, brown, black-edged spot behind the eye; ventral surface yellowish, or marbled with more or less numerous semi-circular brown spots, on the hinder margin of the ventral shields. It attains a considerable length, forty to fifty inches. It is common in Bengal, the south of India, Ceylon and Burmah, and probably may be found all over the plains and on the hills, up to 6,000 feet, in Cashmir, but its usual habitat is lower.

Fowls bitten by it sometimes die in less than a minute. It is nocturnal, is sluggish, and does not readily strike unless irritated, when it bites with great fury; it hisses fiercely and strikes with great vigour. Its long movable fangs are very prominent objects, and with them it is capable of inflicting deep, as well as poisoned wounds. It does not appear to cause many human deaths, but its misdeeds may be sometimes ascribed to the cobra. The daboia is said to kill cattle when grazing, by biting them about the nose or mouth. In proof of its sluggish nature, there is a well authenticated story of a young person having picked one up, and mistaking it for an innocent snake, carried it home. Its true nature was discovered when it bit a dog. It had not attempted to injure the person who carried it.

There is only one Indian species of *Echis*, *Echis carinata* (kuppur, afae). This snake is much smaller than the daboia, but grows to the length of 20 inches or more; it is terrestrial. It is found in the North-West Provinces, Punjab, Central Provinces, Scinde, and generally in the south of India, in the Anamally Hills, in the Carnatic, and in the vicinity of Madras. It is of a brownish-grey colour, with a series of quadrangular or sub-ovate whitish spots, edged with dark brown; a semi-circular band on each side of the dorsal spots enclosing a round, dark-brown, lateral spot; a pair of oblong, brown, black-edged spots on the centre of the head, converging anteriorly; a brownish spot below and a broad streak behind the eye; ventral surface, whitish, with brown specks. The scales are keeled; those on the lateral series have their tips directed downwards obliquely; the friction of these against each other causes a peculiar rustling sound.

The *Echis* is a very fierce viper; it throws itself into an attitude of defence and offence, coiled up like a spring, rustling its carinated scales as it moves one fold of the body against another. It does not wait to be attacked before darting its head and body at its enemy, the mouth wide open, and the long fangs vibrating, presenting a most menacing

appearance. It is very poisonous; the virus is of the same character as that of daboia. There can be little doubt that it destroys many human lives, as men are much more exposed to contact with it than with the daboia. It is said to live largely on the *scolopendridae*, but probably it preys also on small mammals, frogs, and small birds. In some parts of India it is probably chargeable with a considerable number of deaths.

Pit vipers (*Crotalidae*) have several genera in India. They are less dangerous than their American congeners, but are all poisonous. They are remarkable for the pit or depression between the eye and nostril in the loreal region, the triangular broad head, and short, thick body.

*Hypnale* is the only Indian genus or species with any vestige of the caudal appendage, which has given the name of rattlesnake to certain American *Crotalidae*, and in this species it is reduced to a horny spine at the end of the tail.

Many of the Indian *Crotalidae* are arboreal snakes, and in colour resemble the foliage and branches of the trees in which they live. The Indian genera are:—*Trimeresurus*; *T. gramineus*, *T. erythrus*, *T. carinatus*, *T. anamallensis*, *T. monticola*, *T. strigatus*, *T. macrosquamatus*; *Peltopelorus*; *P. macrolepis*; *Halys*, *H. himalayanus*, *H. Elliotti*; *Hypnale*, *H. Nepa* (or carawilla). The bites of most of these do not seem to differ much in their effects from those of the English adder, except the *Hypnale nepa*, or carawilla of South India, which is more dangerous.

There remains only to notice briefly the pelagic colubrine snakes, or *Hydrophidae*. They may be recognised at once by their peculiarities. With one or two exceptions they are all venomous, and inhabit the sea, the salt-water estuaries, and the tidal streams. They have a very wide range of distribution in the Indian and Pacific Oceans. They have a great variety of form, but the transitions are very gradual; some attain a considerable length; I have not seen one of more than five feet, but no doubt they often exceed this. They are very poisonous, and though accidents are rare, yet fatal cases are on record. The fishermen and sailors on the coasts know their dangerous properties, and avoid them.

The *Hydrophidae* have smaller heads, jaws and fangs than the land snakes; the fangs have open grooves in some, but not all. The virus is very active, and appears to operate as speedily and certainly as that of the land snakes. They have an elongated body like the latter; in some instances it is

short and thick; in others it is very thick towards the tail, and most disproportionately elongated and attenuated in the neck, whilst the head is very minute. The colouration is varied, often brilliant and beautiful. The hinder part of the body and tail is flattened and compressed vertically, almost like the fin or tail of a fish, and they swim with ease and rapidity. When thrown on the land by the surf, as they frequently are, they are helpless. Their food is fish and small aquatic creatures. There are certain parts of the Bay of Bengal where they may be seen in great numbers, and their movements in the blue water are agile and beautiful. There are four genera in the Indian seas; *Platurus*, *Enhydrina*, *Pelamis*, *Hydrophis*. *Platurus* has two species, *P. scutatus* and *P. Fischeri* (Bay of Bengal, tidal streams near Calcutta). This genus has several characters of the land snakes, e.g. well-marked ventral shields; body sub-cylindrical, and not compressed like *Hydrophis*; the colour is black, tinged with yellow.

*Enhydrina* has only one species, *Enhydrina bengalensis* (valakadyen); it is very poisonous; body and tail compressed, belly carinate; colour, bluish-grey, with dark bands of same, though deeper colour; no ventral shields. *Pelamis* has only one species, *P. bicolor*. This is one of the most remarkable sea-snakes in the Bay of Bengal; no ventral shields, body flattened, yellow sides and belly, back black; it is called kullundur, and is very poisonous.

Of *Hydrophis* the species are numerous; in the Indian seas about thirty have been described, and there are probably others. They present a considerable variety of form and colouration; some have elongated necks and small heads, the posterior part of the body being larger than the anterior: others have not this characteristic, but they all have a strong family likeness, and may be recognised at once by their compressed bodies, fin-like tails, and the general absence of well marked ventral scutæ. Their colouring is also remarkable, green, yellow, black, in bands or rings being a common pattern. They are pelagic, though they enter the tidal rivers; they seldom live long in captivity.

The mortality from snake-bite in India is very great. The average loss of life during the eight years ending 1887 has been 19,880 human beings, and 2,100 head of cattle yearly. Mr. V. Richards said the cobra causes nine-tenths of the human deaths. The snakes which are most destructive to life are so probably in the following order:—the cobra,

*Naga tripudians*; the krait, *Bungarus ceruleus*; the kupper, *Echis carinata*; Russell's viper, *Daboia Russellii*; the hamadryas, *Ophiophagus elaps*; the raj-samp, *Bungarus fasciatus*.

#### DEATHS FROM SNAKE-BITE IN INDIA IN 1889 AND 1890.

In Madras in 1889, 1,587 human beings, and 2,037 cattle were killed; 340 snakes were destroyed at a cost of Rs. 49. In 1890, 1,424 human beings, and 1,852 cattle were killed; no snakes were destroyed.

In Bombay in 1889, 1,080 human beings, and 74 cattle were killed; 433,795 snakes were destroyed at a cost of Rs. 7,848. In 1890, 1,075 human beings, and 100 cattle were killed; 406,092 snakes were destroyed at a cost of Rs. 7,136.

In Bengal in 1889, 10,681 human beings, and 480 cattle were killed; 41,189 snakes were destroyed at a cost of Rs. 3,439. In 1890, 10,534 human beings, and 538 cattle were killed; 41,115 snakes were destroyed at a cost of Rs. 3,742.

In the North-West Provinces and Oudh, in 1889, 6,445 human beings, and 221 cattle were killed; 25,663 snakes were destroyed at a cost of Rs. 3,137. In 1890, 5,798 human beings, and 247 cattle were killed; 24,083 snakes were destroyed at a cost of Rs. 2,902.

In the Punjab, in 1889, 915 human beings, and 87 cattle were killed; 68,501 snakes were destroyed at a cost of Rs. 8,232. In 1890, 834 human beings, and 32 cattle were killed; 29,941 snakes were destroyed at a cost of Rs. 4,313.

In the Central Provinces in 1889, 1,063 human beings, and 14 cattle were killed; 1,395 snakes were destroyed at a cost of Rs. 558. In 1890, 1,041 human beings, and 54 cattle were killed; 1,554 snakes were destroyed at a cost of Rs. 565.

In Lower Burma in 1889, 208 human beings, and 689 cattle were killed; 6,178 snakes were destroyed, but no rewards given. In 1890, 223 human beings, and 731 cattle were killed; 6,319 snakes were destroyed, but no rewards given.

In Assam, in 1889, 230 human beings, and 71 cattle were killed; 395 snakes were destroyed at a cost of Rs. 23. In 1890, 214 human beings, and 257 cattle were killed; 478 snakes were destroyed at a cost of Rs. 14.

In Coorg in 1889, 1 person was killed; 14 snakes were

destroyed at a cost of Rs. 3. In 1890, 2 cattle were killed; 26 snakes were destroyed at a cost of Rs. 6.

In the Hyderabad Assigned Districts in 1889, 216 human beings, and 120 cattle were killed; 76 snakes were destroyed at a cost of Rs. 26. In 1890, 191 human beings, and 132 cattle were killed; 113 snakes were destroyed at a cost of Rs. 29.

In Ajmere and Merwara in 1889, 53 human beings were killed; 224 snakes were destroyed at a cost of Rs. 10. In 1890, 78 human beings and 3 cattle were killed; 192 snakes were destroyed at a cost of Rs. 20.

In Bangalore, in 1889, 1 human being was killed; 645 snakes were destroyed at a cost of Rs. 231. In 1890, no human beings nor cattle were killed; 746 snakes were destroyed at a cost of Rs. 277.

Throughout India, in 1889 there were 22,480 human beings and 3,793 cattle killed by snakes, while 578,415 snakes were destroyed at a cost of Rs. 23,556. In 1890, there were 21,412 human beings and 3,948 cattle killed; while 510,659 snakes were destroyed at a cost of Rs. 19,004.

“The average result for all the Provinces, shows a mortality of one to every 10,155 of population in 1890, as compared with one to every 9,673 in 1889. The Provinces which showed the greatest loss of life from snake-bite in proportion of population (excluding Ajmere and Merwara) are Bengal (1 to 6,731), the North-West Provinces and Oudh (1 to 8,094), and the Central Provinces (1 to 10,350). The lowest mortality (about 1 to 25,000 of population) occurred in Madras, the Punjab, and Assam.”—*Report of Indian Government, 1891.*

As regards the measures to be adopted for reducing the annual loss of life by snake-bite, the chief points are to make known the appearance and habits of the poisonous snakes, and to institute proper rewards for their destruction. With a plain description, and a faithful representation in colour of each species, such as the Government of India have been put in possession of, the people can easily be made acquainted with the characters that distinguish the venomous from the harmless snakes, and thus learn to avoid or to destroy them.

Until some measures are more uniformly resorted to, there will be no material diminution in the loss of human life from snake-bite, which cannot now be rated at less than 20,000 annually.

It is satisfactory to find that the Government of India are insisting upon the institution of measures having for their object the destruction of snakes. It is, however, to be feared that the last measure proposed, *i.e.*, the cutting down and clearing away of jungle in the vicinity of villages, can hardly be expected to have the desired effect, for the reason that the poisonous snakes do not frequent the sort of jungles that surround villages so much as they do other localities, such as ruins, holes in walls and in the ground, grass and cultivated fields, &c., and that also the probability is that a great proportion of bites are inflicted far from the villages, where such clearances as those proposed by Government could not be effected, even were they useful. I would suggest that a reward should be given for each poisonous snake and for no other; there can be no difficulty in identifying them. This could only be effected by an organised system carried out generally in every district in which poisonous snakes exist. If it were decided to try this plan as universally as it is proposed to cut down the jungle, it is probable that a diminution of the evil might be expected; but whatever efforts are made they should be universally sustained and continuous. It is quite admitted that the problem is a difficult one to solve, but no effort should be spared to mitigate what must be regarded as a preventable cause of death.

The PRESIDENT (Sir G. G. STOKES, Bart., V.P.R.S.)—I may take it for granted that the meeting returns its cordial thanks to Sir Joseph Fayrer for this Paper which deals not only with matters of great interest, but seems to raise points of much practical importance. I trust those present who are familiar with the subject will now take part in the discussion.

Admiral H. D. GRANT, C.B., R.N.—With regard to the question of serpent worship in the present day, it may be of interest if I describe a scene which I witnessed in Java a few years ago. I was invited by a Chinaman to go and see this worship in a temple. On arriving I found at the end of the temple an altar with an extraordinary representation of a serpent; outside there was a huge platform erected which overhung the supports in such a way that it was very difficult for anyone to get on to it. This platform was covered with baskets of raw meat and small animals—



pigs and fowls, sheep and geese—and vegetables, besides which they had representations of the serpent in various forms. There was also a representation resembling the pictures of the brazen serpent of Scripture. A sort of ceremony of a strange and weird nature then took place in the temple, and all sorts of noises with dancing and the beating of tomtoms. On a given signal there was a tremendous rush for the platform, and a detachment of cavalry had to be called out to prevent the people stabbing one another, so great was the excitement; ultimately an athletic fellow managed to throw himself up on the platform and then there was a tremendous scrimmage all over the Temple Square. This was supposed to be pure serpent worship; it was entirely confined to the Chinese—the Javanese took no part whatever in it.

Allusion is made in the Paper to the mongoose. I have repeatedly seen fights between a snake and that animal. It was one of the amusements in camp to get a snake entrapped and see it fight with a mongoose. On such occasions I noticed that when the snake was let out of the basket he seemed to divine that there was a mongoose in the room. I used to take my mongoose—a pet animal—under my arm and directly he was released he would walk right round the mess-room without noticing the snake, while the snake stood in the middle of the room (I use the word *stood* advisedly, for it erected its head, spitting in all directions). But the mongoose walked slowly round the room, and one could hardly notice the difference in the radius of the circles he made, but each turn brought him nearer and nearer to the snake; when, in an instant, before you could realise what it had done, the mongoose had caught the snake by the back of the head and killed it or stunned it;\* it was very rarely that it was killed outright, but it was disabled and then the mongoose would begin to play with it like a cat does with a mouse. In regard to the tradition about its taking an antidote, I do not believe it for a moment. I have always seen the mongoose return to the attack and have never seen it take anything like an antidote such as I have heard described.

Dr. J. S. PHENÉ.—Judging generally, the Paper gives one the impression that there is very little evidence of Serpent Worship in Europe. I take it that means at the present time. In times of

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\* The American prairie dog acts in a somewhat similar manner.—Ed.

the Greek mythology it was so abundant that you cannot go into the British Museum and see a model of a deity without finding the emblem of the serpent. In days far off they kept living serpents in their temples where they were fed, reminding one of the old story of Eden, not that the serpent is there represented as taking food, but still something that is akin to it. It was said that in Egypt there was very little evidence of serpents being treated with greater honour than any other animal, and yet I do not think you will find an Egyptian temple (generally speaking, at all events), without a serpent, very like a cobra, in the height of exaltation. It struck me as very remarkable, on going through and examining the churches of the Pyrenees, in which they had done away with many of the symbolical carvings, to see the exaltation of the serpent about the cross. In a work I once came across in the library of Edinburgh University, I found evidence given of the prevalence of serpent mounds and worship in Europe, and in the Pyrenees there are those serpent mounds and they have been in all cases Christianised, if I may say so; *e.g.*, a church has been built on them and there is generally a cross. One has been carefully excavated and interesting remains have been found of the earliest and rudest form, which are now in a local museum. Up to the present day in the Pyrenees there is practised an extraordinary religious ceremony of burning living serpents on a particular day, attended by a procession with chanting. Serpent mounds exist in the British Isles, in America, Spain, France, Egypt, etc.

Major-General Sir RICHARD POLLOCK, K.C.S.I.—I should like to ask one question that interests me, as an old judge and magistrate in India. It used to be asserted that some of the cases (a small proportion) that were attributed to snake bites were caused by other poisons criminally administered. People came forward saying death resulted from snake-bite, and I should like to know if it is easy to ascertain the difference between the virulent poison of a snake and other poisons used in India, and whether you could suggest any improvement in regard to checking or detecting such malpractices as I have alluded to.

Surgeon-General W. B. BEATSON, M.D.—Sir Joseph Fayrer, having mentioned my name and pointed to the portrait of a snake that I had the good fortune to send him from Nagpore, I will say a few words, principally to relieve the minds of many people who may

be going to India. I think I have known one or two cases of young ladies refusing eligible matrimonial alliances because they did not like to go to a place so full of snakes. No doubt in India there is great mortality from snake bite; but I was a medical officer in India for thirty years, and at civil stations for sixteen or seventeen years; in Eastern Bengal, abounding in snakes, and at Nagpore in the Central Provinces where there are plenty of ruins and holes abounding in snakes, and I had not, during my thirty years' experience, to treat a case of snake bite. No doubt many natives were bitten, but snake bite is so fatal that the cases do not come in for treatment. I have had snake-bitten bodies brought in by the police for examination, and I have been able to find the snake bite, and to prove that it was the cause of death. The only case of snake-bite in a European I can recollect was in Lahore. During the last year of my life in India a party of soldiers encamped in the neighbourhood of Lahore, one of whom while in bed put out his hand, touched a part of the tent and immediately called out that he was bitten. His comrades rose and, running to his assistance, killed the snake, which was an *Echis Carinata*—it was smaller than the one shown on the diagram. They promptly put a ligature on the man's arm and I believe he was treated with ammonia. I had no part in the treatment; but I believe the man lived without symptoms for two days with the ligature on his arm. They then began to think it was a false alarm, and took off the ligature, fearing the hand would mortify. Immediately the ligature was removed the poison began to act and the man died shortly after. That shows how fatal and irremediable are the effects of snake-bite. I have seen very few snakes except in the hands of snake charmers, and I think I could count all the poisonous snakes that I have seen at large, on the fingers of my two hands. The snake I have referred to was, I think, eight feet long—I believe that is the largest cobra Sir Joseph has ever seen, is it not?

Sir JOSEPH FAYRER.—It was not more than six feet.

Dr. BEATSON.—Sir Joseph doubtless knows best but I certainly thought it was more than *six* feet.—But for Sir Joseph Fayrer having written his book on snakes, I think probably that snake would be living now, and would have killed many hundred people. I do not know how old it was, but the snake man who caught it, and gave it up to me with great reluctance, said it was probably a hundred years old and might be two hundred.

With regard to the destruction of snakes, it is a very hopeless matter. Not only is it impossible to hunt through the ruins and holes and jungles of India, but another great difficulty, I believe, is that when rewards are offered, natives are induced to keep snakes and to breed them for the sake of obtaining the reward.

Sir JOSEPH FAYRER.—That would not pay.

Dr. BEATSON.—I have heard that it has been done, and I think I remember reading, in the Indian Annals of Medicine, the story of a collector who, having to pay such rewards became so encumbered with dead snakes that he did not know how to dispose of them. I fear there is no hope of exterminating snakes so long as India is the country it is now.

Mr. LEONARD.—I was going to ask Sir Joseph Fayrer if destroying the jungles round the Provinces is for the purpose of diminishing the number of snakes or for sanitary purposes?

Sir JOSEPH FAYRER.—For the destruction of snakes, as well as for sanitary purposes.

Mr. LEONARD.—We cannot help seeing that although there are many deaths from snake-bite in India, the Government has done wonders in killing half a million a year in Bengal; and, while deploring the amount of suffering and deaths caused, at the same time no doubt Sir Joseph Fayrer himself would admit, that there is great credit due to the Government of India for the enormous efforts they have made in trying to keep down the mortality.

Surgeon-General CORNISH, C.I.E.—We are much indebted to our friend Sir Joseph Fayrer for the very admirable Paper he has read on this occasion. I have only a few observations to make, and chiefly with regard to Serpent Worship in India, where it goes on practically to this day, as I have had evidence under my own eyes.

In the neighbourhood of Madras there is, at this moment, a native temple belonging to some of the Sudra castes, where live snakes are kept in considerable numbers about the premises, and where there are regular feasts and festivals, when these snakes are regularly fed and worshipped by the people. The case is mentioned by Surgeon-General Balfour. In the Northern Circars there is a town called Cajamundri where I was walking one morning on the outskirts and found a large ant-hill six or eight feet high, which was stuck all over with representations of the

cobra, cut out rudely on wood. I suppose there were fifty or a hundred of those wooden representations on the ant-hill. I made inquiry and I heard of a well-known cobra in the neighbourhood, and that numbers of people fed the beast regularly, and that it was an object of worship, especially by those ladies who desired to be fruitful—I believe that is a common mode of serpent worship to this day in India.

With regard to the mortality caused by different kinds of snakes, as Sir Joseph Fayrer says, the cobra, no doubt, is the most fatal snake. I have only seen one instance of poisoning from the *daboia*. I once killed a *daboia* in my own garden, which I sent to the Museum as a specimen. It was 4 feet 5 inches in length. It did not kill its victim; but the man bitten seemed to have his whole blood disorganised. Blood came from under his skin, and he was in a dreadful state; but he ultimately recovered.

The *Echis*, which is common in Madras, I have never seen any case of poisoning from. It certainly comes into houses, because I have killed one in my own house, but I have never seen a case of biting by the *Echis*.

I had on one occasion a very remarkable case of poisoning by a *cobra*. I was in my garden one morning, when I directed my gardener to take away a lot of old broken bricks from the side of a wall. On taking them away I saw one of the men withdraw his hand suddenly, and he came to me immediately and said he was bitten by a snake, and I discovered the marks of two fangs. I immediately adopted the usual treatment: I put on a ligament and incised freely the two fang marks, and made the man himself suck the wound. Although it did not take a minute from the bite to the operation the man was certainly poisoned by the snake, and he began to suffer from all the symptoms of snake-bite. I supported him with brandy and ammonia and took him off to the hospital, and he ultimately recovered, but he was very bad for two days. The snake was killed in my presence, and proved to be a young cobra, nearly five feet long.

The AUTHOR.—I do not think there is very much for me to say except to thank those who have added so materially to the interest of the evening by the remarks they have made, especially those connected with Serpent Worship. They are most interesting and valuable.

I could not in the space of time at my command give a disquisi-

tion on Serpent Worship and the whole history of snake bite. I therefore tried to select the most interesting parts of each subject. I am bound to say that I have heard some very interesting matter, especially from Dr. Phené, and am much obliged to him; there would be ample material for a long Paper on Serpent Worship alone, omitting zoological description.

Sir R. Pollock asked if some of the cases which were attributed to snake-bite were not due to other causes. Very likely, but even when those are accounted for, a very large number of snake-bites remain, and when I say 20,000 a year, I am certain that is far within the death rate that occurs. Long ago, I took the trouble to investigate the question, from the best returns I could get, and made allowance for such causes as Sir R. Pollock has alluded to.

Dr. Beatson referred to the rarity with which snakes are seen. Many people do not see them because they do not go into the localities where they are. It is chiefly amongst the natives who live in huts and walk about bare-footed at night, and those who are engaged in agriculture and who sleep in huts where the cobra may drop from the roof, as in the case we have heard of, where the man stretched out his hand and was bitten. In the rainy season snakes congregate in dry places, and such are the localities where the people suffer.

I was much interested in what Admiral Grant said, especially about the mongoose. He is perfectly right in implying, as I understand; that the mongoose has no more immunity from snake-bite than any other creature. The cat for instance, is reputed to have nine lives, and it is supposed to take more to kill a cat than any other animal, but I have seen a mongoose and cobra fight for hours when they have been shut up in the same box, and both have escaped serious damage by their agility—the cobra has kept out of the way, and the mongoose has simply been scratched. If you take a hypodermic needle and scratch a person's arm you do not poison him, but if you introduce the needle deep into the flesh and then inject the poison it takes effect, and when that happens to the mongoose, the pig, cat, or dog, or whatever the creature, or man, by the cobra bite, death is inevitable, or if the creature does not die it is because it has not got a sufficient dose of the poison or because the snake has been partially exhausted before biting. All the stories told of the mongoose going out and taking an antidote are interesting, but they are not true.

Mr. Leonard, I think, spoke of the action of Government in dealing with this important question of snake-bite. I am sure no one would do more to recognise, or to do justice to the Government of India than I would, and if I appear to criticise it, it is not because I think they have not done enough, but because I think their efforts have not always been well directed. When you find in one place all attempts to do anything repudiated as useless, and elsewhere a good deal done, there is a want of consistency. I am satisfied that if sustained efforts were made to destroy these creatures the evil would, to a great extent, be remedied; and that there can be any difficulty in recognising the poisonous snakes I cannot believe—for anyone who runs may read. I can imagine that here and there you may find one snake looks like another, but the moment you investigate you can distinguish the venomous from the innocent. And if an organised and a systematic attempt were made to destroy the creatures, it would be beneficial.

I do not agree with Dr. Beatson, that they breed snakes. It would be far too costly, but money *is* wasted in giving it away for innocent snakes. It is easy to recognise the four or five kinds that do mischief.—I am convinced that the evil might be mitigated. That poisonous snakes could be exterminated altogether I have never suggested, nor do I believe it; but the death rate from snake bite might be brought down from tens of thousands to a much lower number. I am glad to see that the returns from India show the desire to do something. They have said “We will not pay more money for snakes; but we will cut down all the jungles round the villages.” That would do if all lived there; but they do not. Any efforts made I recognise with gratitude; but I do not think the authorities have always quite realised the necessity as much as they might have done. 20,000 or 25,000 deaths from snake-bite may not seem so great a number out of 250,000,000 people, but it is one in ten thousand or twelve thousand, and if we had such a proportion in this country we should certainly cry out about it.

I was interested in what Surgeon-General Cornish said. It has never been quite clear to me what amount of mortality is due to the *Echis*. I know it is a very deadly snake, and the poison is very virulent, but it can be easily avoided, for it is very demonstrative, and makes its presence known by its rustling. The *daboia* is a more silent and dangerous snake and, happily, is not so common;

what Dr. Cornish said about the condition of the blood of the person bitten is typical. In the case of the cobra it is more of a nerve poison, but both kinds are exceedingly deadly and equally to be dreaded; but the cobra kills his tens of thousands. The *kerait*, perhaps, kills the next largest number. He gets into crevices and open doors and drops on your arm, or you may put your hand into a box and find one. I remember a gentleman who lost his servant in a very sad way: he sent his man to fetch a bottle of soda-water and he was bitten by a snake; the man pointed to his hand and said he was bitten,—he died in two hours.

I am much obliged for the very kind way in which you have received my Paper. (Applause.)

(The Meeting was then adjourned.)

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#### REMARKS ON THE FOREGOING PAPER.

SURGEON-GENERAL C. A. GORDON, M.D., C.B., Q.H.P., &c., writes:—

The learned author of the Paper on "Serpent Worship, and the Venomous Snakes of India," has dealt with the question with his accustomed ability, and with intimate knowledge in regard to the several phases in which he has discussed it; but inasmuch as the scope of his remarks had perforce to be compressed within the limits of an address, he was of necessity debarred from touching upon all the points to which otherwise his attention and that of his hearers would have been directed.

In reference to some of these points I would offer the following remarks:—

*Snake Worship.* In the Gold Coast of Africa, the Fantee natives were said to have among them several snake temples, with each of which priests, otherwise fetish-men, were connected. To these



temples, or rather mud-huts, with reed or leaf covered roofs, the people resorted from time to time, and there, through the medium of the "priests," presented their offerings to the snakes. The latter, which were said to be kept in the temples in considerable numbers, were of the python or boa tribe, and tame; their ordinary resting place the rafters of the hut. They appear to have been worshipped as incarnations of evil, the object of the worshipper being the aversion of misfortune or other evil with which he was threatened. There were also certain incarnate beneficent spirits to which the Fantees made their devotions and offerings, and certain inanimate objects, more especially stones and isolated rocks, were declared *fetish*, and as such had sacrifices and oblations offered to them.\*

*Exceptional instances.* Although the point has not been actually demonstrated, yet analogy indicates the possibility that the effects of snake poison may not be equally pronounced in all persons. This question is unsuited for discussion in notes like the present, but the mere allusion to it may perhaps lead elsewhere to its elucidation. With regard to constitutional peculiarities, it is admitted that in respect to certain animals they bear some relation at least to colour; thus while in some geographical regions those that are white are injuriously affected by particular indigenous plants, those that are black escape altogether. A similar peculiarity is said to exist in regard to the poison of the rattle snake (*Crotalus horridus*) in pigs, the bite of the reptile being fatal to those of one colour, but not so to those of another.

This being the case, presumption is in favour of the account given by Dr. Honigberger, for many years surgeon to the Maharajah Runjeet Singh, "the Lion of the Punjab," to the effect that instances of such immunity against snake-poison had come under his personal observation. Dr. Honigberger's book† is not now accessible to me, but my recollection is that in it such cases of exemption are related; nay more, that the persons alluded to, not only remained unaffected, at least for some time, but after an interval, varying from weeks to months, experienced a desire to be again bitten by the same species of snake, namely the cobra (*Naja tripudians*) for which purpose they resorted to the jungle, there to seek out the reptile. According to Dr. Honigberger, after a few repetitions of this remarkable process, the subject of the idiosyncrasy would fall into general ill-health, and so die. And no wonder. Perhaps further observation and inquiry in the Punjab may tend to confirm, or to refute the correctness of the views thus quoted.

*Fictitious cases of snake-bite.* That such cases may occur is in accordance with analogy and with experience, though doubtless in itself a circumstance of extremely rare occurrence. Of the affec-

\* Such was the case, at least in 1847-8, during which period I served on the Coast.—C. A. G.

† *Thirty-five Years in the East.*

tions capable of being simulated or fictitious, hydrophobia is acknowledged to be one. Of "snake-bite," a remarkable and striking instance was some years ago related to me by Dr. Henderson, then of the 3rd Light Dragoons, in whose person it took place. Suffice it here to observe that Dr. Henderson was a man of strong nervous, as he was of physical, power. While on the line of march with his regiment in India, he had occasion to insert his hand into a box in which were various articles packed in straw. No sooner had he disturbed them than from the box a cobra darted past his hand and so escaped; as it did so he instinctively withdrew his hand, and so observed that blood trickled from a wound upon it. He immediately experienced the symptoms characteristic of cobra-bite; nor was it until the regimental assistant surgeon, who had meantime visited him, convinced him that the wound was caused, not by a cobra, but by a projecting nail that he began to recover from the alarming, though fictitious, symptoms from which he had suffered. May not the question be asked—Are there many such cases on record?

*Snakes in relation to medicine.* From very ancient times, down to and at the present day, serpents, poisonous and innocuous, have been used in medicine, not only among uncivilised people, but among those who possessed all the advantages of civilisation. The *tereak* of the ancient Arabian physicians (whence is derived our word *treacle*); the *theriaca* of Andromachus, variously known at the present day under the names of *treak farook*, and Venice treacle, were and still are, believed to owe their antidotal power against poisons, and curative properties in certain diseases, to the presence in their composition of the dried flesh of vipers or other poisonous snakes.

According to *The Dispensatory*, published in London in 1746, among other constituents of *Theriaca Andromachi*, squills, long pepper, opium, and *dried vipers* were enumerated.

According to Chinese therapeutics, "From the habit of a" particular, but unnamed species of venomous "snake, to seek refuge in hedges and crevices, it is concluded that, mixed with other drugs, it introduces itself into the most secret places of the body," and so exerts its curative properties. Several species of snake are used by the Chinese medicinally, or as food. The viper in particular is conveyed about for sale in baskets, tubs, or jars, either alive or made into broth.

In the islands of the Malayan Archipelago the fat of snakes is held to be a sovereign application to wounds. Even in our own country there are districts in which the flesh of the native viper (*Vipera Berus*) is applied locally, in cases of bite of that reptile.

## SPECIAL COMMUNICATION.

AUGUSTUS MUELLER, Esq., M.D. of Yackandandah, Victoria, Australia, writes :—

1 It may appear an act of presumption on the part of an obscure Australian country practitioner to offer adverse comments on a paper read by so high an authority on snakes and snake poison as Sir Joseph Fayrer, but as the paper has been sent to me by an Australian scientist for the purpose of eliciting some comments, and as the author has not referred to the results of some recent scientific research on the subject of snake poison, I would, in the interest of science, venture on the task. I agree with Sir Joseph Fayrer that snake poison kills by extinguishing the source of nerve energy, if by this definition he means a merely functional depression or suspension of the motor and vaso-motor nerve centres without organic or structural changes in these organs. But I cannot agree that it is also a blood-poison and that the viperine poison is the most potent one. That all snake poison is a nerve-poison, that its action is purely dynamic, that it reduces in strength and in fatal cases completely suspends the currents of motor-nerve force, both from cell to cell and from cell to peripheral fibre, are scientific deductions that I venture to think can now no longer be called into question. I hold that it has also been proven that this suspension of motor nerve-currents is not accompanied by any structural changes either in the nerve tissue or the blood corpuscles, and that whatever changes occur are merely the result of this suspension and not owing to the direct action of the snake poison.

2 That the colubrine poison is a pure nerve poison, has been demonstrated by me as well as by the observations of medical men in Australia. We cure our patients suffering from snake poison in a few hours by strychnine injections, even when pulse at wrists and respiration have nearly and sometimes completely ceased, and when the quantity of strychnine required to rouse the paralysed nerve-cells into action exceeds what in the absence of snake poison would be a fatal dose. Patients regain consciousness and the use of their limbs at once, and in a very short time recover completely, without showing the slightest sign of blood-poisoning or any other structural lesion.

3 Of vipers we have none in Australia excepting the death-adder (*Acantophis antarctica*). Persons bitten by this deadly snake have also been treated with strychnine injections and with the same favourable result. This snake, however, is not a pure viper. It has permanently erect poison-fangs like our colubrines, but they are perforated like those of the vipers and not merely grooved like those of all other Australian snakes. It also has the body of a viper and its poison more nearly approaches the viper poison in its effects, as it acts with special emphasis on the vaso-motor

centres and the small ganglia in the vaso-motor nerve ends. Extravasations of blood around the bite and in the bitten limb, which are very slight after the bite of even the most venomous Australian colubrines, are more conspicuous, and sudden heart failure with consciousness only slightly impaired is much more frequent. Still, even the poison of this snake is purely a nerve poison, as the following case I select from a number of similar ones will show. A child of tender age was brought into a Queensland hospital, bitten, a few hours before, by a death-adder. The bitten part had been scarified, and a ligature applied above it. The limb was much swollen, but the child quite conscious. Having to attend an urgent case in another ward the house-surgeon left the child in charge of a nurse, but scarcely ten minutes had elapsed when the father of the child rushed into the ward and informed the surgeon that his son was dead. Surely enough, the boy appeared so on inspection, lying on a bed, livid, limp, and cold, without any perceptible pulse or respiration. Strychnine injections combined with artificial respiration were at once resorted to with the result that the child speedily revived and was discharged cured on the following day, showing no sign of blood-poisoning. Granting, however, that death-adder bites cannot be cited as viper bites, since the snake is not strictly speaking a viper, any doubts as to the viperine poison being a nerve-poison or a blood-poison are completely set aside by Feoktistow's experimental researches, which prove most conclusively that the viperine poison is also a pure nerve poison. The correctness of Feoktistow's experiments has been guaranteed to the writer by no less an authority than Professor Kobert, who took an active part in many of them, and kindly presented to him Feoktistow's brochure, an inaugural dissertation, not procurable by the ordinary channel of book-trade. It was published in 1888 after the writer had published his theory of the action of snake poison; and Australian and European researches, whilst arriving at the same conclusions, were carried on simultaneously but quite independent of each other.

4 Feoktistow experimented with the poison of Viperidae and Crotalidae only, and drew his supplies of poison from the richly stocked Terraria of the Universities of Dorpat and St. Petersburg, in which some 200 of these reptiles were kept. His snakes were: *Vipera Berns*, *Vipera Ammodytes* and *Crotalus durissus*, and the laboratories of the universities were at his command with the most elaborate of scientific apparatus. In opposing the theory of blood-poisoning, I will only cite one telling experiment.

5 The whole vascular system of an animal that had received a fatal dose of viperine poison was thoroughly washed out with the warm defibrinised blood of four animals of the same species, not poisoned, the blood being infused into the external jugular vein and allowed to flow out of one of the crural arteries. The

infusion was discontinued, when four times the volume of blood the animal carried in his normal state had passed through it, and every trace of the viperine poison, administered by intravenous injection, had been thoroughly removed. Still with blood completely free from poison, the paralysis of the nerve centres remained the same as before the infusion. The blood-pressure, artificially raised during the infusion, fell at once again, when it ceased and the vessels named were closed. The blood rushed again into the paralysed veins of the abdomen, leaving the rest of the body anæmic, and the animal perished.

6 The blood-changes, that have given rise to the theory of blood-poisoning, are very deceiving, and can all be explained on the ground of vaso-motor paralysis. The blood of a poisoned animal becomes almost stagnant in the paralysed and immensely engorged veins of the abdomen, to which the greater part of it is drawn. It becomes saturated with carbonic acid and assumes a very dark colour, but it speedily parts with the acid, and readily absorbing oxygen becomes bright red again, when slightly beaten in the air. No changes could be detected by Feoktistow in the blood-corpuscles under the strongest immersion systems. Only Bizzosero's blood-plates were not present in the usual numbers. The alleged discovery of Weir Mitchell of blood corpuscles in a disorganised condition, Feoktistow ascribes to his using alcohol immersions that give deceiving pictures.

7 It is only where the snake poison remains for some time in a concentrated form in contact with the blood corpuscles, as it does at and around the bitten part, that the stroma becomes dissolved and the hæmaglobin is set free, but this effect is nothing peculiar to snake poison, as it can be brought about readily by other non-poisonous substances and even by the blood of other species of animals.

8 The hæmorrhages that almost invariably take place under the influence of viper poison, but are also exceptionally observed in Australia in colubrine-poisoning, have their cause likewise in vaso-motor paralysis. The blood stream is retarded and the capillaries are dilated, both by the direct action of the poison and by venous engorgement. A capillary tube in its normal state, just wide enough to allow the blood corpuscles to pass through in a single row but in the expanded condition not wide enough for two abreast, becomes blocked through the corpuscles being wedged against each other. At the same time the stomata in the capillary membrane, through which only leucocytes can force their way in the normal state, become larger and permit the less elastic red corpuscles to pass out by diapedesis. Where the venous engorgement is very great, as in the abdomen, the capillary membrane bursts, and side by side with diapedesis there is capillary bleeding and the hæmorrhage becomes more or less profuse. The process of diapedesis has been minutely

observed by Feoktistow on the mesentery of animals sprinkled over slightly with a two per cent. solution of the poison. Whenever a drop of the liquid lodged, the blood-vessels were almost immediately dilated and small point-like extravasations of corpuscles became visible. Gradually enlarging, they became ultimately confluent, forming uniform hæmorrhagic surfaces of greater or less extent.

9 The hæmorrhagic process in viperine poisoning extends over all the internal organs, but more especially the heart, and in the pericardial sack there is generally a large quantity of a sanguiniferous liquid with numerous blood corpuscles in it. *Preparations of the capillaries from any part of the heart, fixed with chromic acid, show healthy corpuscles in them throughout.*

10 These observations seem to me to place it beyond doubt that the theory of blood-poisoning is not tenable, and that vaso-motor paralysis explains all the blood changes, none of which, moreover, are liable to cause death. The whole group of the phenomena called forth by the subtle ophidian poison, when introduced into animal or man, is thus brought under the operation of one law, the law of suspended motor nerve force, and science has once more fulfilled her noble mission. Walking, so to say, in the footsteps of the Supreme Intelligence that guides the atoms as it rules the worlds, science has discovered the plan and design that underlies the subtle action of snake poison, and by reducing the puzzling symptoms it produces to order, she has at last solved a problem that has for centuries past defied her researches. She has done even more than this. With the first problem solved, with an exact and strictly defined knowledge of the action of snake poison, it was a far less difficult task for her to select as a physiological antidote from the vast storehouse of nature a substance having a directly opposite action to that of snake poison on the human system, and this substance science has found in strychnine administered in large doses by hypodermic injection. Feoktistow, whose researches also led him to experiment with the drug, found it also decidedly antagonistic to snake poison. Unfortunately he confined his experiments, as far as can be learned from his work, to frogs only. Finding that strychnine did not re-establish reflex action in these animals, and that they died under the combined influence of the two poisons, he somewhat prematurely gave it up as the physiological antidote to snake poison, and being unable to find another one, despaired at the conclusion of his experiments of its being found, in the present state of science, whilst all the time it was lying ready at his hands. He did not sufficiently bear in mind, that experiments with a drug as variable in its effect on different species of animals as strychnine, more especially when made in combination with snake poison, equally as variable, must be necessarily misleading and unreliable, unless extended over a number of species and, if

possible, up to man. This conviction impelled the writer, after assuring himself of the correctness of his theory of the action of snake poison, to overstep all the animals and proceed straightway with experiments on man, by administering boldly and fearlessly strychnine in large doses to persons *in extremis* from snake-bite. When a human being is in this condition, and sure to succumb to the insidious venom under any treatment medical skill has yet devised, a little risk is justifiable, when it holds out a reasonable chance of success. I did not hesitate to take this risk by administering strychnine in one-sixth-of-a-grain doses, at short intervals, and the result I consider to be a glorious triumph of scientific deduction, now acknowledged as such throughout the Australian colonies, and ere many years have passed, to be acknowledged throughout the scientific world.

11 It is a subject of extreme regret, that a few unfavourable experiments with strychnine on snake-poisoned dogs, made years ago under unfavourable conditions, by experimenters having but vague notions of the *modus operandi* of snake poison, should stand in the way of the strychnine treatment being adopted in India, since there cannot be the shadow of a doubt that the fearful mortality at the rate of over 20,000 human beings per year, mentioned by Sir Joseph Fayrer, would be considerably reduced if the Indian Government not only made the use of the antidote compulsory to medical men, but also took steps to place it within the reach of the natives. Any intelligent person may be taught how to apply a ligature, cut out the bitten skin, and inject the antidote. There is scarcely a settlement in the Australian bush now where it is not kept in pocket-cases containing likewise hypodermic syringe and printed directions for use. Some of the vendors of these cases have added a strong ligature, a pair of scissors to cut out the bite, and even a patent sucker for the more effectual elimination of the poison. The Government did nothing in the matter, but the people helped themselves. Many cases of snake-bite are now successfully treated by laymen too far from the nearest medical man to procure his services in time, and it often happens that when the doctor arrives his patient is out of danger. The native population of India is not likely to follow the Australian example, and it therefore devolves on the Indian Government to initiate a movement so urgently called for, and much more likely to stem the great mortality from snake-bite in India than vain efforts to exterminate the reptiles.

12 That strychnine will save life under all circumstances, its most enthusiastic advocate does not venture to assert. No physiological antidote can be expected to accomplish that task, and a chemical one we are not likely to discover. Strychnine can cope with a fatal dose of snake poison, but when a quantity equal to three or four fatal doses, such as a powerful cobra gives off at one bite, has been absorbed, corresponding quantities of the antidote are

required; for the snake virus, when the system is thus saturated with it, re-asserts itself repeatedly, after having been temporarily subdued by the antidote, and each relapse has to be met by another injection, if not by two or three. In such cases the strain on the delicate nerve-cells, that form the field of battle between the two poisons, may become too great. Only a very robust constitution, as our Australian experience has shown, can withstand that strain, and emerge scatheless from the ordeal. The antidote is also powerless to save life, when it is administered after extensive internal hæmorrhages have taken place. But given a serious case of cobra-bite, in which at an early stage by prompt ligature and deep excision of the bitten part the poison has been partially eliminated, but which would nevertheless terminate fatally under any other treatment, there is not the least doubt in my mind that the strychnine treatment properly applied would cope with it as successfully as it has done with apparently hopeless cases of tiger-snake bite; for our Australian cobra, the tiger-snake (*hoplocephalus curtus*) is quite as deadly as the Indian cobra, in fact, quantity for quantity, its poison is even more so, but fortunately for its victims, given off less profusely.

13 The adoption of the strychnine treatment in India, as well as in all other countries infested with venomous snakes, is merely a matter of time, but time unfortunately means a terrible loss of human life. I therefore appeal most earnestly to Sir Joseph Fayer and other influential members of the medical profession to exert their influence in bringing about an early introduction of the treatment in India, on the lines laid down by me in these comments. My own efforts in that direction have been unremitting, and supported by two Australian Governors, but His Excellency the Viceroy had necessarily to refer them to the medical authorities, and the latter decline to move in the matter on the ground of a few experiments on dogs, the failure of which is easily explained. They cannot be weighed in the balance against the favourable observations made for the last four years in Australia, and the results of recent European research.

#### NOTE ON THE FOREGOING.

All will recognise that Dr. Mueller's statements are specially worthy of attention, and it is a subject for general congratulation that he has already gone so deeply into the task of seeking to give the world a remedy for a great evil.

As regards the remarks in the third paragraph of his com-



munication it is held that both viperine and colubrine venom are not only nerve poisons but blood poisons too, especially viperine. With respect to Feoktistow's experiments, the reptiles referred to are all viperines not colubrines; the poisons of these differ in their action.

In India, the greatness of the need of some remedy for snake bite has caused a large number of cures for the evil to be proclaimed as useful, among them the remedies mentioned in paragraph 11, and including that of strychnine, but it has been urged that its use has not proved successful there; this may perhaps be attributed to the fact that the remedy has not been so carefully used nor with that thorough acquaintance with its proper application that Dr. Mueller's experiments have shown to be so necessary, but it is known that Sir Joseph Fayrer (President of the Medical Board at the India Office) has very lately recommended to the authorities that it should be tried again: but, in dealing with the subject it must be borne in mind that, as a rule, Australian snakes are less venomous than those in India, at least Indian medical men have come to that conclusion.—ED.

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THE FIGURE WITH THE PLAN (KING GUDEA).  
(From De Sarzec, *Découvertes*, Pl. 19.)

## ORDINARY MEETING.\*

H. CADMAN JONES, ESQ., M.A., IN THE CHAIR.

The Minutes of the last Meeting were read and confirmed, and ;

The following Paper was then read by the Author.—[Since he did so the important results of subsequent researches induced him to much extend its limits and to add descriptions of certain valuable discoveries ; hence the Paper and discussion are now, 1893, in an amplified and perfected form.]

### NOTES UPON SOME OF THE RECENT DISCOVERIES IN THE REALM OF ASSYRIOLOGY, WITH SPECIAL REFERENCE TO THE PRIVATE LIFE OF THE BABYLONIANS.

BY THEO. G. PINCHES, ESQ.,

OF THE DEPARTMENT OF EGYPTIAN AND ASSYRIAN ANTIQUITIES,  
BRITISH MUSEUM.

#### I.

*Principally from an Inscription of King Gudea, about 2500 B.C.*

Toiling among the dust of ages, we Assyriologists fulfil, in our own domain, and as far as we may, that dictum which says, that mankind's own true study is man. But mankind's own study is not man only, but everything that pertains to him. The student of Assyriology therefore not only tries in his special line to answer all questions concerning him—his origin in his native land, his history, his surroundings, his thoughts, his feelings, and his religion—but he studies his language too, and tells of his joys and sorrows. Day by day the quest goes on, and the cloud-masses obscuring the vista are little by little cleared away ; and a time will doubt-

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\* 3rd meeting, 26th Session.

less come at last when the wide domain of Assyriology will have yielded up, as far as may be, its secrets, and the history of the human race and of civilization will present there no gap.

Upon an extensive waterway, known as the Shatt-al-Hai, which unites the Tigris and the Euphrates, and runs in a south-south-easterly direction from Kut-al-Amara to Suk-ash-Shuyukh, lies, shut in a bend in the waterway, a series of hillocks or mounds of which the principal, known as Telloh, marks the position of an ancient Babylonian temple or royal palace, from which, from time to time, fragments of sculpture, bricks, &c., have reached the western world. It is on this spot that M. Ernest de Sarzec, appointed Vice-consul of France at Bassorah in 1877, had the good fortune to discover some most important early Babylonian remains, which, by his energy and enthusiasm, have been most carefully and scientifically unearthed, under his direction, for the French government, who has made them accessible to scholars in a splendid publication\* prepared by MM. de Sarzec and Léon Heuzey, Keeper of the Oriental Antiquities at the Louvre.

This site has been long known to Assyriologists as representing the ancient city  $\rightarrow$   $\langle$   $\Xi$   $\Psi$   $\rightarrow$   $\langle$   $\langle$   $\Xi$  (*sir + pur + lā + ki* †) formerly read as Zirgulla, and identified with a site close by, known as Zerghul. As, however, I pointed out in 1883, the true reading is *Lagaš*, of which name Prof. Hommel has found the variant *Laḡaš* (with guttural *g*). This name, with its final *š*, is of importance, in that it implies Kassite or Cossaeian influence, and is parallel to the well-known ancient native name of Babylonia, namely, *Kar-duniaš*, in which the Kassite ending *š* occurs again. The modern name, *Tel-loh*, has been explained as a corruption of the Arabic  $\text{تلّ الوح}$ , *Tell-al-Loh*, "the mound of the writing-tablet,"—an explanation which has its probabilities. For my part, however, I am inclined to regard the second element, *Loh*, as a weakening of *Laḡaš*, with the loss of the terminal syllable (compare *Kar-Duni* for *Kar-Duniaš*) and the weakening or loss of the guttural *ḡ*. If this be the case,

\* *Découvertes en Chaldée*, par Ernest de Sarzec . . . , publié par les soins de Léon Heuzey . . . , sous les auspices du Ministère de l'Instruction publique et des Beaux-Arts. Ernest Leroux. Folio.

† Given by the syllabaries as *Sirpurlākiku*, the form used by the Babylonian scribes when dictating.

there is no need to suppose that the article *al* has been dropped by the Arabs, the correct form of the name being *تل لوه*, *Tell-Loh*, for *تل لوح* or *تل لوج*.

Though the name of *Lagaš* does not occur with very great frequency in the literature of Babylonia in general, yet, as it was a very important place, it was often mentioned under some other name, such as *Ninā*, *Girsu*, *Uru-azaga*, and *Gišgala*, the names, probably, of certain districts within the city. The principal name, after *Lagaš*, was *Girsu*. Thus we have, in the lists of the temples, "the 3rd temple (of Nergal) of Girsu;" "the temple of Nergal of Girsu;" and the temple known as "temple 60" was also in Girsu. *Lagaš* was renowned for a temple known as "the house of the great light of heaven," the 64th on the list of Babylonian temples. The cities of *Lagaš* and *Girsu* are also mentioned in incantations, of which one (K. 2726) reads as follows:—

*Tentative Translation.*

"O spirit of his (Ē-girsu's?) consort, lady whose heart is exalted, she who causeth suffering to go forth from a man, who perfecteth the body (?) with lordly clothing, O Bau, lady of Girsu, shining forth in *Lagaš*, mayest thou exorcise (the evil thing) in Niffer.\*

\* The wedge-text of this is as follows:—



The renown of Bau of Girsu and Lagaš was therefore so great that she was invoked at Niffer, of which city her consort Ninip (= Ê-girsu, "Lord of Girsu") was one of the principal gods—indeed they were both held in great esteem, as the deities of healing, all over Babylonia. Like most of the Babylonian deities, they were known under several different names, some of which occur in the following pages.

The splendid discoveries, so splendidly published, of M. de Sarzec, show us not only what ancient Babylonian architecture, art, and sculpture were, but also reveal to us somewhat of contemporaneous literature. And here a matter of very great importance may be noticed, namely, the Akkadian question. It is all very well for the anti-Akkadists of the continent to say that there was no such thing as an Akkadian language—that the inscriptions said to be written in that language are mere cryptographs—puzzles which the ancient scribes set their successors and themselves. Facts—hard, stony facts—do not bear them out. If there were Sumerians and Akkadians—and this no one denies—it is only reasonable to suppose that they had languages, and they certainly had no use for a cryptography. What king wishing the renown of his name to be spread abroad, and handed down to posterity (and this was a great thing with those who ruled in Mesopotamia of old), would write his records in a language or script which was bound to become so troublesome to read as to make them practically sealed books to the greater part of his people, even though they might know the character in which they were written? The Babylonian kings wrote inscriptions for their own glory, and they were not any more than the Assyrians the people to hide their light under a bushel. Now all the inscriptions from Tell-Loh are written in this so-called cryptography, known among the more reasonable Assyriologists as Akkadian, and the pictures which we now exhibit show what they are like, and with what painstaking

*Transcription of the Akkadian version:—*

1. Zi nitalamāni
2. ni ša-zu maḡa
3. êne
4. saga lu-gišgal-lu
5. unu kuba
6. nam-nunna ba-šul
7. D.P. Bau, ni Gir-su D.S.
8. Lagasa D.S. sig-êa
9. Ellil (D.S.)-gi ḡipā


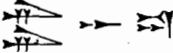




*Tentative restoration of the Assyrian version:—*

1. Niš [ḡamirti-šu]
2. bêl[ti ša libbi-ki štru]
3. muš[têšitu]
4. pu[ḡ amêli]
5. ša [makana lubušti]
6. [bêlūti suklulat]
7. [Bau, bêlit Girsu]
8. ša [ina Lagaš šūpat]
9. [ina Nippuri lutamāti].


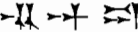
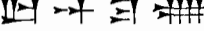



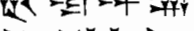
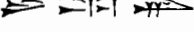
care they have been carved. What trouble—what enormous trouble—to take with a useless puzzle.\*

From the remarkable series of monuments obtained by M. de Sarzec from Tell-Loh, the following royal names (which I transcribe from archaic into late Babylonian), among others, have been obtained:—

Kings.

	<i>Igi-gina.†</i>
	<i>Ili-gala-gina (or Nini-gala-gina),</i> who seems not to have reigned, was father of
	<i>Ur-Nina.‡</i>
	<i>A-Kur-gala,§</i> son of Ur-Nina.
	<i>Uru-kagina.  </i>
	<i>Taltal-kur-gala.</i>

Patesis (viceroys).

	<i>En-te-na.</i>
	<i>En-ana-gin,</i> his son.
	<i>Ur-Bau.</i>
	<i>Gudea.¶</i>
	<i>Ur-E-girsu,**</i> his son.
	<i>Nammağani.††</i>
	<i>Gala-Lamma,</i> son of.
	<i>Lukāni.††</i>
	With a few others.

\* It has been left to the 19th century to invent a Volapük—that addition to the languages of the world which no really practical man can take seriously. There are languages enough to learn already without adding to their number, and even Volapük can hardly be easier for the foreigner to learn than English, a World-speech even now.

† "He who goes before."

‡ "Man of (the goddess) Nina."

§ "The son of Bel (?)."

|| Or *Uru-enima-gina*, "City of the faithful saying"

¶ "The proclaimer" or "prophet."

\*\* "Man of E-girsu," or "Ninip."

†† "His supremacy."

‡‡ Or *Lu-enimani*, "Man of his word."

Besides the royal houses and rulers of that period, however, there are many other things which are illustrated by these often long inscriptions; namely, the religion, civilization, art, occupations, language, manners, and customs of the people of that ancient time. We see, first, the beginnings of their art and writing, both of them rough and crude, the latter in sketchy and inartistic hieroglyphic form—the former stiff and laboured, showing observation and a certain skill, but also much clumsiness and want of finish.

Notwithstanding this, the progress in art and civilization made by the little under-kingdom of Lagaš must have been great. Hampered of yore by dearth of stone, it was the good fortune of King Gudea to have an opportunity of bringing large masses of diorite from Makan, a place which is now regarded as some part of the peninsula of Sinai. Here was a chance for the sculptors of Lagaš, and they used it to such good effect that the little capital must have been the envy of many another state in the Euphrates valley. To-day these works of Babylonian art are the boast of the Assyriologists of France, and they are most important.

We cannot say, unfortunately, that the style of art exhibited by these sculptures is by any means elegant—indeed, the lines are stiff, and the whole is rather clumsy. Probably the hardness of the stone and the solidity of the blocks had something to do with this, and their shape, when in the rough, may have influenced the carver. As a rule, the standing figures are rather squat, the seated ones sit very low (thus exhibiting, to a certain extent, the same defect), and the drapery sticks out stiffly. Nevertheless the appearance of the whole is not displeasing. It is very unlucky that these important statues are, without exception, headless; but, as a slight compensation, two heads have been found without the bodies to which they belong. (Plate II). These heads, as will easily be seen, are a redeeming point, and (supposing all the statues to have had the heads equally well formed and finished as these) make us excuse the shortcomings of the lower parts of the figures. It cannot be said with certainty whether the features are Semitic or not, that important member of the face, the nose, being wanting in both cases. It may probably safely be said, however, that, though the hatless example shows clear Semitic features, the covered one has at least *some* unsemitic indications, high cheekbones, unprominent lips, and a broad, firm, and square chin, a true contrast to the other, which may be regarded as a characterless face, its distinctive features being a round





AKKADIAN TYPE OF FEATURES.



HEAD OF STATUETTE  
SHOWING OBLIQUE  
EYES.

(*De S., Pl. 25.*)



FEMALE HEAD.

(*De S., Pl. 25.*)



SEMITIC TYPE OF FEATURES.

(*From De Sarzec, Découvertes, Pl. 12.*)

head, unprominent cheekbones, pouting lips, and a round chin. The head, with the thick-brimmed hat, seems to me to show distinctly the Akkadian type, whilst the other is distinctly Semitic. Nevertheless it may be regarded as certain that in both cases there is some admixture of foreign blood—Semitic Babylonian in the one case, and Akkadian in the other. In the case of a smaller head from a statuette of baked clay, in which the nose is of truly Roman or German-Jewish dimensions, the general type (which is rather ludicrous) may be regarded as the accidental production of a not over-skilled modeller, this feature not being so noticeable in the case of the small bronze statuettes of Gudea holding the cone or firestick. Again, it may be noted that all the kings and viceroys of this period have most pronounced non-Semitic names; indeed, we do not know how to render some of them into Semitic Babylonian at all, and it is therefore to be expected that we should come across ethnic types indicating difference of race such as is shown in the case of the head with the thick-brimmed hat.

It is also remarkable that these two heads are quite beardless, and agree, in this, with the royal figures on the cylinder-seals: yet in the East the beard is considered such a very important thing. The gods worshipped by these people, however, are invariably bearded, like the bronze statuettes of Gudea. Is it possible that the early non-Semitic inhabitants of Babylonia shaved their beards until they reached a certain age?—it would seem so.

Of course the more noble of these two types—the Akkadian—was destined to disappear in the course of centuries; nevertheless, it left its impress not only on the outward form of the Babylonian nation, and through that, on the Assyrian, but also on the temperament of the two nations. They both exhibit all the energy of a mixed race, the Babylonians in the arts of peace, the Assyrians in those of war—both excelling, though, also in branches which were not their respective specialities; for the Assyrian, though warlike in the extreme, was learned and artistic; and the Babylonian, though a trader, could also act the brave warrior and the learned man and author, and was not without a certain kindheartedness mingled with his shrewdness and closeness in money-matters, as we shall see farther on.

Let us turn, however, to the long and interesting inscriptions with which these statues are covered, for it is there that we shall probably find the best picture of the life of the people of Mesopotamia at that early period. The picture

which I shall try to give will be imperfect, but I shall do the best with the material at my command. When more is known of the Akkadian language, all Assyriologists will doubtless be able to do better; for, could we only translate these Akkadian inscriptions with even the same certainty that we can a great part of those in the Assyrian tongue, the story that we should have to tell would not only be free from lacunæ, but also more precise, and, being deprived of all element of doubt, more interesting.

Of all the kings of that ancient line, Gudea seems to have been the most renowned. Outside of his own capital, it is true, no mention of him has been found. As his realm was one of importance in Babylonia, however, it is to be supposed that it was not seldom mentioned in the records of the land, and the fact that there is no record to hand of Gudea and the renown of his kingdom, must be attributed to mere chance, such as often seems to rule in the domain of antiquarian research.

The inscription of which I now give a paraphrase or attempted translation, covers all the plainer parts of a very fine statue (headless, unfortunately) of Gudea (frontispiece), and is divided into nine columns, with a total of about 366 lines of writing. Portions have been translated by Professors Hommel and Oppert, and renderings of the whole have been given by the late M. Amiaud, the most promising Assyriologist of France, lately deceased. The present rendering differs in some particulars from those already given.

Gudea begins with a kind of superscription referring to the gifts made by him to the great temple of Ê-girsu (the god Ninip). It begins as follows:—

“(This), in the temple of Ê-girsu, his king, (is) the image of Gudea, viceroy of Lagaš, who built (the temple called) Ê-ninnū.”

Here follow the offerings made by him—fermented drink, food (of each 1 *ka*), and two other things (of each half a *ka*). The inscription then proceeds:—

“In the day of revocation, the word of Ê-girsu shall place the ban on the viceroy, who shall revoke them. May his gifts be revoked in the temple of Ê-girsu—may the word of his mouth be cut off!”\*

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\* In Akkadian: *Û gu-ba gallam patesi gu-nibgigia, me Ê-girsu-ka baniplâ. Saduga-na E-Ê-girsu-ka-ta gu-gîbgi, gu ka-ni yišir.* According to the syllabaries and bilingual lists, the following would be the Semitic rendering: *Ina ùm rugummê (or pukurrê) iššakku ša inaggag (or ippal),*

This is followed by the invocation of the god Ê-girsu, "the powerful warrior of Ellilla," or Bel, by "Gudea, the renowned (?) one, viceroy of Lagaš, proclaimed as the faithful-hearted shepherd of Ê-girsu, the favourably-regarded one of the goddess Nina, the power-endowed one of the god Nin-dara, the word-outpouring (= eloquent) man of the goddess Bau, the begotten son of the goddess Ga-tumu-dugu, endowed with the sovereignty and a supreme sceptre by the god Gal-alima, the living-hearted wide-renowned one of the god Dun-šagā, the bright-sceptred chief of Ê-giš-zida, his god."

In the difficult passage which follows I adopt, in part, Amiaud's rendering. The text seems to be to the effect that, "after the god Ê-girsu had looked upon his city favourably, and had chosen Gudea to be shepherd over the people, and among the divisions of men had established his power, he gave to the city a glorious name." A reference to building then occurs, and is followed by some lines which are translated by Amiaud to the effect that he (Gudea) had banished from the city the "adorers of demons (?),\* evokers of spirits (?), necromancers (?), and prophetesses of divine decrees." If this rendering be in any way correct, it may be concluded therefrom that Gudea was firmly set against the childish rites and ceremonies and the foolish superstitions of the Babylonians, so renowned for things of that kind. It is doubtful, however, whether any Babylonian king was ever enlightened enough to throw off the trammels of that superstition which was for ages so rife in the land. The two following lines are translated by Amiaud, "Whoever has not departed obediently has been expelled perforce by the warriors." †

"The temple of the god Ê-girsu," the inscription con-

*paraš Ê-girsu irakkissu (?)*. *Sattukki-su ina bêt Ê-girsu likiḫu, amat pī-šu liktā*. The above is probably a very barbarous rendering, the Assyrian equivalents being probably not the usual ones, but as they are given by the syllabaries, they may be regarded as at least approximately correct.

\* The original list points rather to "preparers of simples" (*lu tuḡa imḡala*, "who edible herb prepares"), the second character of the group being the character for mouth with the sign for vegetable within, in late Assyrian  $\rightarrow$   $\left[ \begin{array}{c} \text{𐎶} \\ \text{𐎶} \end{array} \right]$ .

† In Akkadian: *Ila-bi sale nu-ila, saga ur-saga emunadu*, a phrase that I am inclined to translate: "That ascent (*ila-bi*) had never yet been made (*nu-ila*), (and) the soldiery (*ur-saga*) were at the head" (of the procession). More information, however, is required from the syllabaries.

tinues, "he made like Eridu,\* a glorious place." Then, after two doubtful lines, mention is made, apparently, of certain officials, who, as M. Amiaud has it, "during the execution of this work have worn garments of . . . . (?)." The next few lines are doubtful, but these I am inclined to translate "(Whilst) the construction (?) was in hand,† the high place of the city was not occupied, a funeral-pile was not set,‡ the minister did not perform (?) a service (?), (or) utter lamentation, (and) the mother of lamentation did not utter her lamentation; within the boundaries of Lagaš no litigant has taken a man to the place of swearing the oath, (and) no pledger has taken the house of a man in pledge."

The latter part of this passage, with its mention of lamentation (there is no doubt as to the meaning here), gives the clue to the true rendering of the beginning. We see from this that the "high place" (𒀭 𒀭𒀭 *ki-maša* §) was the spot to which the dead were carried, apparently to be disposed of by fire. The word translated by Amiaud as "corpse + earth" || I have rendered, in conformity with this, "funereal pile," the characters 𒀭𒀭 𒀭𒀭 seeming to me to form a group by themselves, meaning, seemingly, a place where fire was lighted, for this same group, with the prefix for god (𒀭𒀭 𒀭𒀭 𒀭𒀭), forms, as is well known, one of the common ideograms for the moon-god Sin in his special character of "lightgiver" (*Nannaru* = *Nannaros*).

The inscription then continues:—

"For Ê-girsu, his king, whose glory shines forth, he has built the temple Ê-ninnû ("temple fifty") of the bright-shining Zû-bird,¶ (and) has restored its site. He has constructed within it his beloved holy place of cedar-wood."

\* In this case (as also in some others) I believe Eridu ("the good city," also called, as here, "the city of the prince," *Nun-ki*) to be the abode of the blessed in the world to come.

† Amiaud has, "During all the time of (its construction)," a rendering which may be regarded as very close. The text reads: *Nam-sig u-ba mugalam*, "Foundation (?) in hand being."

‡ Amiaud translates: "In the cemetery of the city no ditch has been excavated (?), no corpse has been interred," and this translation may be regarded as giving the sense very well.

§ More correctly "supreme place."

|| The line is 𒀭𒀭 𒀭𒀭 𒀭𒀭, which he seems to have analysed "corpse + earth + not + placed" = "a corpse has not been interred."

¶ 𒀭𒀭 𒀭𒀭 𒀭𒀭 𒀭𒀭 𒀭𒀭. The paragraph here translated occurs on a large number of monuments, mostly small inscriptions. The reading of the third character as 𒀭𒀭 instead of 𒀭𒀭 is based on one

And here comes the more interesting part of this important inscription :—

“When Gudea was building the temple of the god Ê-girsu, Ê-girsu, his beloved lord, delivered all things unto him\* from the upper sea to the lower sea. In Amâlu<sup>m</sup> (= Amânu<sup>m</sup> or Amanus in northern Syria), the mountain of cedar, he has cut and caused to be brought from the mountain cedar [trees] whose [length] was 70 (?) cubits; † cedar [trees] whose [length] was 50 cubits, box(?)-[trees] ‡ whose length was 25 cubits.” With this wood he made various parts§ of the temple Ê-ninnû as well as a fane called Ê-mag-ki-a-sig-dê-da-na (𒂍 𒀭 𒄀 𒀭 𒂗 𒀭 𒀭 𒀭 𒀭 𒀭). He also had trees cut down, near the city of Ursu, in the mountains of Ibla (𒂍 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭). *uru Ursu D.S. ġursag Ibla-ta*). These trees are called *Zabalu<sup>m</sup>* (𒂍 𒀭 𒀭 𒀭 𒀭), *Šaku* (𒂍 𒀭 𒀭 𒀭) = Assyrian *Ašubu*, *Tulubu<sup>m</sup>* (𒂍 𒀭 𒀭 𒀭 𒀭 𒀭), and *Kur* (𒂍 𒀭 𒀭), lit., “wood of the mountain”, and were used as beams in Ê-ninnû. Stone, called *nagal* (𒂍 𒀭 𒀭 𒀭), was brought from 𒂍 𒀭 𒀭 𒀭 (U<sup>m</sup>ali<sup>m</sup>), also readable *Umanu<sup>m</sup>*, *Šamalu<sup>m</sup>*, and *Šamanu<sup>m</sup>*, in the mountains of Menua (𒂍 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 *ġursag Menua*), and from 𒂍 𒀭 𒀭 𒀭 𒀭 (Musalla or Supsalla), in the mountains of Phoenicia (𒂍 𒀭 𒀭 𒀭 *Martu*). This stone was made into slabs (𒂍 𒀭 𒀭 𒀭) for the construction of the temple of Ê-ninnû.

Other stone, called *širgal ġabbia* (𒂍 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭), a kind of limestone, was brought from Tidalu<sup>m</sup> or Tidanu<sup>m</sup>

of these. Cf. Brûnnow, “Classified List,” Nos. 8478 and 8479. For the legend of the Zû-bird, see Sayce’s “Religion of the Ancient Babylonians” (Hibbert Lectures for 1887), p. 293 ff.

\* 𒂍 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭, *Kiš-bi gal-munakad*.

† The word which I here translate as “cubit” was, in reality, a measure of about twelve inches.

‡ Better, perhaps, “cedar-beams,” and “box-beams,” notwithstanding that the words used are probably to be completed 𒂍 𒀭 𒀭 𒀭 and 𒂍 𒀭 𒀭 (cedar tree(s) and box tree(s)) respectively. In Old English the word *beam* meant “tree”—compare the compound *wudu-beamas*, “trees of the wood.”

§ The words employed, and which cannot at present be rendered with any certainty, are as follows : 𒂍 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭, 𒂍 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭, and 𒂍 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭. 𒂍 and 𒂍 𒀭 𒀭 𒀭 are probably simply terminations.

(𐎠𐎫 𐎶𐎶 𐎠𐎫), "a mountain of Phœnicia," identified by Prof. Hommel with the Tidnu of the Semitic Babylonian and Assyrian inscriptions, which would correspond to the mountainous portion of Syria and Canaan.\* This stone was used for the *urpadda* (𐎶𐎫 𐎶𐎶 𐎶𐎶), probably gate-posts, provided with slots to receive the bars of the gates. From a place called *Kâ-gala-ada* (𐎠𐎫 𐎠𐎫 𐎠𐎫 𐎠𐎫)—apparently the Assyrian 𐎠𐎫 𐎠𐎫 𐎶𐎶 𐎶𐎶 𐎶𐎶 *Abulli-âbi-šu*,† which is explained‡ as equivalent to 𐎠𐎫 𐎠𐎫 𐎶𐎶 𐎶𐎶 𐎶𐎶 *Bâbu hurruš*—in the mountains of Ki-maš or Kibar (𐎠𐎫 𐎠𐎫), copper (𐎠𐎫 𐎠𐎫) was brought to make a certain part of the structure or its decorations.¶ From Melugga (𐎠𐎫 𐎠𐎫 𐎶𐎶 𐎶𐎶), identified by Lenormant as part of the peninsula of Sinai, *êšu*-wood was brought for the edifice, and the same kind of wood seems to have been gotten from Kirzan (𐎠𐎫 𐎶𐎶 𐎶𐎶 *Kirzanu*<sup>m</sup>) also. From the mountains named Ġaġu<sup>m</sup> (𐎶𐎶 𐎶𐎶 𐎶𐎶) and from the mountains of Melugga (Sinai) gold-dust (𐎶𐎶 𐎶𐎶 𐎶𐎶 𐎶𐎶 *guškin saġaraba*) seems to have been obtained, not for Ê-ninnû, but for a temple called Ê-martu. Gudea imported also a material called *lidri* (𐎶𐎶 𐎶𐎶), and *ġalup*-wood (𐎶𐎶 𐎶𐎶 𐎶𐎶 𐎶𐎶) from *Gubin* (𐎠𐎫 𐎠𐎫 𐎶𐎶 𐎶𐎶), "the land of *Ġalup*-trees" (𐎠𐎫 𐎶𐎶 𐎶𐎶 𐎶𐎶 𐎶𐎶), apparently to make pillars to support the roof of the temple. A material¶ suggested by Amiaud to be bitumen was obtained from Madga (𐎶𐎶 𐎶𐎶 𐎶𐎶 𐎶𐎶), which is described as a mountainous country by the river Luruda (𐎠𐎫 𐎠𐎫 𐎶𐎶 𐎶𐎶 𐎶𐎶 𐎶𐎶 𐎶𐎶 *ġursag Luruda-ta*, lit., "a mountain in the river Luruda"). From a place called

\* In the lists (𐎠𐎫 𐎠𐎫), printed for the first time correctly by Brünnow. The Semitic rendering is *aharrû*.

† "His father's great gate"—a parallel to the modern "sublime Porte."

‡ W.A.I. II, 52, 55.

§ "The hollow gate" *i.e.* "passage,"—"hohle Gasse."

¶ M. Amiaud connects the name of the place called *Ki-maš* with the word read as *kēmašû*, translated as "copper." This I doubt. The phrase in question in which it occurs is *kima kē-mašši limmašši*, which I translate "may he (the sick man) be pure like bronze." The Akkadian equivalent of *kē-mašši* in this passage is 𐎠𐎫 𐎠𐎫 𐎠𐎫 (*sipar*). *Kē-mašši* may be connected with *kimaš*, but it is worthy of note that one of the words for bronze was 𐎠𐎫 𐎠𐎫, *kū*, which may have been regarded as the nominative case of *kē*, in which case *kē-mašši* would be a compound.

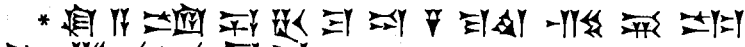
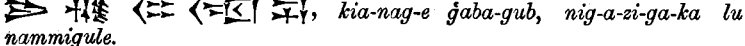
¶ 𐎠𐎫 𐎠𐎫 𐎠𐎫 (= 𐎠𐎫 𐎠𐎫?).

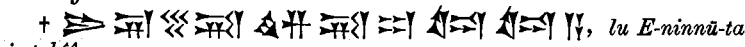


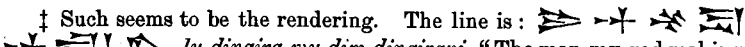
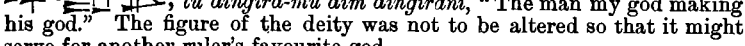



“To his statue I proclaimed aloud: ‘This statue neither of precious metal, nor of lapis bright (?), nor of copper, nor of lead, nor of bronze has anyone made, (but) of *Ēšu*-stone (diorite). Let it remain in this place of libation, and let no man destroy anything that has been brought here.\* The statue before thee, O *Ē-girsu*, (is) the statue of *Gudea*, viceroy of *Lagaš*, the man who built *Ē-ninnū* for *Ē-girsu*. Whoever removes it from *Ē-ninnū*,† and destroys the inscription; whoever shall set aside (?) my fair record (?), whoever shall make my god his god,‡ O *Ē-girsu*, my king, put his people to flight, take away (his) judges, reject (his) gifts;§ make confirmation (?) of the festival instituted by me|| and of my name, removing his name.¶ (In) the sanctuary (?) of *Ē-girsu* every (?) king who does (?) wrong (?) shall not be in his presence. From this day, (O thou) of the glorious seed, *patesi* of *Lagaš*, restore\*\* *Ē-ninnū* of *Ē-girsu*, my king. (As for) the man whose glory shines forth (= *Gudea*), no one shall change his words or transgress his judgment.’”

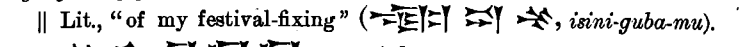
Here follows an imprecation, in which the gods *Anu*, *Bel*, *Nin-gursaga*, *Ēa*, *Sin*, “whose name none repeats,”†† *Ē-girsu*, “lord of the weapon,”‡‡ *Nina*, “lady of interpretation,” *Nin-dara*, “the warrior-king,” “the mother of the city of *Lagaš*,”§§

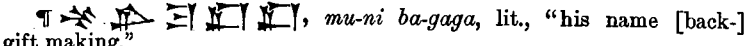
\*   
 *kia-nag-e ġaba-gub, nig-a-zi-ga-ka lu nammigule.*

+   
*lu Ē-ninnū-ta imtabbēa.*

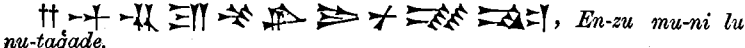
‡ Such seems to be the rendering. The line is:   
 *lu dingira-mu dim dingirani*, “The man my god making his god.” The figure of the deity was not to be altered so that it might serve for another ruler’s favourite god.

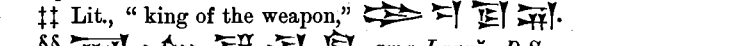
§ Lit., “Rejecting his gifts” ( *nigbaga bā-gigida*).

|| Lit., “of my festival-fixing” ( *isini-guba-mu*).

¶  *mu-ni ba-gaga*, lit., “his name [back-] gift making.”

\*\* Lit., “make.”

††  *En-zu mu-ni lu nu-taġade.*

‡‡ Lit., “king of the weapon,” .

§§  *ama Lagaš, D.S.*

the lady\* Ga-tumuduge (Gatumudue), Bau, "sister of the eldest son of Anu," Innanna (= Ištar), "Lady of battle," Utuki (= Šamaš, the Sungod), "king of the pouring-out of oil," † Sig-saga, ‡ "the ruler of the people of the gods Gal-alima and Dun-šagana," Nin-marki, "eldest child of Nina," Dumuzi-abzu (Tammuz of the Abyss), "lady of the city Kinunir," and Ê-giš-zida are called upon to change the destiny of any man who shall change the words of Gudea, *patesi* of Lagaš, felling him like an ox, and quelling him like a wild bull (*rîmu* in Assyrian) in the fulness of his strength. The inscription then concludes: "May the weapons of my steadfast people throw him down in the dust, may the diminution of his renowned (?) name come to be heard of; may they erase his name from the tablet in the house of his god; may his god not look favourably on his land; may he destroy it with rain from heaven; may he destroy it with the water of the earth (inundations); may he go forth nameless, and may his reign be made (one of) subjection. May that man, like a man doing evil to his chief, § find a habitation || afar under the vault of heaven. May the people proclaim the greatness of the champion (?) of the gods, the lord Ê-girsu."

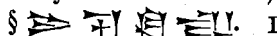
Such is, as nearly as can now be made out, the tenor of the principal inscription, that of "the architect with the plan," ¶ and its importance can hardly be overlooked. It is of value not only for the history and geography of an important part of the world at an extremely early period (at least 2500 years before Christ), but also for an insight into the manners and customs of the time.


The passage which refers to the burning of the dead has already been mentioned, and whilst it must be confessed that the rendering is somewhat doubtful, yet it may be taken as very probable. Excavations made by the German expedition to Al-hibba in southern Babylonia show that they un-

\* Or: "Glorious one."

†  *lugal ni sega-gi.*

‡ The same as the god Išum, the "glorious sacrificer" (a rendering to which, notwithstanding prior publication by another, I lay claim of first discovery and communication).

§  D.P. *si-sa-ra*, lit., "to his director."

||  *šu-na nibari*, lit., "set his hand."

¶ So called from the incised plan which he holds on his knees.

doubtedly buried their dead, but the Babylonian and Assyrian inscriptions seem to show that they also burned them. Many of the ancient kings of Babylonia seem to have been burned when dead.\* The Akkadian words for funeral-pile seem to have been *kuku*, *giškuru*, *kibir* and *giškibir*, and the Semitic Babylonian words *ēšše*, *makaddu*, *kaddu*, *kūru*, and *kibirru* (the last two borrowed from Akkadian). Time alone will show how far cremation was practised with the Akkadian and Semitic inhabitants of ancient Babylonia. Our text testifies to the fact that the Eastern custom of employing professional mourners was in vogue among the Akkadians, and this may also be gathered from the legend of the descent of the goddess Istar into Hades, where male and female mourners for Tammuz her husband are referred to.

Whether the Akkadians were a law-abiding people or not there is but little to show, but it may safely be said, that they were a law-loving people. The paragraph where reference is made to litigation shows what their character was in that respect, and this love for legal forms probably lasted to the end. We know, from the many law-tablets of the later Babylonians, how great their love for legal formalities was, and we may suppose that this was inherited from their Akkadian forefathers.

Like the whole Babylonian race, the Akkadians were, in their way, very religious, and superstitious withal. To this the whole inscription testifies. The part which attracts our attention, however, the most, is probably that where Gudea gives command to his statue to invoke "the statue of his king." If this translation be the correct one, he practically calls on his own statue to represent him in the temple, and probably intends thereby, that it should intercede for him with the god whom he worshipped, when he should be absent from the fane—indeed, he may have intended it to represent him in this way when he should have departed this life.

The power of the daughter to represent a house in which there was no son, testifies to the honour paid by the Akkadians to women in a part of the world where she was, and still is, regarded, more or less, as a chattel. This Akkadian custom seems to have had its influence even to the latest times of the Babylonian empire, as we see from the part which

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\* See Geo. Smith's article in the third volume of the *Transactions of the Society of Biblical Archaeology*, pp. 374-376 (ll. 27, [32], and 37).

Babylonian women took in bearing their share of the burthens of life, as shown by the late Babylonian contract-tablets. The so-called "tablet of Akkadian laws" and other texts also testify to this being the case. Upon this question, however, I shall speak elsewhere.\*

Of course, the Akkadians were slave-holders, but they seem to have been of a kindly disposition, and to have treated their slaves well. In this case seven days' holiday are said to have been given them, and this is the only record known of such a thing. In later times masters showed their appreciation of the service rendered to them by their slaves by conferring on them certain privileges, and it is probable that, at least in the majority of cases, the lot of a slave was not one of hardship.

The question of religion touched upon by this text of Gudea would carry me much farther than I intend to go at present, but there is one important fact, in view of the anti-Akkadian theory, to be noted, and that is, that the names of all the gods mentioned in this text are non-Semitic. Some of their more noteworthy titles I have mentioned, the most striking being that referring to the moon-god Sin, of whom it is said that no man repeats his name (𒀭𒀭𒀭𒀭 𒀭𒀭𒀭𒀭 𒀭𒀭𒀭𒀭 𒀭𒀭𒀭𒀭 *mu-ni lu nu-tagade*, "name + his + man + not + repeating"). The pronunciation of Sin for 𒀭𒀭𒀭𒀭 (the moon-god) is given by a Babylonian syllabary, but we have no indication as to how the characters 𒀭𒀭𒀭𒀭 (the form of his name used in the text now under examination) were pronounced. Their usual value is 𒀭𒀭 *En-zu*, and they probably mean "lord of knowledge," but whether this is the true pronunciation or not is uncertain. It has been thought that they should be pronounced backwards, thus: *Zu-en*, later corrupted to *Sin*, but of this there is no confirmation except that there is sometimes found a phonetic complement *na*, showing that the word, whatever it was, had *n* as its final consonant. It is also worthy of note that the goddess *Bau*, besides being the goddess of healing, seems also to have been goddess of eloquence. This is implied by a passage in this text, in which Gudea is described as "Bau's eloquent one" † (𒀭𒀭𒀭𒀭 𒀭𒀭𒀭𒀭 𒀭𒀭𒀭𒀭 𒀭𒀭𒀭𒀭 𒀭𒀭𒀭𒀭 𒀭𒀭𒀭𒀭 *lu enima sega D.P. Bau-gi*), though this may simply mean that Gudea was eloquent in singing her praises. From the

\* It is to be noted that it was only in Akkadian times that queens really reigned.

† See p. 131.

goddess Nina, "lady of interpretation" (𒀭𒀫 𒀭𒀫𒀭𒀫 𒀭𒀫𒀭𒀫 𒀭𒀫𒀭𒀫 𒀭𒀫𒀭𒀫 *Nina, nin induba-gi* = Assyrian *Nina, bēlit pirišti*), a portion of the city of Lagas, as well as the world-renowned city of Nineveh, seem to have taken their names. What connection, however, the Assyrian Nineveh may have had with that of Babylonia, is unknown.

Court-life in Babylonia at this early period was probably of a very simple kind. The *patesi* or viceroy seems to have been nothing more than a chief among his people, and was most likely also chief priest, as were likewise the Babylonian, Assyrian, and Egyptian rulers in general. We know from the large number of letters which passed between the Assyrian kings and their subjects, what the relations between ruler and ruled were, and it is probable that, if we could only light upon the Babylonian royal record-office, we should find that nearly, if not quite, the same interest was taken by the king in his subjects in Babylonia as in Assyria, in early as in late times—though, as it is probable that fewer persons, in the earlier ages, knew how to write, fewer records referring to this relationship would be found. History indicates, too, that the Babylonian rulers always strove to make themselves popular, and, aside from the petty jealousies which were sometimes rife in the land, seem to have succeeded very well.

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## II.

### *From private documents of about 2300 B.C.*

We have obtained a few glimpses of life in Babylonia at the very early period when Gudea was ruler, from one of his royal inscriptions. Let us now briefly glance at it from the people's point of view.

In studying the tablets of the early Babylonian period, mostly contracts, we are at once struck by a fact which has already been noticed several times, namely, that whilst most of the names are Semitic, yet the documents themselves are, with few exceptions, in the Accadian language. The reason of this is obvious when we examine the texts in question, for it is only the documents whose contents are unusual that are in Semitic Babylonian—almost all the others relate to sales of land and similar things in which a set form of words is used, and the time-honoured expressions employed by the scribes were, as is usual in all cases of the kind, long in dying

out. The following will serve as an example of the style of the wholly Akkadian documents: \*

"One acre of field-land beside the plantation of Ibnî-Sin the gardener and beside the field of Ura-Utu (the chief), (its) end the field-land of the sons of Sin-azu, and its end the field-land of Utuki-šemi, the inheritance of Utuki-idinnam son of Nannar-me-giš. With Utuki-idinnam son of Nannar-me-giš, Šili-Innanna son of Ili-lağ and Apil-ili his brother have priced it,  $1\frac{2}{3}$  shekels of silver they have weighed as its complete price. For future days, for time to come, they shall not dispute, they shall not withdraw. They have invoked the spirit of the king.

"Before Nabi-Bêl (son of Nidittu<sup>m</sup>);

"Before Kîšti-Ura the scribe (?);

"Before Sin-yatu<sup>m</sup> son of Pirḫu<sup>m</sup>;

"Before Ili-ikîša<sup>m</sup> son of Narâm-Addi;

"Before Aplu<sup>m</sup> son of Ša-ili;

"Before Nannar-igi-guba, the *nâru*.

"The tablet of the contracting-parties is ended.

"Year of Tašmêtu<sup>m</sup>."

I transcribe the text here:—

(1) *Aš gan gana-ki* (2) *da (giš)-šar Ibnî-Sin, nu-(giš)-šar*  
 (3) *u da ašag Uru-Utu,* (4) *saga gana-ki du-meš* →† *Sin-azu*  
 (5) *u saga-bi gana-ki* →† *Utuki-šemi,* (6) *ğala* →† *Utuki-idinnam*  
*du* →† *Nannar-me-giš.* (7) *Ki Utuki-idinnam du* →† *Nannar-*  
*me-giš,* (8) *Šili-Innanna du Ili-lağ* (9) *u Apil-ili, šesa-ni*  
 (10) *inšišamameš;* (11) *gi-šanabi gin azaga* (12) *šama-tilani-šu*  
*innánlal* (13) *U-kur-šu, u-nu-mea-kam* (14) *nu-mundapale*  
 (15) *nu-mungigine,* (16) *Mu lugala-li inpada.* (17) *Igi Nabi-*  
 →† *Ellilla, etc., etc.* (25) *Mu* →† *Tašmetu<sup>m</sup>.*†

Anyone with a slight knowledge of Semitic languages will see that the character of the above transcription is not by any means Semitic. In the names, however, he will find Semitic forms, as well as Akkadian, but the former predominate. The names with Semitic elements are Ibnî-Sin, "Sin has made," Utuki-šemi, "Sungod, hear!" Utuki-idinnam, "the Sungod has given," Apil-ili, "Son of God," Nabi-Bêl, "Prophet of Bêl," etc., etc. Out of seventeen names eleven have Semitic elements in them, and it is possible that some of those that I have transcribed as Akkadian

\* Strassmaier's *Texte Altbabylonische Verträge aus Warka*, No. 60. According to the labels, these tablets came from Tell-Sifr.

† The cuneiform text of this tablet with the variants from the version on the envelope, will be given at the end.

were pronounced and read as Semitic Babylonian. Tablets of this class belong, it seems to me, to a period when the Semitic members of the population were beginning to outnumber their Akkadian compatriots.

But this is shown still more distinctly in those tablets which, as already mentioned, refer to the more unusual class of transactions. In the tablet of the brotherhood, translated by me in 1885, besides the few Akkadian names, only 6 short lines out of 36 are Akkadian;\* and in the tablet of the "Rival Claimants" only 3 lines out of 45 are Akkadian. These Akkadian lines are the paragraphs invoking, in the same set terms, the gods and the king; and the date.

In these longer texts the love of legal forms again appears. On the tablet of "the brotherhood" the man mentioned in the contract above translated, Šili-Innanna, and Iriba<sup>m</sup>-Sin, "make brotherhood" (*tapputa<sup>m</sup> ipušū*) and meet to ratify the compact. They "took a judge" (*da'ani ikšudū*), who led them to the temple of the Sungod (*ana bêt Šamaš itrudu†-šunuti*) and caused them to take judgment (*dina<sup>m</sup> ušāhizū-šunuti*) there,‡ and the people answered and confirmed their brotherhood (*āhiati-šunu ūppilū*). The contracting parties had to make offerings (of slaves) to the temple, and then comes some good advice, "Brother shall not be angry with, shall not injure, brother." The priest then proclaimed in the temple of the Sungod, "Brother shall be kind to, shall not injure, brother; and brother shall not make claim against brother." They then invoked the spirits of certain gods and of Hammurabi the king. The list of witnesses and the date follow.

The tablet of the rival claimants is of the same simple nature. It begins, "Concerning the plantation of Sin-magir, which Na'id-Martu bought for silver" (*Aššum kirī ša Sin-magir, ša Na'id-Martu ana kaspi išāmu-šu*). Ilu-banī applied for a royal decree, and went to the judges (*Ilu-banī ana šimdat šarri ibkurur-ma ana da'anē illiku*). The judges took them § (owner and claimant) to the gate of Nin-Marki ||

\* On the envelope, 7 out of 41.

† The Arabic equivalent, طرد, means "to drive away."

‡ In view of this phrase, it has since occurred to me that perhaps *tapputu* means "reconciliation." The bilingual texts, however, are against this. See the introduction to my "Early Babylonian Deed of Brotherhood," *P.S.B.A.* for November, 1885.

§ Here again *itrudu-šunuti*.

|| Name of a deity, "Lady of the west."

and the judges of Nin-Marki. Ilu-banî declared thus in the gate of Nin-Marki: "Indeed the son of Sin-magir am-I, he adopted me as his son (*Lû mâr Sin-magir anaku, ana mârûti lu-ilkia-anni\**). . . . He said thus: "After Rim-Sin (apparently the king of that name) the plantation and house shall descend to Ilu-banî." Sin-mubaliṭ kept back the plantation of Ilu-banî, applied and went to the judges, and the judges took them (Sin-mubaliṭ and Ilu-banî) to the assembled people and the elders, and at another gate of the city the question was again discussed. Ilu-banî repeated his statement before the elders: "Indeed the son am I." They said: "The plantation and house belongs to Ilu-banî. Sin-mubaliṭ shall not withhold and shall not make a claim." The transaction concludes with the words: "They have invoked the spirit of Nannaros, Samaš, Marduk, and Ḥammurabi the king."

Here follow the names of the witnesses, and the words, "the seal of the contracting parties (has been impressed)." On the edge is the date, "Month Tammuz, day 4th, year when Ḥammurabi the king made prayer to Tašmêtu<sup>m</sup>." †

A great many other examples of tablets of this class might be quoted, and from each of them arguments in favour of the Akkadian theory might be drawn, and the picture of ancient Babylonian life might at the same time be continued. As, however, they are all very difficult, I leave them for the present, and conclude this section with a translation of a text of even greater interest, namely, the marriage ceremony.

The text in question is one of great importance. It is written in the two languages, Akkadian and Semitic Babylonian, and this gives additional interest to the contents, besides furnishing us with material of value for philologists. The tablet seems, at first sight, to be one of those containing pattern phrases to be learned by Babylonian students preparing for the position of priest or scribe, the phrases being of a very miscellaneous nature, though they all seem to be classified. The text probably belongs, however, to a certain series of incantations, of which fragments have been found on the site of Nineveh, and to which the attention of scholars has already been directed.

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\* Lit., "To sonship he took me."

† This is the same year as the Contract of Brotherhood was drawn up in.



The interesting part is in Column II, which I reproduce here in transcription (Semitic Babylonian or Assyrian only). It refers to the wedding ceremony, and the bridegroom's party is apparently on its way to the place where the wedding is to be :—

*La bēl ʔlani*

*imtaḥḥarū*

*Ḳati-šu[nu]*

*ana kati-šu [iškunu].*

*Šēpi-[šunu]*

*ana šēpi-šu [iškunu].*

*Ki[šad-sa]*

*itti kišadi-šu [taškun?]*

*Raman-šu*

*uštēbillu*

*Māru rubē anaku, iḳbiš*

*Kaspu, ḥurašu sun-ka umallu*

*Atta lū-aššatu*

*anaku lū-mut-ka*

*iḳbī-ši*

*Ḳīma ʔnib kiri*

*ana šāši lalē*

*ulallī-ši*

*Amelūti [ramani-šu]*

The impious

are approaching.

Their hands

to his hands they place.

Their feet

to his feet they place.

[Her] neck

with his neck [she has placed].

Himself

he has caused to be brought,

“The son of a prince am I,”  
he has said to her,

“Silver and gold shall fill thy  
lap.”

“Thou shalt be (my) wife

I will be thy husband”

he has said to her.

“Like the fruit of a plantation  
to this (woman) abundance

I will fill for her.”

His own people

Here the tablet unfortunately breaks off, but he who wishes to catch yet another glimpse, may consult a text of a more ritualistic nature, published by me in the *Babylonian and Oriental Record* for August, 1887, where the words of the priest, as well as directions as to the offerings to be made, are given.

As a testimony to the extreme antiquity of the above-quoted form of ritual, it is to be noted, that in the story of Gilgamesh,\* where the goddess Ištar makes a proposal to the Babylonian hero, she uses practically the same form of words as is given above, changing only the pronouns. The text is as follows :—

“To the beauty of Gilgamesh the lady Ištar lift up her eyes. Come, Gilgamesh, mayest thou be my husband.

\* Hitherto known as the “Gistubar-legends.”

Thy fruit to me give as a gift; thou shalt be my husband and I will be thy wife. Mayest thou be caused to have a chariot of *lapis lazuli* and gold, the body of which shall be gold and diamond its pole," &c., &c.'"

The words "Give to me thy fruit as a gift" (*Inbi-ka yáši kášu kišamma*), apparently have reference to the words of the extract from the ritual quoted above: "He shall fill abundance to this woman like the fruit (*inib*, construct case of *inbu*, oblique case *inbi*) of a plantation." The phrase is a curious one, however, and probably had some special meaning, now lost.

From two tablets which, by a strange chance, I had an opportunity of copying some months ago, we find that the wedding contract was made in duplicate, differing slightly in form, though the same phrases, with the essential words ("husband" and "wife") transposed, were used. Each "contracting party" brought special witnesses. It is worthy of note, that the woman, as well as the man, might pronounce the words of divorce ("thou art not my husband"), but whereas he was only fined, the woman was regarded as worthy of (practically) excommunication. Infidelity was punished with death.

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### III.

#### THE CHARACTER OF THE BABYLONIANS.

*From documents of the time of the later Babylonian Empire.*

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There is a question which doubtless occurs to many of us, and that is: "I wonder what sort of people the Babylonians were to deal with?"

It is difficult to say whether we ought to make any distinction between them and the Assyrians. There was probably but slight difference between them. The Babylonian seems to have been less warlike than the Assyrian, that is all. The Babylonian was warlike too when the occasion demanded it.

The Babylonian was a keen trader, careful in money-matters, ever ready to drive a hard bargain. He lent money out at an interest of about 20 per cent. per annum, and took substantial security, as a rule, for the same. In later, as in

earlier times, too, he dearly loved a lawsuit. Unlike the nations of modern times, he seems easily to have adapted himself to foreign rule; whether his kinsman the Assyrian did so or not we do not know.

Three excellent points, however, did the Babylonian possess:—

Painstaking in study, he easily became a learned man in his own particular way; but better than this, he was kind-hearted; respectful and considerate to his parents; and steadfast in friendship.

One of the most interesting texts bearing upon this is now in New York (it belongs to the Wolfe collection, which was obtained by Dr. Hayes Ward in Babylonia). It is a will, in which a man, named Nabû-šum-iddina, whilst leaving certain slaves and the produce of certain lands to his wife, Tabluṭu, takes care also to make provision for his mother. Day by day, and year by year, as long as she lived, she was to receive a certain quantity of grain, fruit, &c.; as well as meat and poultry. The sustenance of the parents, indeed, seems to have been regarded as an obligation, as witness the following letter:—\*

“[Letter of] Iddina-âhâ [to] Rêmut his son. May [Bel] and Nebo bespeak peace and life for my son. He, my son, knows that there is no corn in the house. Let my son cause 2 or 3 *gur* of corn to be brought by the hands of someone whom thou knowest. Wilt thou not send by the hands of the boatman whom thou indicatedst? As for him, [he is coming?] unto me—send a gift, cause it to go forth to (thy) father. To-day I pray Bel and Nebo for the preservation of the life of my son. Rêmat asks after the peace of Rêmut, her son.”

There is something plaintive about this gentle but urgent appeal. And then the ending, in which the father mentions Rêmat, the mother after whom Rêmut has apparently been named, adds, by the suggestion of her needs, to the gentle urgency of it.

Steadfastness in friendship, how often do we see it now? The tendency of the world is to believe ill of others—to listen to slanders of the most spiteful kind, and to act accordingly. A slight fault, or even no fault at all, but merely a supposed one, is magnified, and repeated to the disadvantage of another. He who is going down-hill is sped on his way, and the sooner he arrives at the bottom

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\* This text is in private hands.

the better,—at least, such seems to be the policy now-a-days, and slanderous tongues wag to good (or bad) effect.

Steadfastness in friendship is always a rare thing—probably the Babylonians did not possess it in any special measure, but what they were capable of the following letter shows clearly:—

“Letter from Nabû-zēr-ibnî to Ugaraa, Balatu, Nabû-bêl-šumati, and Šamaš-udammik, his brothers. I now pray Nebo and Nanâ to save the life of my brothers. Bêl-êpuš, who is along with you, is my brother. Whoever will speak his slander (lit., evil words, *dibbi b'išati*), as my brothers wish to do, let him be silent. As for him (*i.e.*, Bêl-êpuš), from the beginning to the end brothers of each other are we (*ultu rêš âdi kit âhê âhaweš nîni*). As warning to my brothers I send this. May my brothers do what is right. Let me see an answer to (this) letter from my brothers.”

Of course the words “brother” and “brothers” here mean “friend” and “friends” respectively. It is on the whole a remarkable letter. For one man to write to four others in this strain, telling them clearly that they were slanderers, is a thing which but few would be bold enough to do. But Nabû-zēr-ibnî did it, and fate—or providence—has preserved his letter as a lesson to the people of our own day, after 2500 years.

The next stage, that of charity to people in distress, was not unknown among the Babylonians. The lending of a fairly large sum of money, without interest, for an indefinite period, during a time of famine, is not what every businessman would do; but Rêmut, in the year 648 B.C., when the armies of Aššur-banî-apli had devastated the land, did so, and the following record of the event has come down to us:—

“Five-sixths of a mana (50 shekels) of silver from Rêmut, son of . . . , unto Mušêzib-Marduk, and Kullâ, his wife, for necessities. In the day when the face of the land sprouts (again) (*ina ûme pan mâti ittaptû*) the money, five-sixths of a mana, in its full amount, Mušêzib-Marduk and Kullâ will repay to Rêmut.”

Here follow the names of five witnesses and the scribe, with the date, “Babylon, month Tebet, day 9th, year 19th, Šamaš-šum-ukîn, king of Babylon.”

Then comes the paragraph:—

“At this time, in the city of Lamima (?), want and famine (are) in the land, the people are dying for want of food.”

Here is a man who, at a time when everything was in confusion, lends money, without interest, to two other people, only stipulating that it should be repaid "when the land sprouts again." This may not have taken place—that is to say, in a profitable way for the people to whom the silver was lent—for a long time, and the lender stood the chance of losing his money altogether if the borrowers should die in the meanwhile. A man who lends money at interest is always obliged to take the risk, when not covered as he usually is, by some substantial security; but Rêmut, in this document, evidently takes the risk out of pure kindness. Naturally inscriptions of this kind are rare, but this one shows that fellow-feeling was not by any means absent from the hearts of the Semites of the Euphrates valley.

In the present paper I have tried to reproduce some of the more noteworthy traits of the private life of one of the most interesting nations of antiquity. I am aware that my attempt is not by any means as it should be—it is simply a series of rough sketches hastily strung together. Such as they are, I trust that they may be found not altogether valueless. To add to and perfect them will be one of the ends which, in my studies, I shall keep in view. In the printing of the present paper, I hope to add to its permanent value by giving the cuneiform text of most of the inscriptions here published for the first time, and this, with the notes I shall give, will help to add interest to, and to round off, some of the pictures of Babylonian life here presented. My apology for such an imperfect paper must be, that the subject is a difficult one, especially from the all-important point of view of philology. This, however, is a part of the study which is better understood every day, and which, in the end, will bring us to that certainty in the matter of translation which is absolutely needful not only to this, but to every other branch of the science of Assyriology.

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24. 𐎶𐎵  
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## TRANSCRIPTION.

- Aš gan gana-ki  
 da (giš)-šar Ib-ni-Sin, nu(-giš)-šar  
 3. u da ašag Ura-Utu ¶  
 saga \*\* gana-ki †† du-meš Sin-a-zu  
 u saga-bi gana-ki Utuki-še-mi  
 6. ga-la Utuki-i-din-nam du Nannar-me-giš  
 ki Utuki-i-din-nam du Nannar-me-giš  
 Ši-li-Innanna du I-li-laḡ  
 9. u A-pi-il-i-li sesa-ni  
 in-ši-šam-meš  
 gi-šanabi gin azaga  
 12. šama-til-la-ni-šu in-na-an-lal ††  
 Ū-kur-šu, u-nu-me-a-kam §§  
 nu-mu-un-da-pal-e  
 15. nu-mu-un-gi-gi-ne,  
 mu lugala-bi in-pada  
 Igi Na-bi-El-lil-la |||  
 18. Igi ki-iš-ti-Ur-ra ra-bi ¶¶

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\* The envelope gives 𐎶𐎵 instead of 𐎶𐎵 𐎶𐎵.

† The envelope has 𐎶𐎵 𐎶𐎵 𐎶𐎵 𐎶𐎵 𐎶𐎵 𐎶𐎵 𐎶𐎵 𐎶𐎵.

‡ These two last characters of the line are written below.

§ The envelope here inserts 𐎶𐎵. || The envelope has 𐎶𐎵 𐎶𐎵 𐎶𐎵

𐎶𐎵 𐎶𐎵 𐎶𐎵 𐎶𐎵 𐎶𐎵 𐎶𐎵 𐎶𐎵 𐎶𐎵 𐎶𐎵 𐎶𐎵 𐎶𐎵 𐎶𐎵 𐎶𐎵 𐎶𐎵 𐎶𐎵 𐎶𐎵 𐎶𐎵 𐎶𐎵

¶ The envelope here adds D.P. šu-liru (or šu-gubru).

\*\* The envelope has *saga-bi*.

†† The envelope has the Semitic preposition *ša*, "of," after *ku*.

‡‡ The envelope here adds *meš* (*innantalmeš* = *innanlaweš*).

§§ The envelope has *ka*.

||| The envelope has *Igi Na-bi-El-lil du Ni-di-it-tum*.

¶¶ The envelope has the ideograph for this word.

- Igi Sin-ya-tum du Pi-ir-ḥu-um \*  
 Igi I-li-i-ki-ša-am du Na-ra-am-Addi  
 21. Igi Ap-lum du Ša-ili  
 Igi Nannar-igi-guba, nar  
 Dup D.P. † enim-ma-bi-meš  
 24. ip - ra  
 Mu D.P. Taš-me-tum ‡

TRANSLATION.

- One acre of field-land  
 beside the plantation of Ibni-Sin the gardener  
 3. and beside the field of Ura-Utu, § .  
 The end || the field-land of the sons of Sin-azu  
 and its end the field-land of Utuki-šemi  
 6. the inheritance of Utuki-idinam son of Nannar-me-giš  
 With Utuki-idinam son of Nannar-me-giš  
 Šili-Innanna son of Ili-laḡ  
 9. and Apil-ili his brother  
 they have priced it—  
 1½ shekels of silver  
 12. as its complete price they have given  
 For another day, for a day not existing  
 they shall not dispute  
 15. they shall not withdraw.  
 They have ¶ invoked the spirit of the king.  
 Before Nabi-Elilla (or Nabi-Bêl); \*\*  
 18. Before Kišti-Ura, the scribe;  
 Before Sin-yatum †† son of Pirḥum;  
 Before Ili-ikišam son of Narām-Addi;  
 21. Before Aplum son of Ša-ili;  
 Before Nannar-igi-guba, the *nāru*  
 The seal of the contracting parties  
 24. has been impressed.  
 Year of Tašmētu<sup>m</sup>. ††

\* The envelope has *Igi Sin-mu-ba-li-iṭ du Pir-ḥu-um*.

† The envelope has (*ki* (?) before *enima*).

‡ The envelope has *Iti kin D.P. Innanna, ud u-kam | Mu Ha-am-ru-ra-bi lugal-e | D.P. Taš-me-tum gu-šeg-šeg-ga-ni*.

§ The envelope adds "the chief."

|| The envelope has "its end."

¶ Literally: "he has."

\*\* The envelope adds "son of Nidittum."

†† The envelope has "Before Sin-mubalit."

‡‡ The date on the envelope is:

"Month Elul, day 10th  
 year Ḥammurabi the king  
 Tašmētu<sup>m</sup> besought."



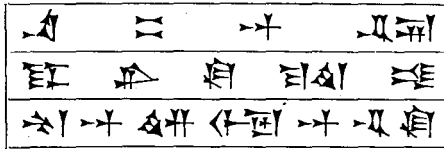
## REMARKS.

It is to be noted that the reading of many of the names is uncertain, especially when one of the component parts is the name of a god. The following Semitic readings may therefore be substituted for some of them:—

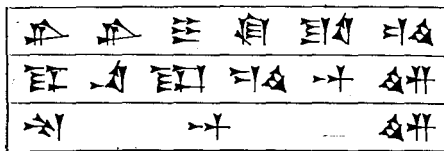
Arad-Šamaš for Uru-Utu  
 Šamaš-šemî for Utuki-šemî  
 Šamaš-idinnam for Utuki-idinnam  
 Nannaru-ḳâli-šemî for Nannar-me-giš  
 Sili-Ištar for Šili-Innanna  
 Īli-pušuši for Ili-laḡ  
 Nannaru-manzaz-pani for Nannar-igi-guba.

In line 19 the substitution of Sin-mubaliṭ for Sin-yatum would seem to imply that the former was brother of the latter, and had taken his place as witness when the envelope was inscribed.

For the sake of completeness I transcribe here such inscriptions of the seal-impressions as can be read easily:—



Na-bi-Bêl	<i>Nabî-Bêl</i>
du Ni-di-it-tum	<i>son of Nidittum</i>
ura Meri u En-ki	<i>servant of Meri* and Enki†</i>



I-li-i-ki-ša-am	<i>Ili-ikišam</i>
du Na-ra-am-Addi	<i>son of Narâm-Addi</i>
ura Meri	<i>servant of Meri*</i>

In Semitic these would be *Nabî-Bêl*, *mâr Nidittum*, *arad Addi û Aê*, and *Ili-ikišam*, *mâr Narâm-Addi*, *arad Addi*, respectively.

\* Meri is the Akkadian name of the god whom the Assyrians and Babylonians called Addu (Hadad) and Rammanu (Rimmon).

† More generally called Êa (Hea), better Aê (Oannes).

The seals of Kišti-Ura (who calls himself “servant of Nergal”) and the son of Pirḫu<sup>m</sup> also occur, but are very difficult to read.

THE BILINGUAL PHRASE-TABLET.\*  
(The Tablet of the Wedding Ceremony.)

(See pp. 143-5).

81—7—1, 98.

COLUMN I.

3	
6	
9	
12	
15	
18	
21	

\* This text was discovered by Mr. Rassam.

† All the characters in outline are attempts to restore the text from syllabaries, parallel passages, &c.

COLUMN II.

	<p>對 十 小 四 四          對 十 小 四 四</p>	<p>四 一 命 十 十          四 一 命 十 十</p>
3	<p>四 四 四 四 四          四 四 四 四 四</p>	<p>四 一 命 十 十          四 一 命 十 十</p>
6	<p>四 四 四 四 四          四 四 四 四 四</p>	<p>四 一 命 十 十          四 一 命 十 十</p>
9	<p>四 四 四 四 四          四 四 四 四 四</p>	<p>四 一 命 十 十          四 一 命 十 十</p>
12	<p>四 四 四 四 四          四 四 四 四 四</p>	<p>四 一 命 十 十          四 一 命 十 十</p>
15	<p>四 四 四 四 四          四 四 四 四 四</p>	<p>四 一 命 十 十          四 一 命 十 十</p>
18	<p>四 四 四 四 四          四 四 四 四 四</p>	<p>四 一 命 十 十          四 一 命 十 十</p>

COLUMN III (reverse).

	<p>全 國 多 十 十 十 十          全 國 多 十 十 十 十</p>	<p>全 國 多 十 十 十 十          全 國 多 十 十 十 十</p>
3	<p>全 國 多 十 十 十 十          全 國 多 十 十 十 十</p>	<p>全 國 多 十 十 十 十          全 國 多 十 十 十 十</p>
6	<p>全 國 多 十 十 十 十          全 國 多 十 十 十 十</p>	<p>全 國 多 十 十 十 十          全 國 多 十 十 十 十</p>

COLUMN III (continued).

9	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫
12	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫
15	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫
18	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫
21	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫
24	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫

COLUMN IV (reverse).

3	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫
6	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫
9	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫
12	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫	𐎲𐎠𐎧𐎺𐎠𐎢𐎽𐎫

COLUMN IV (continued).

15	𒀭 𒀭 𒀭 𒀭 𒀭 𒀭	𒀭 𒀭 𒀭 𒀭 𒀭 𒀭	𒀭 𒀭 𒀭 𒀭 𒀭 𒀭	𒀭 𒀭 𒀭 𒀭 𒀭 𒀭
18	𒀭 𒀭 𒀭 𒀭 𒀭 𒀭	𒀭 𒀭 𒀭 𒀭 𒀭 𒀭	𒀭 𒀭 𒀭 𒀭 𒀭 𒀭	𒀭 𒀭 𒀭 𒀭 𒀭 𒀭

TRANSCRIPTION OF COLUMN I.

<i>Akkadian.</i>		<i>Semitic Babylonian.</i>
	[Dun sila - ta sigsiggabi] . . . . .	[Idlu ša ina] sūku šakummeš . . . pani-šu ittanaššabu
3	[Dun šu namtarrāni šu] [gigga ii]	Idlu ša ina kat namtari-šu maršiš inuḫku
6	[Dun namtarrā]na [sigsigga ba - keš]da	Idlu ša ina šimti - šu šakummatu raksat
	[Dun sila - ta ir]ra [ama ] - tudda	Idlu ša ina suki bikītu umma šūldu - šu
9	[Dun ] - e [su - ni ]-siga	Idlu ša nissatu zumur - šu iṣrupu
12	[Dun dingira]- bi [gulleš in]nadibbida	Idlu ša ili - šu limni[š] iḫmū - šu
	[Dun ] innana - bi [innan] - silla	Idlu [ša] D.P. ištār - šu islituš
15	[Dun da]ma nu - du [du nu] ê - a	Idlu ša aššatam la ihuzu māru la urrabū
18	[Dun ur] - dama - ta [hi - li ] šu - mu - tagga	Idlu ša ina sun aššati-šu kuzub la ilputu
	[Dun ur] - dam - a - na - gam [ku - ba šu - nu] - unsigga	Idlu ša ina sun aššati-šu šubata la išputu
21	[Dun ê] ebura - na - gi . . ba - ra - ê	Idlu ša ina bêt êmuti-su šūšū
24	. . . . .	Idlu . . . . .

COLUMN II.

	<i>Akkadian.</i>	<i>Semitic Babylonian.</i>
	Dun dingira - nu - tug - ra gaba - immanrieš	La bēl il[ani ] imtaḥḥarū
3	Su - nenea šu - ni bangarreš	Ḳati - šu[nu] ana ḳati-šu i[škunū]
6	Gira - nenea gira - ni bangarreš	Šēpi - [šunu] ana šēpi-šu [iškunū]
	Gu - ni gu - da immangara	Ki[šad - sa] itti kišadi-šu [taškun]
9	Nite - a - ni šu - bala - babšin - šā	Raman - šu uštēbillu
	Du-nanna mae-men ban-ingu	Māru rubē anaku, iḳbīš
12	Guskin, kubabar ura-zu baninsi	Kaspu, ḥurašu sun-ka umallu
	Dama - mu ḡimen mae dama - zu ḡia	Atta lû - āššatu anaku lû-mut - ka
15	munabbī	iḳbī - ši
	Duru (?) ḡiš - šar - dim nig la - la	Kima inib kirī ana šāši lalē
18	immingara	ulalli - ši
	Lū - ḡišgal - lu nite - a - ni . . . . .	D.P. Ame[li ramani-šu . . . . .

COLUMN III.

	u . . . . .	. . . . .
3	Su-e-ḡal (?) u - mu - un - ni[ - in] - kešda	. . . . . ruku[s - ma]
	Su-esir giri[- ne - ne] umunnin [kešda?]	Šēnu ana šēpi šu[nu] šēn - ma
6	Su - a - mal - la kešda - ni umunnin - šumu	Naruḳa rakistu idin - šunuti - ma
9	Su-nig-na guskin kubabar ku-tuga - bi umunnin - kešda	Kīsu kaspi ḥurasi ina sisikti - šunu rukus - ma

<i>Akkadian.</i>		<i>Semitic Babylonian.</i>			
12	Kisega ġabarab -	edinna lale	Kispa likallim	ina -	šēri ka
	Ku* u - giri - ku umunnin - gubu		Ina i[ <i>bid</i> ?] šuzi[s] -		asāgi sunuti - ma
15	Īgi - bi D.P. Utu - šua - šu umenim - gara ku umeni - ġur		Pani-šunu ana erib D.P. Šamši tašakkan - ma kima tesru - ma		
18	Zi dingir - galgalēne i - ri - pada ġabara - dun		Niši ilāni rabūti utammi - ka lû - tattallak		
21	[Ku]šurra En - ki - gi - pad sub - ba D.P. Šilig-lu-šar du guru- [dug-ga gi		Kušurra ša D.P. Êa šutukku . . . . ša Marduk [ <i>mâr Eridi</i> ]		
24	anna - ta - pale		la ta- . . . . .		

## COLUMN IV.

	. . ta . . zu (?) ab na . .		. . . . .
	. . ra-ba-an-ma-ku(?) . .		. . . . .
3	[Ê] - a en - na [ba]ranta - rinaš		Adi [ina bēti (?)] la . . . . .
6	[Uru] - a en - na [ba]raziga - ennaš		Adi ina âli la tassuġu
	[U ba]ranbab - kuen [a ba]randab - lagen		Akalu lâ tâkal mê la tašatta
9	[A - aab]ba a - duga [a - šeš]a a id - Digna [a ] id - Puranunu D.S.		Mê tântim <sup>m</sup> , mê tâbutu mê marrutu, mê Idiglat mê Purattu
12	[A - tul - ta] a - idda [baran] - šušu - nen		me bûri, mê mâri la telime
15	[Ana - šu ba - ]ri - en [pa nan - tug]tug		Ana šamê napriš-ma gappa e - taršî
	[kia - šu ba - gubb]en [tur namba - ga]ga		Ina iršitim nakli-ma šubta e - taškun
18	[Dun du dingirra - ]na . . . . . . . . . .		Idlu D.P. mâr îli-šu . . . . . libîb . . . . . -muttu

## TRANSLATION.

## COLUMN I.

[Apparently a list of unfortunate men worthy of the commiseration of the Deity.]

- [The man whom, in] the street, like a barrier,  
[the evil spirit?] before him keeps sitting ;
- 
- 3 The man who, by the hand of his fate  
evilly is treated ;
- 
- The man whom, by his wierd,  
6 a barrier binds ;
- 
- The man whom, in the street, with weeping,  
his mother was caused to bring him forth ;
- 
- 9 The man whom grief  
his body afflicts ;
- 
- The man whose god  
12 evilly binds him ;
- 
- The man whose goddess  
torments him ;
- 
- 15 The man who has no wife,  
(whose) child is not grown up ;
- 
- The man who on his wife's bosom  
18 Has not taken pleasure ;
- 
- The man who on the bosom of his wife  
has not torn the garment ;
- 
- 21 The man who from the house of his affinity  
has been sent forth.
- 
- The man . . . .
- 
- 24 . . . . [Many lines lost.]

## COLUMN II.

[The Words of the wedding-ceremony.]

- The impious  
are approaching
- 
- 3 Their hands  
to his hands [they have placed]
- 
- [Their] feet  
6 To his feet [they have placed].
- 
- [Her] neck  
with his neck [she has placed].
-



- 9 [Him]self  
he has caused to be brought,  
"The son of a prince am I," he has said to her,
- 12 "Silver and gold shall fill thy lap,"  
"Thou shalt be my wife,  
I will be thy husband,"
- 15 he has said to her  
"Like the fruit of a plantation  
to this (woman) abundance
- 18 I will make abundant to her."  
His own people  
.....  
[Many lines lost.]

## COLUMN III.

[Apparently a ceremony after the wedding.]

- ...  
A leathern . . . .
- 3 bind thou on, and  
The shoe on their feet  
place, and
- 6 A strap for binding  
give them, and  
a purse of silver and gold
- 9 in their garment  
bind, and  
a spot in the desert
- 12 may he point out to thee.  
By the stalk of the thorn-vine  
cause them to stand, and
- 15 Before them at sunset  
thou shalt stand, and  
a garment thou shalt put on [and]
- 18 (When) the spirit of the great gods  
has called thee,  
mayest thou go.
- 21 The robe of the god Êa,  
the *sutukku*, . . . .  
of Marduk, son of Eridu
- 24 thou shalt not [transgress?]

COLUMN IV.

[An incantation, probably the continuation of Col. III.]

.....  
 .....

3 Whilst [in the house]  
 thou hast not settled,

Whilst into the city  
 6 thou hast not removed,

Food thou shalt not eat,  
 water thou shalt not drink

9 The waters of the sea, sweet waters,  
 bitter waters, the waters of the Tigris,  
 the waters of the Euphrates,

12 well-water, river-water,  
 thou shalt not taste.

To heaven departing, though  
 15 wings thou hast not gotten?

In earth remaining, though  
 a seat thou hast not made?

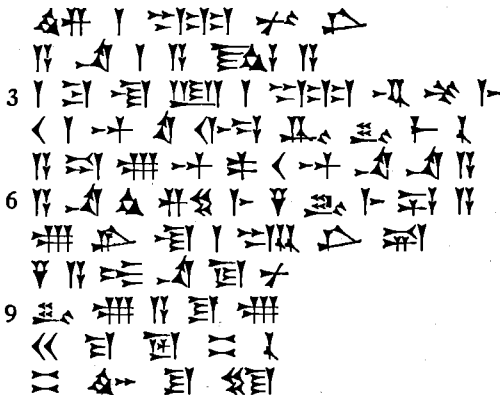
18 The man, the son of his god,  
 . . . . let him be pure.

.....

THE LETTER OF NABŪ-ZÉR-IBNĪ.

(See page 147.)

82-3-23, 925.


  
 3  
 6  
 9

REVERSE.

12 𒀭 𒀭 𒀭 𒀭 𒀭  
 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭  
 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭  
 15 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭  
 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭  
 18 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭  
 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭  
 21 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭  
 EDGE. 𒀭 𒀭 𒀭 𒀭

LEFT HAND EDGE. 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭 𒀭

TRANSCRIPTION AND TRANSLATION.

OBVERSE.

Duppi Nabû-zēr-ibnî a-na Ugar-a	<i>Tablet of Nabû-zēr-ibnî to Ugar,</i>
3 Ba-la-tu, Nabû-bêl-šumāti u Šamaš-udamm-iḫ âhê-šu	<i>Balaṭu, Nabû-bêl-šumāti, and Šamaš-udammik, his brothers.</i>
A-du-u Nabû u Na-na-a 6 a-na balat napšati ša âhe-e-a	<i>Now Nebo and Nanâ For the preservation of the life of my brothers</i>
u-sal-la. Bêl-êp-uš ša a-gan-na-ku-nu	<i>I pray. Bêl-êpuš who (is) along with you</i>
9 âhu-u-a šu-u man-ma dib-bi-šu bi-i-šu-tu	<i>my brother (is) he, whoever his words evil</i>

REVERSE.

12 i-dib-bu-bu ki-i ša âhe-e-a i-li'-u	<i>speaks as my brothers wish</i>
15 lu-sak-ki-tu Šu-u ul-tu re-eš a-di ki-it âhê	<i>let him be silent. As for him, from the beginning to the end brothers of</i>
18 a-ḥa-weš ni-ni Ki-i na-kut-ti a-na âhê-a	<i>each other (are) we. As warning to my brothers</i>

21 Lu-u-tâbu ša âhe-e-a  ip-pu-šu-nu gab-ri ši-pir-ti ša âhe-e-a lu-mur	I send this May it be good what my brothers will do An answer (to this) letter from my brothers let me see.
---	--

THE TABLET OF THE LOAN DURING THE FAMINE.\*

(See page 147.)

81-11-3, 71.

OBVERSE

3  
 6  
 7  
 8  
 9  
 10  
 11  
 12  
 13  
 14  
 15  
 16  
 17  
 18  
 19  
 20  
 21

REVERSE.

9  
 12  
 15  
 18  
 21

\* This and the previous text (p. 161) were also discovered by Mr. Rassam.

## TRANSCRIPTION.

## OBSERVE.

- Parap mana kaspi ša Rêmut mâr . . .  
 ina êli Mušêzib - Marduk u Kul[lâ]  
 3 aššati - šu ana ħubuttu.  
 Ina ūme pan mâti ittaptû,  
 kaspâ, parap mana, ina kaḫḫadi - šu  
 6 Mušêzib - Marduk u Kullâ  
 ana Rêmut  
 inamdinnu.
- 

## REVERSE.

- 9 D.P. Mukinnu: Ablâ,  
 mâr Arad - bêt - Nergal;  
 Šapik - zêri mâr Mušêzib - Marduk;  
 12 Bêl - upaḫḫir mâr Tullubu;  
 Ugarâ mâr Šippê;  
 Nabû - šum - ūsur mâr paḫari;  
 15 ū D.P. rabi, Marduk - êtir. Bâbili,  
 ârah Tebeti, ūmu tišû, šattu tišu - êšrit,  
 Šamaš - šum - ukîn, šar Bâbili.  
 18 Ina ūme - šu, ina âl Lamîma (?),  
 sunkû u dannātu ina mâti [šakin - ma]  
 nisê ina lâ mâkalê  
 21 imuttu
- 

## TRANSLATION.

## OBSERVE.

- $\frac{5}{8}$ ths of a mana of silver from Rêmut son of . . .  
 unto Mušêzib-Marduk and Kullâ  
 3 his wife, for necessities.  
 In the day the face of the land sprouts,\*  
 the money,  $\frac{5}{8}$ ths of a mana, in its full amount,  
 6 Mušêzib-Marduk and Kullâ  
 to Rêmut  
 shall pay.
- 

## REVERSE.

- 9 Witnessing: Ablâ  
 son of Arad-bêt-Nergal;  
 Šapik-zêri, son of Mušêzib-Marduk;
- 

\* Or, "is ploughed," lit. "opened" (Tallqvist).

- 12 *Bél-upaḥḥir son of Tullubu ;  
Ugarā son of Sippé ;  
Nabū-šum-ušur, son of the potter ;*
- 
- 15 *and the scribe, Marduk-ētir. Babylon,  
month Tebet, day 9th, year 19th,  
Saosduchinos king of Babylon.*
- 
- 18 *At this time, in the city of Lamîma (?)  
want and famine is in the land,  
the people without food*
- 21 *are dying.*

## NOTES.

Page 124. Mr. Hormuzd Rassam tells me that the Shatt el Hai is, in his opinion, "a natural outlet from the Tigris to the Euphrates, because, from the nature of its channel, and the flat banks that surround it, there is not the least sign of any embankment having been formed from the soil which naturally would have existed had "the Shatt" been dug out by human hands."

The suggestion that Tell Loh means "the mound of the tablet" I first heard some years ago from Prof. Hommel. It is also to be found in M. de Sarzec's *Découvertes en Chaldée*, p. 8; footnote. With regard to this etymology Mr. Rassam writes to me that in Arabic Tel-loh is written تل لوه, and not تل لوح. "There is a tradition," he says, "in Southern Mesopotamia that Noah lived, after the Deluge, in those parts, and the word لوه may therefore be a corruption of نوح." The derivation which I propose, namely, that *Loh* is a corruption or shortening of *Lagaš*, depends greatly upon the old pronunciation of the *g* in that word. With regard to the disappearance of the last syllable, *aš*, that may have taken place in comparatively recent times. It is worthy of note that a gentleman whose native tongue is Arabic, when speaking of the king whose name has been transcribed *Hammuragaš*, always called him *Hammuraga*. Probably the next stage of weakening would have been *Hammurah*—the same mutilation as the name *Lagaš* seems to have suffered.

Page 125. The god Ê-Girsu seems to have been named after the city Girsu, mentioned in the text here quoted. The principal temple seems to have been called simply  $\text{𒂗𒂗} \text{ 𒂗𒂗} \text{ 𒂗𒂗}$ , *Êgirsu*, which is explained (80-6-17, 1024) as the temple of Ê-Girsu ("the Lord of Girsu" = Nergal) or *Mersû* (the dialectic form of the name).\* From traces of another explanation of the name  $\text{𒂗𒂗} \text{ 𒂗𒂗} \text{ 𒂗𒂗}$  given on the above-quoted fragment, it would seem to have been called, in Semitic Babylonian,  $\text{𒂗𒂗} \text{ 𒂗𒂗} \text{ 𒂗𒂗} \text{ 𒂗𒂗}$ , *Bit teliltu*, "house of glory," which is also the translation of another temple-name, namely  $\text{𒂗𒂗} \text{ 𒂗𒂗} \text{ 𒂗𒂗} \text{ 𒂗𒂗}$ , *Ê-gia*,† the temple of the consort of Ê-Girsu, one of whose many names is  $\text{𒂗𒂗} \text{ 𒂗𒂗} \text{ 𒂗𒂗}$ , *Ê-êgia*.

Page 129 (description of the early Semites of Babylonia). The Chaldean Christians of the Euphrates valley still show the same type.

Page 130. *Ê-ninnû* ( $\text{𒂗𒂗} \text{ 𒂗𒂗}$ ) means "Temple 50." Why it was distinguished by this name we do not know. It was a common thing, however, as will be seen from page 125, to give the temples numbers, though on the tablet there quoted it may have been simply for convenience of reference.



Page 131. *Ga-tumu-dugu* ( $\text{𒂗𒂗} \text{ 𒂗𒂗} \text{ 𒂗𒂗}$ ) ‡ seems also to have been one of the names of the goddess Bau. It was usual with the Assyrians and Babylonians to invoke the same deity under several different names.

"Among the divisions of men *had established his power.*" This is the general sense of these lines. The original has "had set his hand therein."  $\text{𒂗𒂗} \text{ 𒂗𒂗} \text{ 𒂗𒂗} \text{ 𒂗𒂗} \text{ 𒂗𒂗} \text{ 𒂗𒂗}$ , *šū-ni ba-ta-an-ubbā*, Col. III, 18). The "reference to building" seems to record that "he set the beams, he arranged the brickwork" (*gišuru mu-gar, šege nepā*). The characters which Amiaud renders "adorers of demons" are  $\text{𒂗𒂗} \text{ 𒂗𒂗} \text{ 𒂗𒂗} \text{ 𒂗𒂗}$ , for a better rendering of which see the footnote. The phrase "prophetesses of divine decrees" is written with the characters  $\text{𒂗𒂗} \text{ 𒂗𒂗} \text{ 𒂗𒂗} \text{ 𒂗𒂗}$ , *sal-\*-dug-ga*, "woman-\*-saying."




























































\* The full dialectic form of the name of Ê-Girsu is  $\text{𒂗𒂗} \text{ 𒂗𒂗} \text{ 𒂗𒂗}$ , *U-mersi*.

† There is evidently some confusion here on account of the Babylonian  $\text{𒂗𒂗}$  standing for both  $\text{𒂗𒂗}$  and  $\text{𒂗𒂗}$  (Assyrian). From W.A.I. II, 59, 29 the pronunciation *gagia* would rather be expected.









‡ The dialectic form of this name is  $\text{𒂗𒂗} \text{ 𒂗𒂗} \text{ 𒂗𒂗} \text{ 𒂗𒂗}$  *Mašibšib*.

The character represented by the star may be a variant of either  or .

Page 132. The wedge-text of the phrase beginning with line 6 is as follows:—

   	Nam-sig šu-ba	<i>The construction*</i>
   	mu - gal - am	<i>being in hand</i>
   	ki-maga-uru ka	<i>the high place</i>
  	al - nu - gar	<i>of the city was unoccupied</i>
   	uru-ki nu-gin	<i>a funeral pile was unset</i>
    	gal-e* nu-gin	<i>{the minister performed* not the service,* lamen- tation he gave not forth.</i>
   	iri-nu-ta-ê	
   	ama iri-gi iri	<i>the mother of lamentation</i>
  	nu-še-gu	<i>uttered not lamentation</i>
  	ki-šur-ra	<i>in the boundary</i>
   	Lagaša(D.S.)-ka	<i>of Lagaš</i>
  	lu - di - tug	<i>a man making judgment</i>
   	ki-nam-erima-šu	<i>on the place of the oath</i>
  	lu nu - gin	<i>set not a man</i>
  	lu - hur - ra	<i>a man pledging</i>
   	ê lu gu-nu-tağa	<i>the house of a man claimed</i>
		<i>not</i>

The words marked with a star are doubtful, and the whole translation must be regarded as somewhat tentative.

Page 133, line 4, and note\*. The words in the original literally mean "He opened everything to him" (*GAL-muna-KADA*), the compound separable verb *GAL-KADA* being equivalent to *pētû*, "to open." The name *Amālu*<sup>m</sup> (line 5) is written    *Ama-a-lum*—a combination of syllables foreign to *Semitic* Babylonian or Assyrian, who would rather have written     () *A-ma-a-lum* (-num). *Šaku* or *Ušubi*-trees (lines 15-16) were hardly to be regarded as a foreign product, as a plantation of them is mentioned on B. 78 † as existing at Tel-Sifr, or at Warka in the time of

† Strassmaier's *Altbabylonische Verträge aus Warka*.



Samsuiluna (B.C. 2150), but perhaps those of our text were remarkably fine specimens, as they are really called "great šaku-trees" (𐎶 𐎶𐎶𐎶 𐎶𐎶𐎶 𐎶𐎶𐎶).

Page 134. A similar name to *Kâ-gala-ada* = *Abulli-abi-šu*, "His father's great gate," is to be found in the name of the kingdom of which Damascus was the capital, namely *Ša-imēri-šu* ("the country or city) of his ass," probably a derisive etymology manufactured by the Assyrians.

Page 135. The cuneiform text of lines 21-22 is as follows:—

𐎶𐎶𐎶 𐎶𐎶𐎶 𐎶𐎶	Gu-de-a	<i>Gudea</i>
𐎶𐎶𐎶𐎶 𐎶𐎶	šalam - e	<i>this statue</i>
𐎶𐎶𐎶 𐎶𐎶𐎶 𐎶𐎶	gu - im - ma -	<i>command</i>
𐎶𐎶𐎶𐎶	šum - mu	<i>gave:</i>
𐎶𐎶𐎶𐎶 𐎶𐎶𐎶	Šalam lugala -	<i>"The statue of</i>
𐎶𐎶𐎶𐎶	mu	<i>my king</i>
𐎶𐎶𐎶 𐎶𐎶 𐎶𐎶𐎶	u - na - gu	<i>invoke!"</i>

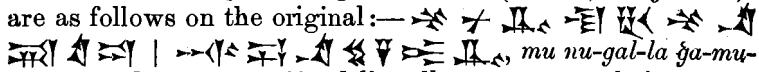
The ending 𐎶𐎶, *e*, seems here to have a demonstrative force.\* GU-imma-SUMMU, is a form from the compound separable verb GU-ŠUMMU, "to give a command,"† and literally means "word it-to gave." *Unagu* (root *gu*) is formed with the imperative prefix *u*, and the infix *na*, literally "do it-to speak." Many read *ša* instead of *u*, which is possible.

Page 136. The original text of lines 15-17 is as follows:—

𐎶𐎶𐎶 𐎶𐎶𐎶 𐎶𐎶𐎶	Ū-ul-li-a-ta	<i>From this day</i>
𐎶𐎶𐎶 𐎶𐎶𐎶 𐎶𐎶𐎶	numuna-ia-ta	<i>of the glorious seed</i>
𐎶𐎶𐎶 𐎶𐎶𐎶 𐎶𐎶	pa-te-si	<i>viceroy</i>
𐎶𐎶𐎶 𐎶𐎶𐎶 𐎶𐎶𐎶	Lagaša D.S.	<i>(of) Lagaš</i>
𐎶𐎶𐎶	Ê - ninnū	<i>Ê-ninnū</i>
𐎶𐎶𐎶 𐎶𐎶𐎶 𐎶𐎶𐎶	D.P. Ê-gir-su	<i>(of) Êgirsu</i>
𐎶𐎶𐎶	lugala - mu	<i>my king</i>
𐎶𐎶𐎶 𐎶𐎶 𐎶𐎶𐎶	u-na-du-a	<i>make</i>

\* See p.136, l. 1, where, on the original, "this statue" is also expressed by 𐎶𐎶𐎶𐎶 𐎶𐎶𐎶 *šalam-e*. See also note \* on the next page, *kia-nag-e* "this place of libation."

† Instead of GU-ŠUMMU, ENIMA-ŠUMMU is also a possible reading.

Page 137. In lines 17 and 18 the words "may he go forth nameless, and may his reign be made (one of) subjection," are as follows on the original:—  
  
*mu nu-gal-la ġa-mu-na-ta-ê, bal-e-na še-gar ġi-gal*, literally "name not-being, may he it-from go-forth, reign-his subjection may it be."

Page 143. (Tablet of the Rival Claimants.) It will be remembered that, in the legal transaction recorded in the book of Ruth, Boaz went to the gate of the city, and agreed with his kinsman there, before the elders, concerning the land which he afterwards redeemed, and the question of wedding also Ruth the Moabitess.

Page 144. It is not unlikely that the whole tablet refers to the wedding-ceremony, but the text has too many and too extensive gaps to enable this to be decided satisfactorily. A translation of the whole will be found on pp. 159-161.

Page 147. Further testimony to the famine in the 19th year of Samsâš-šum-ukin or Saosduchinos occurs on tablet 83-1-18, 2597. We there learn that a man and his son sell their female slave for so much money and *so much corn*. This was in Tammuz—six months earlier than the date of the tablet published. Another tablet, in the possession of Miss Ripley (published by Dr. Budge in the *Proceedings* of the Society of Biblical Archæology for Jan., 1888), dated in the *eighteenth* year of the same king, also makes reference to the famine. The note at the end of this text reads:—"At this time also want and famine are in the land, and mother to daughter opens not the gate" (*Ina ūmu-šu-ma sunku u dan-natu in a māti iššakin-ma ummu ana mārti ūl ipattī bāba*). The state of the country at the time was evidently most appalling.

Pages 154, 157, and 159 (col. ii, ll. 1 and 2). A roughly-written tablet, rather mutilated, apparently gives, as an extract from this text, these two lines and some others preceding them. The corresponding portion of this new text (81-7-1, 207) is as follows:—



DUN DINGIR-NU-TUG-RA GAB-im-ma-an-RI-eš.


Ana idlu lâ-bêlu-îlâni  
 imtaḥḥarū.

"To the man the impious are approaching."

The words "To the man" are omitted in the Semitic version of the large tablet, otherwise the text is the same, except that the small one has  $\text{II}$  for  $\text{VII}$  (the 4th character). It is worthy of note that whilst the Semitic expression for "impious" is given as *lá bêl-îlâni* "a-not-lord-of-the-gods," the Akkadian equivalent of the same is *dingir-nu-tug* "god-not-having." This, as will be seen, explains the variants in the two texts, the one having gotten rid of the post-position *-ra*, "to," by making it the phonetic complement of the of the verb *urra* or *ura*, in *dingira-nu-urra*, and having joined *dun*, "man," "hero" on to this (*dun-dingira-nu-urra*), thus forming of it a single expression, translated by the Semitic *lâ-bêl-îlâni*. "To the man the impious approach," would be, in Akkadian, *Dun-ra dingira-nu-tug gab-immanreiš*. There is no doubt that the copyist of the larger tablet felt the difficulty of this *ra* at the end of the line, and emended his text accordingly.

Line 12. Whilst the Akkadians always, or almost always, said "gold and silver," the Semites of the Euphrates said, "silver and gold." This would imply that the Akkadians had always been acquainted with these two precious metals, and used them as a medium of exchange from the earliest times. The Semitic Babylonians, however, probably at first used silver exclusively. (See also col. iii, l. 8.)

Pages 155, 157, and 160 (col. iii, l. 8). The word *kîsu* here is worthy of note. It is the same as the Hebrew קִיס (Syriac ܩܝܫܐ, Arabic كيس), a purse for money, and for stones used as weights (see Proverbs xvi. 11). The following extract from W.A.I. II, pl. 37, l. 48, will be of interest in connection with this:—


  
 na su- nig-na do. ki - i - si [abnê?]

This shows that the Akkadian *na su-nig-na*, "stone of a skin of stone" is equivalent to the Assyrian *âban kîsi âbnê*,\* "stone (*i.e.*, weight) of a bag of stones" (or "weights"), and some of them are mentioned lower down in the above-named list.

Pages 155, 156, 158, and 161 (col. iv, ll. 3—18). Apparently incantations were to be performed fasting. A similar direction not to eat or drink during a ceremony of this kind

\* Such is, apparently, the way in which it is to be completed.

occurs in W.A.I. IV, pl. 1, col. ii, ll. 56—63. This is in Akkadian only, and has furnished the material for the restoration of the present text.

The words "To heaven departing," etc., seem to mean, "Dost thou think to reach heaven without wings? to remain on earth without a resting-place? Purify thyself, then, with fasting, that, being a son of thy god (*i.e.*, a pious man) thou mayest attain thy desire."

We must wait patiently for the East to yield up its treasures, to enable us to complete this mutilated, but interesting and important text.

Page 163 (Loan-tablet, l. 19). The characters *šakin-ma*, at the end are restored by comparison with the other British Museum text referring to this famine, mentioned on p. 169. Miss Ripley's tablet gives *iššakin-ma*, which is also a probable restoration.

The CHAIRMAN (H. CADMAN JONES, Esq., M.A.)—I am sure I may return the thanks of the meeting to Mr. Pinches for his exceedingly interesting Paper. It is now open for those present to take part in the discussion.

Mr. W. St. C. BOSCAWEN, F.R.Hist. Soc.—I think this is one of the most important Papers that I have seen for many years, and one which has long been wanted, and as Mr. Pinches has devoted so much study to the Akkadian every word of his comes with a special force. I have worked at the same study to some extent; and I must say the conclusions that I have come to are, almost in every case, the same as his. The importance of the monuments which he has described is very great, not only on account of the inscriptions on them, but also by reason of their value from an archæological point of view. The fact that the stone used for these monuments is not to be found anywhere in the neighbourhood of Babylonia, but, as was pointed out at a recent meeting held here, evidently came from the Sinaitic Peninsula, is extremely important, because it has a bearing on the connection between Egypt and Chaldea. At a time when the sixth dynasty were relinquishing the quarries and mines, their place would appear to have been taken by traders from Babylonia. I have lately received from Paris a small chip of the porphyry used in these Babylonian monuments, which I had always been inclined to think were not from the quarries worked in Roman

times, and it is now very interesting to find that it is not the Mons Porphyriticus porphyry, but another kind found in the immediate neighbourhood of Magharah. Mr. Pinches' Paper gives us an extremely valuable insight into Babylonian civilisation. It settles many questions; but one of the most interesting which it leaves open, and which I always maintain will be left open, is that of the disposal of the dead. No doubt in the number of little state communities which grew up in Babylonia, various customs would prevail, as shown by the words themselves. The word for burial may also be used in many cases for burning, as the custom changed. We know in our own country our words for trees have changed from one class to another, as shown by the late Professor Rolleston.

Another point in Mr. Pinches' Paper, which I think of special interest, has reference to the types of the faces. They go to prove that we are not, in Babylonia, to deal emphatically with pure races. It has always been a country of mixed races; and to say this is an Akkadian head or that a Semitic head is almost impossible. From the earliest times we find traces of mixed races there, and no doubt men rose to power in those days by intermarriage; therefore, to get a purely characteristic ethnological type would be extremely difficult. Indeed we never have found, and probably never shall find, any evidence of such purity of type as you find in Egypt; in Egypt the Egyptian language was the one language from the Cataract down to the Delta; with the exception of the infiltration of the Nubian words in one element, and Semitic in the others, it has been changed but little.

With regard to Mr. Pinches' defence of the Akkadian language, I do not think it needs defending. The theory put forward by a Continental Assyriologist is simply a crotchet which scarce requires notice, although indeed even from crotchets one does sometimes get a valuable hint.

M. BERTIN (the late).—I agree with what Mr. Boscawen has said about the theory put forward in regard to the Akkadian language. I would go further and say that it seems a mania. There are two people on the Continent who take up that theory of cryptography. One of these cannot bear the idea of anyone not of Semitic race inventing anything; and so, when any discovery in Assyrian civilisation is attributed to the Akkadians (who were non-Semites), he finds a simple way of doing away with it by sup-

pressing Akkadian and the Akkadians. The other is a very learned German Assyriologist who has found so much difficulty in Akkadian that he has adopted the very simple way of ignoring the existence of the language. But no one can really settle any question in that way!

The Paper is very important, for it deals with the subject practically, and shows us something of the inner life of the people. As to the burial and burning of the dead, I think I was one of the first who expressed the idea that the Akkadians burned their dead. The burning of the dead has been an expensive process at all times. In Holland, for instance, all the rich people were burned and the lower classes, who could not afford to pay, were buried; and so in Egypt, all the rich people were turned into mummies, and the poor were buried; and those who were killed in battle, unless they were victors, were burned to avoid pestilence. As to the remains which are found in the East (in Babylonia and Assyria), showing that people were buried, I do not believe in them, because in all cases where the monuments have been attributed to the Assyrians and Babylonians it has been found on examination that they were neither Assyrian nor Babylonian, but of a later period—the Greek period generally. I have not seen the monuments found in Germany, but I think the Akkadians and the Germans used to burn the dead, and the lower classes were buried like dogs, because they were of no importance.

I believe Akkadian was a dead language a very long time before these inscriptions were written, but that it was the official language to a late period, and that these remains were written in Akkadian at a time when their language was Semitic, and very likely their names were not those given in this Paper, but a Semitic translation. I think that Gudea's name was really Nabû, and not Gudea. The name does not prove the nationality or the language, because people often have names that are not of the language they speak. I am very thankful for what Mr. Pinches has done in regard to this subject, and I hope he will publish much more about those inscriptions of Tell Loh.

Rev. W. ST. CLAIR TISDALL, M.A.—Although I have not yet studied Akkadian very thoroughly, yet what little I do know of it has satisfied me that it was very closely connected with the Turkish family of languages. This is by no means a new discovery, as I am aware, having been pointed out by others. I venture to record

my opinion on the subject, merely in confirmation of this view, and because I have arrived at it independently through comparing Akkadian with modern Osmanli, and more particularly with the Turko-Tâtâric tongues of Central Asia. The resemblance is not confined to words, but on a comparison being instituted between these languages and Akkadian, one is struck by observing that the methods of expressing grammatical relationship, the terminations of the cases, the pronouns and pronominal affixes, and in fact the system underlying, so to speak, the whole framework and arrangement of these tongues are very similar. This incidentally proves—what it is hard to realise having ever been doubted—that Akkadian was really a spoken language, and not a merely artificial tongue invented for the purpose of preserving the secrets of the priesthood. (A similar theory was once urged and learnedly supported by Professor Dunbar with regard to Sanskrit, which he believed was never a spoken tongue, but a literary language formed out of Latin and Greek by the Brâhmans!) The grammar of the Akkadian is so very different in system from that of all Semitic languages that it is impossible seriously to maintain the theory that it was *invented* by Semites.

It has occurred to me—though I have not worked the idea out—that we may still find in other languages words borrowed from the Akkadian which bear witness to the early proficiency of the Akkadians in architecture. The Hebrew הֵיכָלֹּא Aram. הֵיכָלֹּא הֵיכָלֹּא; Syr. ܫܘܚܠܐ, ܫܘܚܠܐ; Arabic هَيْكَلٌ, “a palace,” “a temple,” are known to be derived from the Akkadian *He-gal*, “large house,” “palace.” The Assyrian word *temennu*, “foundation-stone,” is known to be of Akkadian origin. May not the Osmanli-Turkish تَمَلْ (*temel*) “foundation,” be the same word, and is the Greek θεμελιος or θεμελιον certainly a purely Hellenic vocable?

The Akkadian *nen* means “mother.” I have heard the same word in the form *nana* from the lips of a native of Tabrîz, who told me that the word is used as frequently in his native city in this form as in the form *ana* (أنا) which alone is found in Turkish dictionaries. It is well known that the Akkadian *Dimir* or *Dingir*, a god, is the Turkish تَنْغْرِی (*tangri, tengri, tenri*); Chagataish *tangri*, God;

Uigour *tangri*, *tingri*, God, heaven; Yakutish *tañara*, heaven, deity (*vide* Professor Vámbéry, *Etymologisches Wörterbuch der Turko-Tatarischen Sprachen*.)

As to the question of burning *versus* burial in Akkad, it may be worth while to mention that, as we know, in Idian at the present time *both* practices are in vogue among the Hindûs. All caste people, I believe, are burned when they die, as are also some who belong to no caste at all. But many who have no caste or are of very low caste are buried even at the present day. The latter custom seems to have prevailed in India in pre-Aryan times.

Rev. JAMES NEIL, M.A.—First, may I ask when we in England shall know more about those invaluable tablets of Tel-el-Amarna. I was much struck, when Mr. Pinches spoke about the conveyance of land, by the fact that the only tablet referring to that speaks of a plantation and houses. In a Paper recently read here, I called attention to the fact that to this hour there was not anywhere throughout the East, and there never was in ancient times, any individual holding, in broad acres. In all Eastern lands lots were cast every year for every rod of arable ground, owned, as it was, in common by the whole village. Now it is very interesting to see that in all cases where a holding in severalty is mentioned, it was that of plantations and houses, not of broad acres. Almost everything is the same to-day as in Mr. Pinches' pictures of this wonderfully primitive life. I do not know that I could quite yield to so early a date as 3800 B.C., but, not being an Assyriologist, it becomes me to speak very modestly on this point; but the life referred to is evidently very ancient. In the East, as I have said, life is much the same now as then. Money is still lent in times of distress without interest, and they punish infidelity with death. This last is going on now everywhere throughout Palestine and all Syria, and the Turks try in vain to stop it. As to the words of address to the wife, about which Mr. Pinches spoke of feeling some difficulty, it is most interesting to observe that the expression "Be thou the Mother of Millions" is to this very hour the Eastern symbolic way of addressing a bride on the occasion of her marriage by all her relatives and friends. It is indeed an ancient life that Mr. Pinches' Paper reveals, but it is, most of it, the life of to-day.

The HON. SECRETARY (Capt. F. PETRIE).—With regard to the



question asked in reference to the Tel-el-Amarna tablets, it will be in the recollection of all that the first description of them was given to this Institute by Professor Sayce in the 1889 Annual Address. At that time those in the Museum in Berlin and in the Egyptian Museum (now transferred from Boulaq to the Palace at Gizeh), were admirably arranged; and the Berlin Museum afterwards issued excellent photographically illustrated descriptions of those in its possession.\*

REV. W. J. ADAMS, B.A., D.C.L.—Before the discussion closes may I ask a question on a point of Assyrian history? We are told in Scripture that Manasseh, King of Judah, was carried away captive by Esar Haddon out of Jerusalem to Babylon. Now, as Esar Haddon was an Assyrian monarch, we should naturally suppose that he would have taken his captive to Nineveh, not Babylon.

THE AUTHOR.—I will not occupy you very long, as I am a man of few words, as a rule. I will take Dr. Adams' question first because the reply is brief. King Manasseh was taken to Babylon because, as research has shown, Esar Haddon was at that time master of Babylon as well as of Assyria, and held court in that city.

With regard to the question of Anti-Akkadism, of course it is well to remember that one cannot nip error of this kind in the bud too soon. The fact that both the scholars referred to by M. Bertin, one in France and the other in Germany, are Anti-Akkadists, and that they have pupils, makes it probable that they will teach the erroneous doctrine to their pupils, which will naturally bring discredit on our Science later on, because, if we do not shew a bold front and try to disprove these wild statements at once, people will probably say, as they have said before, that we are not agreed amongst ourselves, and are probably very much in doubt as to the reading of words, and the whole history of Assyria and Babylonia. The question of Akkadian being connected with Turkish, as Mr. Tisdall has said, has already been mooted, and he has cited the word "Temen" in support of this. There is still another word which is often quoted, however, and that is the Akkadian "*Dingir*," meaning God, which

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\* Last year, 1892, the British Museum followed suit in this respect.

is compared with the Turkish word (I do not know Turkish except from books) which is, I believe, "Tengri."

I thank those who have spoken for their very appreciative remarks, and I am very pleased that I have succeeded in presenting something which may be regarded as of interest. (Applause.)

The meeting was then adjourned.

## REMARKS ON THE FOREGOING PAPER.

FROM MAJOR C. R. CONDER, B.E., D.C.L., &c.

Mr. Pinches has contributed a most valuable and interesting Paper to the Institute, and no one in England is better fitted to write on the subject. It is satisfactory to see that he attributes a date about 2500 B.C. to the inscriptions, representing the civilisation of Babylonia about the time of Abraham or rather earlier; for some scholars have spoken of these statues as dating about 4000 B.C., for which date there is no sound reason, while the advance in the character of the writing from its first hieroglyphic state to the conventional forms used, is far more probably to be assigned to the date which Mr. Pinches adopts.

De Sarzec's work has been in my possession for the last two years, and represents one of the most important of recent additions to knowledge of Cuneiform writing, and of early Asiatic history. The texts are not only in Akkadian, but in a character so archaic and so nearly approaching the original hieroglyphic forms of the emblems, as to make it clear that these were originally rude sketches of natural objects. None who are unfamiliar with the history of this character would, at first sight, suppose the signs to be the same which in a much modified form were used by the Assyrians 800 years later, but the labours of Amiaud and Méchineau have shown the gradual changes which went on, and have served to connect the oldest and latest forms in a satisfactory manner. It is now clearly shown that the emblems, which at Tell Loh stand upright, while the syllables of the words (as in Hittite) are placed in vertical columns, and the words in the line divided off by vertical divisions, were afterwards turned on their sides, and are so used in the Assyrian and later Babylonian writing.

It was this change which at first rendered it so difficult to understand the hieroglyphic meaning of the emblems.

My interest is chiefly in the light which these and other texts cast on the Hittite question. The Hittite and Cuneiform were separate scripts; yet there can be little doubt that both, with the Chinese and probably the Egyptian, sprang from one original source—a rude system of picture writing—although they developed separately, so that the signs used as grammatical symbols—verb and noun endings, &c.—have no connection. I believe that at least 70 out of some 200 emblems used at Tell Loh may be recognised as having had the same sound and meaning with similar emblems used by the Hittites. These include the signs for “water,” “sprout,” “bird,” “bull,” “yoke,” “ship,” “run,” “city,” “eye,” “heart,” “wind,” “take,” “put,” “go,” “sheep,” “key,” “star,” “throne,” “altar,” “town,” “footprint,” “plant,” “no,” “sun,” the plural emblem, “heaven,” “stag,” “dog,” “tablet,” “tree,” and “arrow;” but when we come to pronouns and case endings the two systems show no connexion at all.

The character of the sculpture at Tell Loh, rude as it is, is superior to that of the Hittite monuments, which are perhaps of the same age or even older. As regards the language there is now every reason to suppose that Akkadian and Hittite were cognate dialects. At Tel-el-Amarna a letter has been found, nearly 1000 years less ancient than the Tell Loh texts, addressed by a Hittite prince to Amenophis III., and written in a dialect very like Akkadian—as has been recognised already in Germany. This fully confirms the theory I proposed in 1887 as to the Hittite. There is also a letter by Dusratta, king of Mitani, written in the language of Mitani—a region in Mesopotamia, east of the Euphrates and opposite the Hittite city of Carchemish. This dialect, as I hope to show in a paper now in print for the Palestine Exploration Fund, was also akin to Hittite and Akkadian, by aid of which it can easily be read.

Now on these views Mr. Pinches' inscription from Tell Loh casts most important light. We see that the Akkadian prince Gudea ruled from the South Sea (Persian Gulf) to the North Sea (or Mediterranean near Alexandretta) and cut cedars on Amanus, the northern mountain of the Lebanon chain. Hence we perceive that in 2500 B.C. the Akkadians were already extending into the Hittite country west of the Euphrates. They have never

before been shown to have gone so far west, but this proof, with the facts deducible from the Tel-el-Amarna letters, viz., that the Hittite language was an Akkadian dialect, and that the intermediate people of Mitani, between Babylonia and Hittite Syria, spoke a kindred language, serves to connect the Hittites and Akkadians, and to shew that the old Mongol race was very widely spread over Western Asia.

I venture to think Mr. Pinches is too modest in speaking of an "Akkadian question." His own labours have added to our knowledge, and it is agreed by authorities such as Sir Henry Rawlinson, Oppert, Lenormant, Delitzsch, Hommel, and Dr. Sayce, that such a language existed, that it was not Semitic, and that in grammatical structure and vocabulary it is closely akin to the Mongol, Turkic, and Finnic languages of later times. I am not aware that any great name save that of Halévy (a Semitic scholar) can be quoted on the other side, and the theory as to cryptograms and secret characters is but one of those clumsy excuses which are set up to bar the way for scientific progress, by prejudiced scholars. We have bilingual texts in Akkadian and Assyrian, bilingual syllabaries explaining to Semitic scribes the Akkadian language, and other such aids to study, which prove beyond doubt the existence of this old Mongolic speech; and the translation of Akkadian texts by scholars who, being versed in Semitic languages, know how to distinguish texts which are not Semitic, puts the question beyond the pale of controversy.

As regards the racial type, the round-headed and high-cheeked personage at Tell Loh is clearly more like a Mongol than like any other type. It may be that these Akkadians shaved—the Phœnicians shaved head and upper lip in 1600 B.C., and the Egyptians shaved—but it may be that the bare face shows Mongol nationality; for the Tartar beard grows very late in life, and the bearded figures—kings and deities—may be intended to represent very ancient and venerable persons.

As regards further study of Akkadian, it seems to me that the method followed by Oppert and Lenormant is the safest, namely, the comparison with the most archaic living dialects of Turkic, Finnic, and Mongol speech. It is true that Chinese has a remote radical connexion with this group; but even the oldest known Chinese dialects are so much corrupted, and have so much in them that is foreign, that Chinese could only be used to illustrate

Akkadian as English could be used to illustrate Sanskrit. The knowledge of English would not enable anyone to understand a Sanskrit book.

If it be finally established that Gudea says that he drove out necromancers and wizards—as Saul drove them out in Israel—this does not of necessity show a very great advance of thought on his part. The Zulu kings in our own day spend most of their time in cooking various magic decoctions, to be used in “smelling out” wizards and witches, and this, which was common to all the ancients, may be here intended. The mention of a holiday, when slaves and masters were equal for a week, reminds us of the Saturnalia among the Romans, celebrated in the middle of December each year. Their Saturnalia also lasted for a week. Slaves were allowed free speech, and even to ridicule their masters. The Roman custom may have been of Etruscan origin, and have come from the East; for there are sound reasons for supposing the Etruscans to have been a people from Asia Minor, of the same Mongol stock with the Akkadians and Hittites.

If Magan really means Sinai or the region near Egypt, the Akkadians in 2500 B.C., would have probably been acquainted with the whole of Palestine. Magan may mean “the wall of the land,” or “the walled land,” and be connected with *Shur*, the wall on the east limits of Egypt. Lenormant has written fully on this question, but it would be well to know for certain that diorite cannot be found nearer than Sinai to Tell Loh. The Hittites also used basalt for their inscriptions, but this they found near them in Syria. As regards *Kimash*, it may perhaps be legitimate to ask whether this has any connection with *Kar-Kamasha* (Car-Chemish) the Hittite Capital. The latter name might mean “City of Kamasha,” and the country might be called Kimash or Kamash. It is not very far from the mountains of the Taurus chain.

The mention of the King having a “tablet in the temple of his God,” is very interesting. It perhaps explains the use of the tablets of Gudea found at Tell Loh and elsewhere; and one cannot but be reminded of the Chinese ancestral tablets, so carefully preserved and, indeed, worshipped. This again is a very characteristic Mongol custom.

As regards burial and burning, it does not seem to have been ever shown that Semitic peoples or Egyptians burned the dead. The Mongols and the Aryans had the custom in early times. In

India and in Europe burial and burning existed, and still exist side by side among the same peoples. It must be remembered that burning was always expensive as compared with burial. Hence only the rich could afford a splendid pyre. But at Tell Loh itself we have a representation of the dead laid in rows head to foot alternately, and covered with a mound to which labourers are bringing baskets of earth. This may represent burial of enemies slain in battle; but there seems to be much to suggest that, while the Semitic peoples buried, the Akkadians burned the dead, or at least burned their chiefs, as did the Tartars in early ages of their history.

I venture to make two suggestions as to Akkadian words: *lu dingirra-nu-dim* might perhaps be rendered "this godlike man, or literally "man + God + this + like." On the other hand, Akkadian syntax would hardly allow of *En Zu*, meaning "Lord of Knowledge," the word should be *En Zu-na* or *Zu-en* "Lord + Knowledge + of" or "Knowledge + Lord."


The freedom of women among Akkadians distinguishes them somewhat from Semitic peoples, though, as Mr. Pinches notes, the Babylonian ladies in later times engaged in trade and business on their own account, and the freedom of women in the East is still, among Arabs, much greater than we suppose at home. The Etruscans also did not seclude their women, who sat at table with the men, and engaged in dances with them.

I may be excused for saying that Mr. Pinches takes rather a gloomy view of the present as compared with the past. We know how furious were the cruel passions of the Assyrians, and I think Assyrian scribes no doubt fell foul of each other as to their writings, much as a certain class of modern pedants have done, not only now, but ever since Jerome's days. M. Mohl complained of this spirit of unworthy bickering when he was President of the Asiatic Society of France, in the days of Botta's first explorations. On the other hand, Mr. Pinches will admit that there is no lack, either at home or abroad, of honest and kindly scholars, who are willing to recognise the value of the work of others, and to take interest in their progress. I at least, as a student of Oriental antiquities, have always found such help, and not least from Mr. Pinches himself.

## FROM THE REV. H. G. TOMKINS.

Mr. Pinches has given in his paper some interesting results of very laborious research. May I offer a few brief remarks :

1. The geographical names in Gudea's long inscription (*pp.* 133-4) deserve careful study. The reading, *Samalu<sup>m</sup>*, reminds me of the Samalna of the list of Thothes III. of Northern Syria


(No. 314. ) which seems to be the Sam'alla land of Assyrian inscriptions, as I have long ago suggested.

2. Is it possible that *Gubin* (*p.* 134) was Gebal, the Kapîna of Egyptian record ?

3. The old name Magan always reminds me of the Mûkna or Makna of the land of Midian, to the east of the head of the Gulf of Akaba, and of the Sinaïtic Peninsula.

4. If Gudea really commanded his statue to invoke the statue of his God, as he would have done if present himself, this would be parallel with the deputed functions of statues in the religious ideas of the Egyptians.

5. *P.* 135. It is very curious to find the characteristic "misrule" of the *Saturnalia* at so very early a date in Southern Babylonia, and the period of seven days is to be remarked in connexion with the institution of the Sabbath. The kind treatment of slaves agrees with intimations in the history of Abraham in the Book of Genesis.

6. *P.* 138. As to the funeral pyre and the supposed cremation, one would like to see the result of further research. The process of burying the dead in a mound is given in a *stela* found by M. de Sarzec (*see* woodcuts in *Babylon*, *pp.* 42, 76), and Loftus and Canon Rawlinson have given much on that subject. Are we really to think that the familiar  of the Hebrew originated in the pyre of cremation, and not in the burial-tumulus ?

7. *P.* 142. The oath by invocation of the spirit or life of the King is of course equally characteristic of the ancient Egyptians. One would greatly rejoice to find monumental information as to the intercourse of these early Chaldæans with the Egyptians, who in their very early dynasties worked Sinaïtic quarries and mines, and used with such consummate skill the intractable diorite, which

was brought eastwards with hardy enterprise for the sculptors of Gudea.

These primæval *rappports* of the great races of the Nile and the Euphrates are among the most attractive problems of history.

I wish to add a few words on the researches of Mr. Pinches in their Biblical bearing : p. 124.—Is it possible that the name *Lagash*, transplanted to Southern Palestine, is the name of the celebrated Canaanite city לַגִּישׁ, *Lakish*, whose ruins Dr. Flinders Petrie has been exploring at Tel-el-Hesi ? I do not see any difficulty in the name being identical, and I should like to know what Assyriologists say to this suggestion.

In my book, *Studies on the Times of Abraham*, I tried to show the value of an enlarged view of the conditions of life under the great primæval civilisations of the Old World as illustrating the narratives of the Old Testament. All that Assyriology and Egyptology can tell us of these things have that specific interest, I mean in their Biblical aspect, as well as the importance that belongs to them in their general bearings on universal history, anthropology, and the like.

For instance, what we learn from these sources of the status of daughters and wives (p. 138), of the confidential and easy condition of house-slaves, of the solemnity of marriage (144), of the strong and trusty special alliance of friends (142-6), and the well-known legal assurances and transfers of property, all bear out the conditions of life under which we find Abraham and his family to have fulfilled their course. I need not refer to well-worn Bibles for proof of these congruities.

Again, the methodical care of registrations and records, of pedigrees and muniments of title; the minute elaboration of the commercial system of securities, and of testamentary dispositions, both in Chaldea and in Egypt, all show how ludicrously defective were our familiar notions of Old World affairs.

But time does not allow me to add more.

This sort of inquiry is making excellent progress; and the readers and lovers of Holy Scripture have nothing to fear, but everything to hope, from such lore as this.



## FURTHER REPLY BY THE AUTHOR.

In Major Conder's valuable remarks I see that he has mentioned the date, 2500 B.C., which I have fixed as that of Gudea, as it is a date which seems to me to be most reasonable; but I am bound to confess that I may be wrong. Perhaps it may be as much too late as that of the French Assyriologists is too early. It may, indeed, be as early as 4000 B.C., but until we get more certain information I think it is better to keep to the lower figure—2500 B.C., or a few hundred years earlier, and I am glad that Major Conder is in agreement with me in this. His note about Carchemish is very interesting; for Kimaš may really be, as he suggests, connected with the second element of the word, namely, *chemish*. The Assyrians call the city Gar-gamiš or Kar-gamiš. The termination *iš* (= *ish*) is suggestive, and recalls various other parallels, such as Ša-imērišu, the Assyrian name of the kingdom of Damascus, probably from a native form Šāimēriš (Šāimēriš); the Rev. H. G. Tomkins's suggestion as to Lachish would bring that name, with Lagaš, into the same category; and the well-known name of a part of Babylonia, Kar-duniaš (Kar-duniash), seems to exhibit the same termination, which, under the form of *aš*, was a common one in the Kassite language. Upon this question, however, a great deal might be said.

I am very glad to see Mr. Tomkins's other remarks; he is a scholar who has taken much interest in the geographical side of the question, and one cannot criticise his statements offhand; nor, indeed, should I feel inclined to do so, because they require consideration.

The oblique-eyed head on Pl. II is a noteworthy illustration of Major Conder's remarks as to the racial type being Mongolian, and bears out in a remarkable way the researches of the Rev. C. J. Ball and Prof. de Lacouperie.

## ORDINARY MEETING.\*

THE PRESIDENT (SIR GEORGE G. STOKES, BART., V.P.R.S.),  
IN THE CHAIR.

The Minutes of the last Meeting were read and confirmed, and the following Elections were announced :—

MEMBER :—The Hon. Martin Brimmer, United States.

ASSOCIATES :—Rev. S. Hungerford, New South Wales ; Rev. H. M. Ladd, D.D., United States ; C. B. Phipson, Esq., J.P., Ireland ; Rev. C. H. H. Wright, D.D., Liverpool.

The following Paper was then read by the Author :—

*THE PHILOSOPHICAL BASIS OF THE ARGUMENT  
FROM DESIGN.* By the REV. J. H. BERNARD, D.D.,  
Archbishop King's Lecturer in Divinity in the University  
of Dublin.

NO one who studies with any diligence the history of the Theistic controversy, since the last century, can fail to be struck with the marked change of tone that has come over the literature on the side of the defenders of religion, as well as on the side of its opponents. The flippancy of Tom Paine and writers of his class has been replaced by a sad and sober criticism ; while on the other hand the confident and dogmatic statements of Paley are exchanged for a cautious and apologetic presentation of the philosophical basis of religion, which shrinks from no charge with such dread as the charge of special pleading. And there is no question but that this change is, on the whole, for the better. No one can doubt that the object of the philosophical writers of our own time who deal with religion is rather to find out the truth than to score a point in controversy with an opponent ; and I am not sure that that could always have been said of religious literature in England.

But laudable as are the motives which keep one from overstating one's case, or from misrepresenting one's opponents,

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\* April 4, 1892.

it is plain that truth is not best served by timidity or by an understatement of what we think the facts before us imply, in order that we may be reckoned generous and large-minded controversialists. I make no apology, therefore, for bringing before the members of the *Victoria Institute* one of those lines of reasoning which have been commonly held by apologists, until quite recently, to attain to all the rigorousness of strict proof. The word "proof" in this connection has, I know, gone out of fashion; but yet we may use it provisionally. Among the various proofs which natural religion has offered for the existence of a supreme and intelligent Governor of the Universe, the argument from design has always been prominent and popular. Alike by Theists and Atheists, by sceptics and believers, it has been regarded from the time of Aristotle as one of the strongest bulwarks of the fortress of religion. It gives at once the most complete, and the most generally intelligible, justification to reason of faith in God; and so deserves the best attention of all seriously-minded persons. I desire to consider as simply as possible in this paper, the basis of the argument, and to discuss briefly one or two objections to it which seem to be of importance at the present time; and if I ask you to accompany me for a brief half-hour into the desert of metaphysics, which many persons regard as a trackless and barren wilderness, rather than invite you to journey along the straight road of so-called common sense, it is because I am convinced that in this journey, as in so many others, the longest way round is really the shortest way home.

Most of us are accustomed to speak as if we regarded the popular distinction between mind, the thing which knows, and matter, the thing which is known, as scientifically accurate, and as a complete statement of the case; and the argument from design then comes to this. We see in the laws and phenomena of the universe traces of order and arrangement beyond what we can ascribe to chance; we see that the world is *κόσμος* not chaos, and hence we conclude that there must be an intelligence behind, which is guiding and controlling the forces of nature in their energies. To this train of reasoning two distinct classes of objections have been made, which we shall consider in order—

1. The materialist first puts in his counter-plea; and though his pleadings may be differently drafted, yet the fundamental principle upon which he relies has been the

same ever since the days of Democritus. Everything that exists, he says, results from a fortuitous concourse of the atoms which are the ultimate constituents of things. What you call mind is but a function of your bodily organism. Thought is merely the result of movement in the grey matter of the brain; it is, in fact, viewed on the subjective side, a secretion of the brain, just as bile is a secretion of the liver. As it was cynically said by a German physiologist, "Was man *isst*, das *ist* er"; man is what he eats, no more. There is no need to assume any entity—to use a barbarous, but convenient term—any entity, distinct from matter to account for the phenomena of personal consciousness, and so *a fortiori* there is no need to assume—nay, by the philosophical Law of Parsimony we are absolutely forbidden to assume—any such mysterious power as the basis of nature. The principles of natural selection and of the survival of the fittest furnish us with a sufficient illustration of the order that we fully admit is traceable in the universe; and that for the simple reason that nothing that is not orderly can continue to exist. Now however sceptical we may be as to the principles of natural and sexual selection being the last word that science has for us on the subject of the order of the universe, yet the general objection here implied would, I believe, be unanswerable if the philosophical creed from which it starts, the creed of materialism, were true. The conclusion seems to follow rigidly from the premises. "Nullus in microcosmo spiritus, nullus in macrocosmo Deus," is a more reliable maxim than most of the aphorisms of scholastic philosophy. And so we cannot dispense ourselves from considering the value of materialism as a system of things. We can never persuade a materialist that the design argument is of any value at all. I shall try to put the accustomed answer of idealists, from Plato down to Green—an answer which seems to me entirely convincing—in two forms.

(a) We assert boldly that the materialist is guilty all through of one of the commonest of logical fallacies—the fallacy of circular reasoning—and that in the following way. He professes to explain away the necessity for *spirit*, *soul*, *mind*, by asserting that what we call mind is only a function of the bodily organism.

But let us ask him, what does he mean by the organism, how does he propose to define those atoms whose co-operation he so often invokes? Now mark his reply, his definition

must be *made in language which is only intelligible for a mind*. His attempt to explain the intelligence as a function of matter ends in nothing, for his account of matter in the ultimate resort will be made by describing it as possessing attributes which have no meaning *except for an intelligence*. And if there be not an intelligence somewhere in the first instance, no satisfactory account is given of matter or, consequently, of the genesis of mind. This answer, be it observed, has nothing whatever to do with theories of Biogenesis or Abiogenesis; the physical possibilities of matter in which eminent scientific men have found "the promise and the potency" of life are not in question. The problem is entirely a metaphysical one and not to be solved in the chemical laboratory.

No matter how far material processes may be investigated, materialism cannot give any rationally complete account of *mind*; for in attempting to explain the genesis of any given individual intelligence, it at least assumes another intelligence behind to watch the process.

But this is not all. Many materialists urge, and they can appeal to well-known scientific facts in support of their assertion, that different forms of mental activity can be localised in different parts of the brain. It can be shown without much difficulty that brain processes always precede mental processes; now, it has been asked, what more is needed to prove that mind is a function of body? The answer to such a question when put in the crude form in which I have stated it, is not far to seek. Suppose it admitted that a certain bodily motion is always the antecedent, a certain mental state the consequent. It does not in the least follow that the bodily antecedent is sufficient *by itself* to account for the mental state which is spoken of as a "consequent." To make such an inference would be to fall into the old logical fallacy, *post hoc, ergo propter hoc*, the blunder of mistaking consecution in time for causation. It may be said that, as a matter of fact, few materialists would urge that the motion of the grey matter of the brain afforded a good and scientifically complete account of thought. They would probably say, as some of them have said, that any mental state may be regarded from two aspects, the objective and the subjective; and that while science gives a sufficient account of the former, the latter aspect is outside its proper region. But it must be remembered that this distinction between the subjective and objective, though valid for an idealist, has no proper meaning and cannot be appealed to by a thorough going materialist, and

to admit such a distinction in the nature of things is to admit all that the most ardent idealist would ask for. The truth is that no matter how accurately the physical antecedents of thought may be determined, this fact must always remain; there is a great gulf fixed between *thought* and *motion*, over which we shall never be able to throw a bridge. The word motion, as I have indicated already, has no meaning except for a mind: and so to explain thought as a mere process of movement is to be guilty of a *circulus in probando*.

(b.) Let us examine this last position from another point of view, that we may see not only the logical inadequacy but the logical impossibility of materialism as a philosophical creed. Plato makes the assertion, and it has never been refuted, that *motion* is only appreciable through *rest*. Now if this be true, it is plain that any theory which would reduce everything in the universe to a modification of motion, must be untrue. If motion cannot be appreciated except by something not itself subject to the laws of motion, it does not give us a complete solution of the problems of nature. Take a fanciful illustration—a borrowed one—but which was originally used (by Prof. W. K. Clifford) to illustrate something quite different. Suppose the case of a worm living inside a perfectly smooth circular tube so uniformly constructed that at no point could there be any sensible difference of bending from any other point, a tube inside which there were no landmarks, so to speak. Is it not plain that the worm—no matter how philosophical a worm he might be—would never know that the tube in which he lived was circular? Suppose him constantly to move round and round, he would never know that he had returned to the same point, and he would not regard the bending of his body as due to anything else than the configuration of the space in which he lived? *He would not know that he lived in a circular space*. How do we know it? Simply because we are not confined within the tube ourselves; we see the worm's limits, and so are beyond, and independent of them.

*Mutato nomine, de te fabula narratur*. If we reflect upon our own mental experience, we shall at once perceive that we regard everything that happens to us, every action in which we are concerned either directly or indirectly, as occurring *in space and in time*. We are not like the worm of our fable, for we are conscious of the limits within which our activity is exercised; and we have seen that such consciousness of limitation implies that the limits are viewed

from a higher stand-point. Now what does this involve? Just this: that the I, the Ego, *das Ich*, who or which experiences everything as in space or in time must itself be timeless and spaceless. The consequence is inevitable. If we are conscious of succession we ourselves do not change, we are permanent. That which is conscious of any series of events cannot itself be part of the series; that which clamps the series, so to speak, is not one of its links. And thus the simplest act of experience is sufficient to lead us to the recognition of that inexplicable mystery which we always come to in our endeavours to explain anything completely. First principles, from their very nature, are not susceptible of proof; otherwise they would not be *first* principles. And so it is impossible, if you will, to demonstrate the presence of the Ego as a distinct factor in any act of consciousness simply because that very demonstration would itself imply the Ego. In the forcible language of the late Mr. Green: "The crowning absurdity of speculation is the endeavour to explain the genesis of thought. . . . To attempt to explain the intelligence by the intelligence is to cut the ground from under your own feet."

The conclusion, then, to which we are impelled by an inexorable logic is that in order to give any intelligible account not merely of the more complicated workings of nature, but of the simplest act of consciousness, we must assume the intelligence, mind, thought,—call it what we will—as an ultimate mystery which baffles explanation and which lies at its root. And therefore it is that materialism is not a satisfactory solution of the problem before us, because it is in truth a huge *petitio principii*, a begging of the question.

Having thus recognised the necessity of assuming what we call *mind* as the basis of our own individual conscious life, it is not hard to see why we attribute minds of like nature to other men. We see that other men act as we do, and that the most reasonable way of accounting for their actions is by supposing that they have minds like ourselves, that they are possessed of an active and spontaneously energising faculty, which is the seat of their personality. But it is instructive to remark that we cannot *demonstrate* this; to cross the chasm which separates my personality from your personality requires a venture of faith, just as emphatically as any theological formula. I can by no means *prove* that that complex of sensations which I constantly experience and which I call the Prime Minister is anything

more than a well-ordered machine. It is improbable that this is the case—highly improbable; but the falsity of such an hypothesis cannot be proved as you would prove the falsity of the assertion that two and two make five. But then though the hypothesis cannot be thus ruled out of court by demonstration of its absurdity, it is not the simplest hypothesis nor is it that one which best accounts for the facts. The assumption, on the other hand, that the men whom I meet every day have minds like my own, perfectly accounts for all the facts, and is a very simple assumption. It merely extends by induction the sphere of a force which I already-know to exist. Or in other words, materialism not giving me an intelligible account of my own individual consciousness I recognise mind, *voûs*, as a *vera causa*, as something which really does produce effects in the field of experience and which therefore I may legitimately put forward as the cause of those actions of other men which externally so much resemble my own. But again, I repeat, this argument, though entirely convincing to any sane person, is not demonstrative; it is open to the more serious of the objections urged by Kant against the design argument for the existence of a Deity. In his technical language, the reasoning here used would seem to be valid only for the reflective and not for the determinant judgment; for the principle of design, as he is never tired of telling us, or conscious adaptation of means to ends, is not a constitutive principle of experience; it is only a regulative principle introduced to account for the facts.

Leaving this aside for a moment, however, what I am endeavouring to show is that the steps by which I manifestly arrive at my knowledge of the existence of other finite minds are exactly similar to those by which the upholder of the design argument claims to arrive at the existence of an Infinite Mind as the basis of nature. For what is that argument? It is this. I observe certain phenomena occurring with order and regularity; I further observe that all so-called natural processes tend towards an end, that nature is full of purpose, that her working seems to be teleological, not merely mechanical; and I assert that the simplest—nay for me the only intelligible—way of accounting for this wonderful order and purpose is to assume a Mind as the Author of it all. And in making such an assumption (and this is the point I wish to emphasise) I am introducing no new and unknown cause; I appeal to no



*Deus ex machina.* I merely say that a force similar to that which I am compelled to regard as the basis of my own personality, similar also to that which I believe to be the spring of action of other human beings, regulates, controls, and orders the energies of Nature. That is the design argument in its simplest form; and so viewed, it is not open to the charge of invoking the aid of a new and unknown force merely to account for the phenomena; but it asserts the operation in nature of a force like to that which we *know* to exist in ourselves.

2. To this analogical way of stating the argument from design, a formidable objection has been lodged by Kant, which has been held to be unanswerable by many of his followers. In Kant's last great work, the *Kritik of the Faculty of Judgment*, the latter part of which is altogether concerned with the problems of teleology, he maintains that although it is perfectly legitimate to conclude from the actions of the lower animals which seem to involve plan, that they are not, as Descartes alleged, mere machines; yet it is not legitimate to conclude from the apparent presence of design in the operations of nature that a conscious mind directs these operations. For Kant argues that in comparing the actions of men and the lower animals, or in comparing the actions of one man with those of another, we are not pressing our analogy beyond the limits of experience. Men and beasts alike are finite living beings, subject to the limitations of finite existence; and hence the law which governs the one series of operations may be regarded by analogy as sufficiently explaining the other series. But the power at the basis of nature is utterly beyond definition or comprehension; and thus we are going beyond our legitimate province if we venture to ascribe to it a mode of operation with which we are only conversant in the case of beings subject to the conditions of space and time. To quote his own words (§ 90 *loc. cit.*): "We can in no way conclude according to analogy, because in the case of finite beings intelligence must be ascribed to the cause of an effect which is judged artificial, that in respect of nature the same law of action which we perceive in men belongs also to a Being quite distinct from, and transcending nature." The same view is thus presented by Hume with his accustomed clearness and force, "In human nature there is a certain experienced coherence of designs and inclinations; so that when from any fact we have discovered one intention of any man, it may often be

reasonable from experience to infer another, and draw a long chain of conclusions concerning his past or future conduct. But this method of reasoning can never have place with regard to a being so remote and incomprehensible, who bears much less analogy to any other being in the universe than the sun to a waxen taper, and who discovers himself only by some faint traces or outlines, beyond which we have no authority to ascribe to him any attribute or perfection.\* Now this position is the root of what is called Agnosticism; and it is a position adopted by many persons who, in other matters, do not call Kant master. It is urged that the whole line of reasoning here adopted proves only what every scientific man—be he a Theist or not—would admit; it only proves that the principle of purpose must be brought in to give any satisfactory explanation of nature; it does *not* prove that nature is really full of purpose, but only that it seems so to a discursive intelligence like ours; and more particularly it fails to prove that that apparent purpose points to a conscious mind.

i. In the first place it is worth while to pause for a moment to note the great concessions which Kant makes to the Theist. He admits fully—nay he insists with emphasis—that the principles of mechanism are quite inadequate to account for the phenomena of nature, *e.g.*, for the phenomena of organic life. We cannot explain organised life in any way without bringing in the idea of purpose; the language of Biologists eloquently shows the impossibility of eliminating at least the idea of design from our investigation of nature, and he adds that we cannot comprehend in any way the apparent adaptation of means to ends in nature unless we bring in the idea of a Supreme Mind (§ 75). For the theoretical needs of biological science, as well as for the practical needs of morals, the idea of God is indispensable; although it is, too, an essential point in the Philosophy of Kant that God's existence cannot be proved to demonstration from the evidence afforded by external nature. It is significant to observe, I think, that this was an essential part of the philosophy of the founder of modern criticism.

ii. But then we go on to inquire: why precisely is our analogical reasoning illegitimate in a theoretical point of view? It is conceded on all hands that it does not amount to demonstration. No analogy does. It is urged that it is

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\* *Essay On a Providence and a Future State.*

because when speaking about man and his mind we thoroughly understand what we are talking about; but in speaking of the Mind of Deity we are dealing with something of which we have no experience, and of which therefore we have no right to predicate anything. The difficulty is real and serious; but let it be observed that even when we infer the existence of another finite mind from certain observed operations, we are making an inference about something which is as mysterious an *x* as anything can be. Mind is not a thing, as we have seen, that is subject to the laws and conditions of the world of sense; it is "*in* the world, but not *of* the world." And so to infer the existence of the mind of any individual except myself is a quite different kind of inference from that by which, *e.g.*, we infer the presence of an electro-magnet in a given field. The action of the latter we understand to a large extent; but we do not understand the action of mind, which yet we know from daily experience of ourselves does produce effects in the outer world, often permanent and important effects. Briefly, the action of mind on matter (to use the ordinary phraseology, for the sake of clearness) is—we may assume for our present purpose—an established fact. Hence the causality of mind is a *vera causa*; we bring it in to account for the actions of other human beings, and by precisely the same process of reasoning we invoke it to explain the operations of nature. It is quite beside the point to urge that in the latter case the intelligence inferred is *infinite*; in the former, only *finite*. All the design argument undertakes to show is that mind—whether finite or infinite it is beyond its province to say—lies at the basis of nature. There is always a difficulty in any argument which tries to establish the operation of mind anywhere, for mind cannot be seen, or touched, or felt; but the difficulty is not peculiar to that particular form of argument with which theological interests are involved.

The real plausibility of this objection arises from a vague idea, often present to us when we speak of *infinite* wisdom or *infinite* intelligence, namely, that the epithet *infinite* in some way alters the meaning of the attributes to which it is applied. But the truth is that the word *infinite*, when applied to wisdom or knowledge or any other intellectual or moral quality, can only have reference to the number of acts of wisdom or knowledge that we suppose to have been performed. The only sense in which we have any right to speak of *infinite* wisdom is that it is that which performs an

infinite number of wise acts. And so when we speak of infinite *intelligence*, we have not the slightest warrant, either in logic or in common sense, for supposing that such intelligence is not similar in kind to that finite intelligence which we know in man.

If all this be granted, it would seem at first sight as if all were granted which the defender of the design argument claims. If the phenomena of nature really exhibit purpose, intelligence, is not this the goal of our inquiry? It would seem as if it might be fairly expected that we had now reached the end of our tedious and intricate journey. But yet some of those who follow us to this point hesitate to go one step—a necessary step—further. The remarkable developments of what is called in Germany the *Philosophy of the Unconscious* have produced yet another class of objections, about which a word must be said.

Nature, it is admitted, works towards an end; yes, that has been proved, but, does it work *consciously* towards an end? Is there any conscious force behind the intelligence that pervades its operations? And it has been argued that though the workings of nature may certainly be described as *intelligent*, for they plainly have a *purpose*, yet we have no right to describe them as *conscious* workings. Nature may be intelligent, but not governed by any conscious Power. But it is hardly too much to say that if human language has any meaning at all, intelligence implies consciousness; if there be a *purpose* in any process it must be a purpose *in* and *for* some mind. For what is intelligent action? It is that action as Dr. Martineau puts it, in which the future dominates the present—the future consequence determines the present operation. But the future can only be thus influential if it is present in idea, and where there is an idea there must be a conscious mind. Of course it is easy to say that this commonplace and simple argument is *anthropomorphism* disguised; and no doubt it is unpleasant to have any argument on which we rely described by so long a word. But what does the charge amount to, what does the statement mean? If it means that I use the words *intelligence* and *purpose* when applied to the mysterious force at the basis of nature in the same sense in which I use them when applied to myself, then the argument is anthropomorphic. But if I do not so use the words I am playing fast and loose with language; if words are not constantly used in the same sense, our theories and our syllogisms are absolutely without value. The point is:

there is no conceivable sense of the words *intelligence* or *design* which can exclude consciousness. An unconscious intelligence is as much a contradiction in terms as a *round square*. And so if scientific evidence sufficient to prove that nature is intelligent, and that its energies are full of purpose, can be adduced—it is only putting our conclusion in another form to say that the force at the basis of nature is a conscious mind, like to that which each one of us experiences as really himself. There is good philosophy in the adage: “Of God above, or man below, what can we reason but from what we know?”

I have not said anything as to the bearing of the doctrines that are generally associated with the name of Darwin upon the argument before us to night, and that for two reasons: (1) In the first place I feel that no one but a properly trained scientific man, who is personally conversant with the laws of the evolution of species, has a right to speak before an assembly like this about theories, the details of which do not seem to an outsider to be yet finally settled; (2) and in the second place, it does not appear that the doctrines in question affect the philosophical basis of the argument to any appreciable extent. No doubt our increased knowledge of natural law would prompt us in this century to state the argument in a somewhat different form from that in which we find it, for example, in the pages of Paley's *Natural Theology*; but in substance it would remain the same. The question before us was, supposing there to be an overwhelming amount of scientific evidence for what looks like design in the phenomena of the universe, what is our philosophical warrant for attributing that to a conscious designer? Of course the objection that comes from certain of Darwin's disciples—I do not think he would have made it himself—is an objection not on the score of logic, but on the score of fact. It is said that what looks like design in organic life may be otherwise accounted for. It is not a case—to use Professor Caird's felicitous phrase—of the environment being adapted to the organism, but of the organism adapting itself to its environment, and so being able to survive. But it is easy to see that this does not touch the real fact of importance which is that the process of the universe is such that it seems to imply purpose somewhere, however we express its law. To suppose that there are such things as organisms at all, in which each part is reciprocally end and means, is quite enough as the basis of the teleological argument; for this involves purpose.

The fact of organic life seems to be *the* conspicuous fact which helps us to unite in one great conception the phenomena of mind and the phenomena of matter, to all appearance so contrasted. And the root of teleology is the principle that nature is not blind mechanism, but that it is the development of freedom, that it is the field of operation of One of whom it was said: "Of Him and through Him and to Him are all things."

The PRESIDENT (Sir G. G. STOKES, Bart., LL.D., D.Sc., V.P.R.S.).—I will ask you to return your thanks to Professor Bernard for this very learned and valuable Paper, and invite discussion thereupon.

Professor E. HULL, LL.D., F.R.S.—Before any discussion commences I should think some of us would very much like to have the views of the President. I do not know whether it would be agreeable to him to make some observations at the outset?

The PRESIDENT.—I would rather hear observations from others. The fact is, my own mind is not of a metaphysical cast, my attention having been rather directed to other subjects.

Professor HULL.—First, I may, I am sure, say for all here that we have listened with great interest to this very logical Paper. I think that as most of us are accustomed to deal with physical or biological subjects, rather than with metaphysical speculations, we must find it salutary to our minds to have to look at questions from a metaphysical point of view. We have heard metaphysics described as "an attempt to explain to another a subject which we ourselves do not understand"; but I am sure you will all agree with me this evening that Dr. Bernard does not come within that category. He has thoroughly grasped the subject with which he deals, and he has treated it in a very convincing manner from his point of view. Now I am afraid that most Physicists, Biologists, Geologists and others of that school, have been accustomed to regard evidence of design in Nature mainly from a physical point of view based upon the consideration of the wonderful adaptation throughout the whole of natural phenomena whether physical or

biological. How, we ask ourselves, could such a wonderful system of adaptation have been introduced into Nature without the exercise from outside of Infinite Wisdom combined with infinite power, by One who comprehends the end from the beginning? We, as human beings, if we are Theists, believe that the Almighty had an end in view in the organisation of this Universe; and that Man himself, if not the great end in view, was at any rate a very important part of that organisation. But Professor Bernard has clearly shown that we really, as biologists, cannot assert the existence of an All-Wise Creator outside and beyond our world as a distinct logical or mathematical proposition, such as that two and two make four. It is a conclusion that we arrive at from inference and analogy; and he has pointed out the analogy. I come to a certain conclusion with regard to certain results; and I suppose that another person, from the action of his mind, has come to a similar conclusion. But I have no positive proof that that is the case (I am describing what Dr. Bernard has in effect said); I cannot demonstrate *that* as I can that two and two make four. It is an inference; and as he has shown, with regard to the operations and the results of natural phenomena and their bearing on the argument from design, we can only reason from analogy and from inference. But after all, does that work in opposition to the views of the Theist? I do not think it does. It amounts to this—which is the more probable—that this world, with its wonderful adaptations, organic and physical, and their adaptation to their environments, should have resulted from “chance,” or from “a fortuitous co-operation of atoms,” rather than from the action of some intelligent Being outside and beyond our world? I should think that when we come to the doctrine of chances, the doctrine would be infinitely against the former supposition. It would be infinitely in favour of the latter supposition; and it is just on those grounds that we maintain, though we cannot demonstrate it as a mathematical proposition, as we can demonstrate that two and two make four, that there has been design in the operations and results of natural phenomena; and I, for my part, am satisfied with that position. I think that ought to be perfectly satisfactory to the Theist, and that it is not necessary that the demonstration should be of a mathematical kind such as two and two make four, or that the three angles of a triangle are equal to two right angles. That is the conclusion I have

come to on hearing this Paper. I myself have gathered some ideas and views from it which I had not previously entertained, and I am very glad now to have heard them so clearly put.

Mr. W. H. ROBINSON.—I have listened with the greatest interest to the Paper, and I have only one fault to find with it—that it is so conclusive, that I really discover nothing to differ from. The design argument of Paley, in spite of modern discussions, I think, stands exactly where it did. I read Paley in my boyhood, and have watched the course of discussion ever since, but have met with nothing whatever that really contradicts Paley, although it is fashionable just now to look upon his argument as quite behind the present state of intellectual advancement. I have, however, seen much that widens the field of his observations. What does he say? He says, “If passing across a heath I kick my foot against a stone, I might say, if I were asked, that the stone had lain there for ever; but if I had kicked my foot against a watch, and had looked at that watch, I should have seen the minute adaptation of all its parts to a designed purpose.” He then reasons, from a like mechanical or material adaptation of the works of nature, to prove the existence of an intelligent personality, who designed them for an evident purpose. He reasoned in this material or physical way, because he wrote in a mechanical age, at a time when the great machines of Watt and others were just coming into use, and his arguments were adapted to his period. But now we live in an age when more abstract modes of discussion prevail, and we can go further. A writer of to-day would not say, if I kicked my foot against a stone, that it had lain there for ever; for the science of Geology has taught us that the stone itself, whatever it may be, is an organic substance, and that there are certain analyses to which it may be subject. In these remarks, I am only indicating a general line of reasoning which may be followed, not only with respect to the mechanical and physical objects which Paley regarded, but through the whole development of human thought, and every chapter of history of the human race. We see, as our great modern poet says, “One purpose runs through all the Suns”—and that it is a purpose, or a power, working for righteousness. We see it in operation everywhere—we see it in our meeting to-night. We see it all around us, persistently sapping the foundations of evil and triumphing over it—not only in the field of biology and



materialism, but also in the moral and spiritual world. We see, I say, design everywhere. Are we to say in contradiction to all who have gone before us,—that there is no Mind above? In the physical kingdom, I contend that wherever we are able to trace the origin of a force, it is always found in what we call, for want of a better term, Mind. In the long run we always dissociate it from mere physical processes in our thoughts. For example, what has impelled me to stand up to-night? Neither the voice of the speaker, nor even the printed page,—these are mechanical or physical causes—but an impression given to my mind. A force of the same kind made the speaker write his Paper, and thus mind operates upon mind to produce physical effects. In whatever direction force is manifested, whenever we *can* trace its origin, that origin is invariably Mind—whether it be the mind of the brute beast, or the mind of man. Then are we to stop short in those cases where we cannot trace the origin of a force, and to say it does not originate in Mind? That would be a contradiction of common sense, and I think common sense ought to have a little weight, even in metaphysics. I think the last speaker is quite right in saying that Biologists cannot, as Biologists, affirm the existence of God. You cannot prove it in that capacity. But surely the Biologist does not give up his human nature because he is a Biologist. He must acknowledge the truth of the axiom of Descartes which has never yet been refuted. *Cogito; ergo sum.* “I think; therefore I am.” We all feel that we are—we all feel that we think. Analyse the brain as much as you like, talk about the transformation of the grey matter of the brain as much as you please; and you have not got to the mind yet. You have only approximated to it, and the attempt to reach it, and define it exactly, will be like the asymptote lines which every mathematician knows, though for ever approaching a certain curve, yet can never possibly reach it. So it is that Science, or to be exact, Physical Observations, to which the name of Science is incorrectly given, will never find out God. It is not the Biologist’s proper aim to do so, it is not his work. What could he expect? The Modern Philosopher speaks of God as The Unknowable, and the definition of Scripture is that He is “past finding out.” Therein comes out the beautiful harmony of Scripture, even with the most advanced philosophical results, which gives a credit, I might almost say a merit, to faith. We must believe even when we cannot prove. I

do not think, as Christians, that we are bound to consider that Biology and Science can find out God. They cannot do it, but still they point to God. They point beyond themselves to Him. Hence it is that we cannot, even tentatively, account for the Universe. We cannot even use the language of Biology or Science itself, unless, as Dr. Bernard pointed out, we have the postulate of a Divine Being—a design, and then there must be a Designer.

The Rev. A. K. CHERILL, M.A.—I should like to say a few words about the scope of the argument from design. It seems to me that the whole argument has suffered a very considerable change in scope and direction of late since Evolution has come so much forward. In the old times, before Evolution was much thought of, when Paley brought forward his argument from design in the way that has just been described, there was this objection taken against it by unbelievers, and it appeared to be a very formidable one: they said, "Design will prove a Designer, of course, but it will not prove any more. It will not prove a God or a Creator of Infinite Power—but on the contrary, the very idea of *design* involves a finite power, an adaptation of means and ends in using and dealing with material. So that the most you can prove by the argument from design is a finite dealing with matter, the work of one who had to do the best he could with matter and to use contrivance and design in order to bring about his purposes!" That *seemed* to be a formidable answer when the argument from design was brought forward to prove the existence of a God, for if He were infinite He would be capable of producing such effects immediately without the necessity for contrivance and design. Then came the theory of Evolution, which, as has been well pointed out at the conclusion of the Paper, turned the whole argument, as it were, quite round, for it proceeded on the adaptation of creatures to their environments; trying to make out that the whole world had resulted in that way—that it had been formed from some primitive state of things by a long process in which, by gradual changes, creatures had become adapted to their environments. Those same elaborate adaptations that had formerly been put forth as proving a designer, were taken up by Evolutionists, and were said to prove the theory of Evolution. It was adaptation looked at from the opposite point of view. But it seems to me that some Evolutionists have rather failed to recognize that the same objection which Agnostics

brought against the argument of design applies with equal force to the argument of Evolution—*i.e.*, if design only proves a finite designer and not an Infinite God, so, in the same way, Evolution only proves a limited course of change, because in order to start Evolution, you want an organism to be evolved to begin with, and an environment in which it is to be placed and to which it shall be adapted. Therefore Evolution cannot be substituted for creation, but it must start from a beginning already produced in some other way, and it cannot trace things down to their first origin. Then what becomes of the argument of design under the theory of Evolution? If you pursue this theory to its furthest extreme, as some try to do, and say that everything has been evolved by a process of natural selection, or that everything is an adaptation of some kind from primordial matter, where then does the argument of design come in? It seems to me that it comes in in this way. Darwin's hypothesis of Natural Selection is sometimes expressed in these terms—that offspring are not exactly like the parent; but there occur chance variations, and that anyone of those variations which happens by chance to be beneficial to the offspring is preserved and intensified by the action of natural selection. That is all very well as far as it goes, but I suppose every student of Evolution, if the point were pressed on him, would have to admit that when he talks about *chance variation*, he is only using a provisional expression accommodated to his own ignorance. Science has nothing to do with chance. Every effect must have a cause. That aphorism lies at the very basis of Science and therefore we may say that there is no such thing as chance. When we talk of chance, we simply mean an effect of which we do not know the cause. It must have some cause, though we do not know what that cause is. Hence, if we pursue the theory of Evolution to its fullest extent, we arrive at a process, leading from undifferentiated matter up to what we have now. In this process there is no chance and therefore its course could not have been other than it has been; or in other words the course of evolution must have been determined beforehand. By what then was it determined? Supposing there were this undifferentiated matter, why did it evolve in one way and not another? The only possible answer that can be given is that there must have been design—a purpose—some purpose to which the whole process of Evolution tends, and if we ask what that

purpose is—if Evolution can tell us anything about it—Evolution tells us in scientific language that it is the “adaptation of creatures to their environments.” Then, as to environment, what is it? I regard myself, for example, and speak of the rest of the world as my environment. If you fix your attention on any one creature whose evolution you are tracing, everything else constitutes the environment of that creature, and consequently, it follows that not only the separate creatures themselves are suffering change, but the environment is changing also. Therefore there is a process not only of adaptation of the creature to a fixed environment, but the adaptation of the creature and of the environment at the same time one to another, proceeding as it were on parallel lines, an advance here, an advance in another place, all advancing together to a more perfect harmony, adaptation and agreement. The end, then, of Evolution, according to the theory of Evolution itself, should at last be perfect harmony between all things and the environments in which they find themselves; and it seems to me, according to theology, that the end of all things is the same; for the end of which theology tells us is “the Communion of Saints”: rational and spiritual beings living in perfect harmony with each other and with their environment. Therefore as far as we can trace an analogy of one to the other, science and theology tell us of the same design in nature, working out to a predestined or foreseen end, which necessarily implies what we may call an Infinite Designer—a Designer who knew the end from the beginning.

Mr. J. KENNEDY, M.R.A.S.—I think the whole argument must ultimately be based on experience. We infer the existence of God as we infer the existence of our fellow-creatures—by experience. The Agnostic denies that he has this experience. Now we can refer him to one source of experience in which the argument from design is most manifest. I refer to the working of God’s Providence. There are laws of Providence as well as laws of Nature, although they are more difficult to discover. The laws of Nature and of Providence are the expression of the nature and the will of God; in both does He reveal Himself, but while the laws of Nature deal with the general conditions of being—and are therefore more easy to discover—the laws of Providence deal with those special circumstances and conditions necessary to produce particular ends; and thus reveal the traces of design in their most striking forms.

The education of this world is full of the overruling Providence of God in the history of nations. The east wind which drove back the waters of the Red Sea is paralleled by the great wind which dispersed the Spanish Armada. But it is in the private history of our own lives that we realise most fully the workings of Providence—how we were led by ways we knew not to ends we dreamt not of. If a man cannot discover the traces of God's designs in the ordering of his own life, then God must for ever remain a hidden God to him.

Professor H. LANGHORNE ORCHARD, M.A., B.Sc.—I think the remark of the last speaker that design is traceable not only in creation but also in Providence, is of very great importance indeed. There is a remark made on page 6 of this Paper which appears to me to go to the very root of the matter. "If we are conscious of succession we ourselves do not change, we are permanent. That which is conscious of any series of events cannot itself be part of the series." In the same way, I suppose, it would be fair to add that that which is conscious of matter cannot itself be material. If this argument be allowed (and it certainly appears to be irresistible) it does away, of course, with Materialism at a stroke.

I should like to make one or two observations on Kant's argument and those remarks which Professor Bernard quoted from Hume. Kant's argument is that "the power at the basis of Nature is utterly beyond definition or comprehension; and thus we are going beyond our legitimate province if we venture to ascribe to it a mode of operation with which we are only conversant in the case of being subject to the conditions of space and time." I think it is tolerably obvious here that Kant assumes a thing which he ought to prove. Is it so, that "we are only conversant with it in the case of being subject to the conditions of space and time"? That is a *petitio principii*. It is surprising that a mind of such extraordinary philosophical power as that of Kant should use so very inconclusive an argument. The reasoning from adaptation of means to ends, to the purposes of such adaptation, has nothing whatever to do, I submit, with being distinct from Nature or being part of it—with transcending nature or not transcending it. He has brought into this argument what is altogether irrelevant to the point of the argument. Hume's argument that we must not infer that a taper and the sun are in any respect of the same character appears really to refute itself. Surely if a taper gives light and

the sun also gives light, it is a fair argument and a logical conclusion to arrive at that there must be something similar in the two things.

I should be glad to hear Professor Bernard's reply to the common hackneyed objection to the anthropomorphic argument. The very fact that man has an idea of God at all proves that there must be some community of nature between God and him. Further, that man was made in the image of God is pretty good proof that we may, within limits, argue from that respecting Him of whom man is the created image. Man was created in the image of God, and, from the image, we can reason 'up to Him of whom he is the image. The argument of the materialist, with regard to Design seems to follow on his vague and foggy notion as to what Cause is. "Cause is invariable antecedent;" says Mill, but if we understand that there is power to produce a change then at once we get something more than mere antecedence. For instance, the presence of food in the mouth must precede the swallowing of it. To argue that that is the cause of swallowing the food is evidently absurd. The argument from design I think really rests on this basis. In any case in which we are able to trace the adaptation of means to ends to a cause, in every case in which we actually do trace it, we find that cause is intelligent—that it proceeds from one's self or other intelligent being. We also find that if we throw, say, a number of papers, up at random in the air, and do that several times, they do not come down in the same order. We find, if we are to produce a certain order of things, there must be design. In cases where we cannot directly trace the cause of this adaptation, it is reasonable to infer, in the absence of any other possible cause with which we are acquainted, that the cause is similar. The principle that "Like causes produce like effects" is a principle that lies at the very root of all inductive experience. If we reject this principle, we reject the principle of induction. I infer from my own mind that other people have minds like my own, for they perform actions which imply design, and I infer that those people have design and purpose, and therefore intelligent minds; and that assumption, or induction, rather, I would say, is found always to work satisfactorily when I apply it to my fellow-creatures. It is not only the only possible reasonable way of accounting for the facts, but, in every case in which it can be verified, it is found to be true, and it is the principle of induction that

I apply to nature in general. I find, in all things in nature, an adaptation of means to ends, both animals to their environments and their environments to them, and so on. I therefore suppose that in those cases, too, the cause of such adaptation of means to ends is conscious intelligence. That can be none other than God the Creator. This seems to me to be a fair statement of the argument of design which is clearly a matter of logical induction. It is not assumption further than is the intuition, "like effects, the like causes"; but that lies at the basis of all our experience. I wish the Paper had been a little longer, and I join with Professor Hull in expressing the hope that the President will favour us with some observations. Those who heard his Gifford lectures would certainly not be disposed to think with himself that his mind had no metaphysical bias.

Rev. A. I. McCaul, M.A.—I had the advantage of reading the Paper beforehand, and must say that I did so with great pleasure. It is very interesting now-a-days to have arguments in defence of design. I would venture to suggest that the unbeliever's objection (to which Mr. Cherrill referred at the beginning of his remarks) that postulates the finite mind, is extra logical—it has nothing whatever to do with the logical process at all. The last speaker conveyed the impression that is on my mind. The argument as to the watch appears to me to be placed on the same footing exactly as the argument for design. The scientific man compares the eye of the fish with that of the human being, and he sees that the former is so constructed as to be able to see under water and he compares it with that of the fly, and so on, and he comes to the irresistible conclusion that these things cannot have come about by chance, but that they involve absolute design. As was said by one speaker, though it may not be capable of mathematical demonstration, yet it is a recognised logical process of induction. A number of instances have been examined by scientific men, and they have come to the conclusion that there is only one conclusion that is possible, and that is, that these results are to be attributed to an intelligent mind. Whether that mind is Infinite or finite has nothing whatever to do with the logical process. The process arrives at the conclusion that there is an intelligent mind, and those who are capable of examining the matter further go on and by a principle of exclusion come to the further conclusion that these exquisite results cannot be attributed

to man—they surpass all human art, power and wisdom, and cannot have come by chance and must, therefore, be attributed to a supernaturally intelligent mind.

I was greatly interested in the way in which Professor Bernard pointed out the ambiguity with which the word "Infinite" is used, and the difficulties and irregularities to which it leads, and it is, I quite think, an explanation of a great deal of the obscurity that attends these special arguments.

The AUTHOR.—Besides the fact that I have to thank the assembly for the very patient hearing they have given a somewhat tedious and intricate Paper, I think I owe those present an explanation and an apology; and as apologies are not always pleasant things, I had better take that first and get it off my mind. The Paper was said, I think, by one speaker, and felt by all, to be obscure. I know it was; but the truth is that, as Bishop Butler points out in the preface to his *Sermons*, obscurity may arise from different causes—it may be due to confusion and obscurity of thought in the speaker, or to carelessness of expression (and I do not pretend that both those causes are entirely absent in my own case). But there is another cause of obscurity, and that is the inherent difficulty of certain subjects. Now the problems of metaphysics can never be popular, just because they are the deepest problems on which the human mind can employ itself. Let me plead then that it is especially hard in a subject of this sort to combine simplicity of expression with scientific precision.

When I was asked by the Victoria Institute to read a Paper, I selected this topic for two reasons. First, my own studies have chiefly lain in the direction of metaphysics as bearing on Theology, and it is better to speak about something with which one is tolerably familiar. And in the second place, this argument of design has been attacked so much of late years from the philosophical side, that it seems desirable to restate in modern language the philosophical basis of the argument, for unless we have a firm grasp of this, it is in vain that we heap up scientific details.

I am happy to find myself in cordial agreement, in the main, with the criticism which Professor Orchard made on Kant's objection to the validity of teleological reasoning as applied to nature. Such an objection, if sound, would prohibit us from inferring the agency of design as the explanation of the actions of



other men, just as surely as it would forbid us to infer the existence of God from His natural operations.

I again thank you for the kind reception accorded to my Paper.

The PRESIDENT.—Before the meeting separates, I wish to say that if I did not join in the discussion myself, it was not from any idea of the obscurity of the Paper, except in so far as the subject itself is necessarily a somewhat obscure one, but because I thought there were others present who were better able to deal with a question of that kind than myself. Therefore I hope the author will not understand that it implied any disparagement of his Paper.

The Meeting then adjourned.

### ORDINARY MEETING.\*

THE REV. PREBENDARY R. THORNTON, D.D., VICE-  
PRESIDENT, IN THE CHAIR.

The Minutes of the last Meeting were read and confirmed, and the following Elections were announced :—

ASSOCIATES :—General C. W. Darling, Cor. Sec. of the Oneida Historical Soc., Utica, United States; Rev. C. W. Cushing, A.M., D.D., United States; Rev. J. M. Mello, M.A., F.G.S., England.

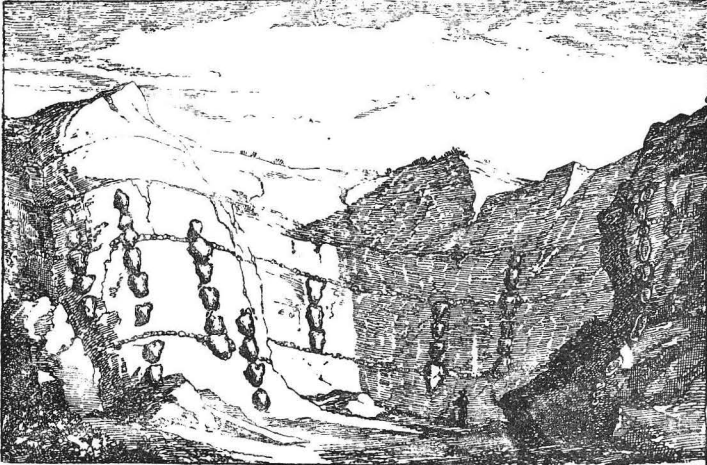
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\* March 21, 1892. The Proceedings at this Meeting are not yet ready for publication.

## ORDINARY MEETING.\*

T. CHAPLIN, ESQ., M.D., IN THE CHAIR.

The following Paper was read by the Author :—



VIEW OF A CHALK PIT AT HORSTEAD, NORFOLK, SEPT. 1, 1838.  
*Showing the Paramoudras in their natural position, but so buried in  
 the chalk that the shape of each individual stone cannot be distinguished.*

**ON THE ENIGMATICAL FLINT BODIES BEARING  
 THE NAME PARAMOUDRA AND WHICH ARE  
 ONLY KNOWN IN THE CHALK OF NORFOLK,  
 AND THE CHALK OF ANTRIM.** By EDWARD  
 CHARLESWORTH, ESQ., F.G.S., &c.

“OF what materials is the Earth composed, and how are those materials arranged?” Such is the brief but most pithily worded proposition with which the late Sir Charles Lyell commences the first edition of the small duodecimo bearing the name “Elements of Geology,” and which supplemented his great work called “Principles of Geology,” the publication of which at once gave its Author a position in the field of scientific research, and philosophical generalisation founded on research, which no future progress made in the same channels of human investigation is ever likely materially to modify. Of what materials then is the

\* 26th Session.

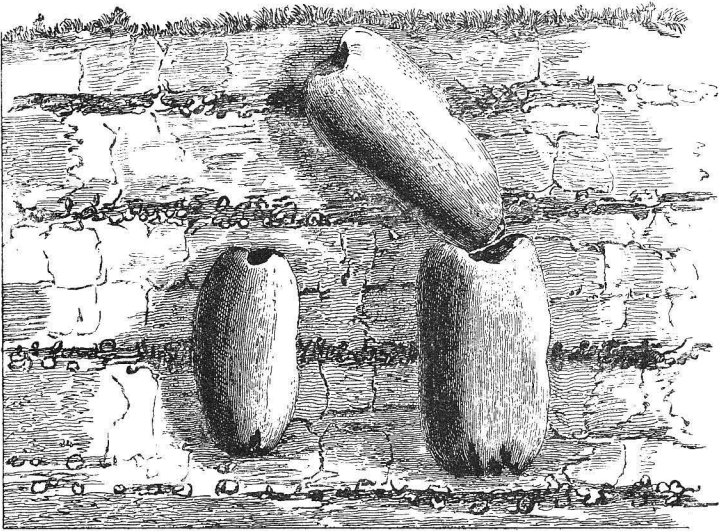
Earth composed? This evening I propose to invite the Members of the Victoria Institute to the consideration of some of the phenomena presented by one of those materials, and that one is the substance known to mineralogists by the name "Silex." This substance under a great variety of forms has a large share in the constitution of that small portion of the Earth beneath the surface accessible to human observation, and which, for the sake of convenience rather than correctness, is called its "*crust.*" In this crust then we find as forms of Silex, the beautiful substance known as Rock-crystal, also Jasper, Carnelian, Chalcedony, Agate, and many others; but the form of Silex with which everyone is familiar, and which in its mass exceeds by millions of times all other varieties of Silex put together, is *flint*, a material which in many parts of England is found so valuable in road-making and in building; many of the churches in East Anglia owing their high preservation and beauty to the flint stones so largely used in their construction. A geological student going into one of the numerous chalk quarries which are to be seen on both sides of the Thames between Gravesend and London, has his attention at once arrested by horizontal strata of flint stones imbedded in the chalk; these flint strata being separated by three or four feet of chalk. Attached to these flints and sometimes enclosed in them are various fossils of the same species as are found in the chalk, consequently the chalk and the flint, though so entirely distinct mineralogically, must be regarded as one geological formation. But flint does not characterise the entire thickness of the chalk, being found only throughout its upper portion. There its presence furnishes the geologist with both mineral and zoological evidence for the identification of the upper portion of the great chalk formation; and while on the one hand, mineralogists and chemists have occupied themselves in attempting to explain the solution of flint in an ocean which must have been so highly charged with lime, and its precipitation from time to time in the condition we now find it, paleontologists, attracted by the numerous organic bodies it preserves, have naturally been led to speculate upon what may be termed the cretaceous aspect of a mineral to which they owe the possession of some of the most interesting objects of their study. Now flint is by no means peculiar to the chalk formation, but the conditions under which it comes under our notice in chalk, constitute a phenomenon of the highest possible scientific interest.

Werner, one of the distinguished names in the early history of geological science, started the theory that during the deposition of the chalk, a quantity of gas was set free, which being unable to escape, gave rise to numerous cavities in the chalk, and that the flint being precipitated from its solution was infiltrated into these cavities. Dr. Buckland refuses to accept this theory, and suggests that probably flint and chalk were deposited together in the form of viscid fluids; and that as the process of consolidation went forward, these two substances separated by cohesive or attractive forces operating uniformly upon the respective atoms of each.

The late distinguished chemist, Dr. Turner, referring to this subject in the *Philosophical Magazine* for July, 1833, observes that although if we now reduce flint to the state of powder no sensible portion of it is dissolved when steeped in water, yet at the moment of its separation from a state of combination with some other mineral body, it is readily soluble: but that while so dissolved the slightest cause will occasion it to revert to the solid state. It is, then, only necessary to assume that the cretaceous ocean had access to rocks in the constitution of which silex was an ingredient, and that these rocks were undergoing decomposition. The silex at the moment of its separation from the other rock constituents would be taken up and disseminated through the waters of the ocean, and its subsequent reversal to the solid form is attributed by Dr. Turner to the emission of gases from the decomposition of organic bodies.

Thus far I have been dealing with the great geological problem of flint as found in chalk. I now pass on to submit to the members of this Society the consideration of a most remarkable enigma connected with the chalk-flint story, which has been an enigma ever since when in the transactions of the Geological Society for 1816 it was brought under the notice of philosophers and men of science by the great geologist, Dr. Buckland. The title of Dr. Buckland's paper is "*Description of the Paramoudra, a singular fossil body found in the chalk of the north of Ireland.*" These singular fossils, says Dr. Buckland, are found in many of the chalk-pits from Moira to Belfast and Larne, but are most numerous at Moira. They are known at Belfast by the name Paramoudra, a word which I could trace to no authentic source, but shall adopt. They have, I believe, never yet been found in the chalk of England, except at Whitlingham near Norwich, whence there is a good specimen in the Geological Society

equal in size to the largest I have seen in Ireland, being about two feet long and one foot in diameter. No two of these bodies are found exactly alike in all their proportions. Their length commonly varies from one to two feet, their thickness from six to twelve inches. Their substance in all cases is flint. These bodies have a central aperture passing through their long diameter. These apertures are always filled with chalk of the same nature as the chalk in which the flint masses are imbedded. Then Dr. Buckland goes on with descriptive details which I pass over, but I quote the Doctor's account of the position of these bodies. The Paramoudras



THE PARAMOUDRAS AS SEEN BY DR. BUCKLAND IN THE CHALK OF ANTRIM.

sometimes lie horizontally, sometimes inclined or erect. They are generally insulated, and altogether unconnected with the ordinary horizontal strata of flints which accompany them. Sometimes the extremities of two specimens are found in contact; but this seems to be the result of accidental juxtaposition, not of any original connexion. But I mention it because an idea used to prevail at Belfast that the Paramoudras are occasionally found linked together in a kind of chain.

The animal history of these fossils, says Dr. Buckland, is involved in much obscurity, as they display no traces of

internal organization sufficient to develop the habits and character of the original bodies whose external features are so distinctly preserved. The central aperture was calculated to allow water to have access to the interior of the animal, as is the case in many hollow sponges which have large single tubes passing into their centre, and usually closed at their lower extremity. It is possible that the Paramoudra, having a tube with two apertures, may have possessed a character intermediate between a sponge and an ascidian. I have broken many of these fossils, and only in one found the smallest trace of organization, and this trace, I think, must have been due to an accidental inclosure of a foreign organic body.

The mineral history of the Paramoudra seems intimately connected with that of many other spongiform bodies which we find in chalk-flints. In all these cases the organic bodies thus preserved appear to have been lodged in the matter of the rock while in the state of a compound pulpy fluid, and before that separation of the flinty from the calcareous ingredient which has given origin to the flints in chalk.

The date of Dr. Buckland's above communication on Paramoudras to the Geological Society was at a time when my opportunities for making geological observations were limited to the out-door excursions of my nurse, for I was then passing through that interesting stage of human evolution known as the long clothes period; and, as in that day, to record in print any facts or opinions brought forward in discussion at the meetings of the Geological Society was regarded as an inexcusable misdemeanour, I am wholly unable to find what kind of reception was given to Dr. Buckland's paper on the occasion of its being read at a meeting of the Geological Society. But what greatly surprises me is this—that Dr. Buckland after the publication of his remarkable discovery seems never to have followed it up. I should have expected that the Doctor, filling as he did the Chair of Geology in the University of Oxford, would have felt it incumbent upon him at an early day after his return from Ireland to have travelled to Norwich and in the chalk quarries of Whitlingham and Horstead have seen how far the Paramoudras of the Norfolk chalk agreed with or differed from the Paramoudras of the chalk of Ireland; and then I should, moreover, have expected that he would have put himself in correspondence with foreign geologists and have learned whether Paramoudras were known in more

parts of the world than England and Ireland. But I am compelled to infer that in the matter of the Paramoudra enigma, Dr. Buckland had no desire to trouble himself further by an attempt at its solution. Happily, however, for the interests of geological science, Dr. Buckland had a contemporary of a very different stamp, and that was Sir Charles Lyell. It was not possible that so remarkable an incident in geological history as massive flint tubes occurring in the chalk of Antrim and Norfolk, and utterly unlike any mineral or organic forms of matter previously known, could be passed over by the lynx-eyed Sir Charles Lyell. Accordingly we find in the volume of the "Proceedings of the British Association" for the year 1838, that the following paper by Sir Charles was read before the Geological section.

"It has long been known that near Norwich the horizontal beds of flint nodules are crossed by perpendicular rows of much larger flints. These larger and vertical flints are locally called 'Potstones,' and are the same as those which occur in the chalk of Ireland, and which have been described by Dr. Buckland under the name 'Paramoudra.' At Horstead, about six miles from Norwich, an excavation has been made nearly half a mile in length, through 26 feet of white chalk, covered by strata of sand, loam and shelly gravel to the thickness of about 20 feet. The rows of vertical 'Potstones' are remarkable for their number and continuity. It is affirmed by those who for more than twenty years have been engaged in quarrying the chalk, that every column of these vertical flints has been found to extend from the top to the bottom of the chalk, so far as the quarrying has been carried downwards. The columns occur at irregular distances from one-another, usually from 20 to 30 feet; and they are not portions of continuous flints in a vertical position, but piles of single flints. Few of the flints are perfectly symmetrical, and they are very unequal in size, usually from a foot to three feet in their vertical length. At the point of intersection between these vertical and the ordinary horizontal lines of flint there is no mutual interruption or shifting; but the two are united as if both were formed at the same time. Each Paramoudra is not a solid flint as is the case with the horizontal flints, but contains within it a cylindrical chalk nucleus, which when deprived of its flint envelope has the form and smooth surface of a tree when stripped of its bark. This internal mass of chalk is much harder than ordinary chalk, and does not crumble under the action of frost. It is seen at the top and the bottom of each Paramoudra. A ventriculate sponge was observed on one occasion in the chalk-nucleus. In conclusion, Sir Charles Lyell invites geologists, who reside near Norwich, to study these phenomena more minutely, and, adverting to the late discoveries of the distinguished German philosopher, Ehrenberg, declares his

expectation that the origin, both of the vertical and the horizontal lines of flint, would be found to be intimately connected with the fossil remains of infusoria, sponges, and other organic bodies."—Sir Charles Lyell, British Association Report for 1838.

As I shall have occasion to refer later on to Sir Charles Lyell's subsequently published views as to the nature of the Paramoudras, for the present I will content myself with remarking that nothing can be more vague and unsatisfactory than saying that flints, whether disposed in vertical or in horizontal lines, are intimately connected with infusoria, sponges and other organic bodies. A precipitation of flint from its solution in sea-water, whether thrown down as silicious jelly or as extremely fine powder, would necessarily be mixed up with the various organic bodies lying on the sea-bed; and in this way all flints may be spoken of as connected with the remains of oceanic life. But how does that connection, if admitted, give us the smallest insight into the nature of a Paramoudra, whether regarded as a single massive flint tube or as a chain of such tubes?

Dr. Buckland's suggestion that a Paramoudra was a link connecting sponges with ascidians, however inadmissible in the present condition of natural history science, was at all events something tangible to be accepted or rejected; but the view put forth by Sir Charles is so vague as to be utterly worthless.

I now proceed to treat of a most important step in the Paramoudra history—one that has been most strangely neglected, and which involves the entire remodelling of the story as known up to 1840. There is living at the Woodlands, Norwich, a magistrate, and enthusiastic antiquarian and geologist, named Fitch (Robert Fitch, Esq., J.P., F.G.S., F.A.S., &c., The Woodlands, Norwich). Sir Charles Lyell, in his Paramoudra article, bearing date 1838, expresses a hope that Norfolk geologists will be led by it to study the Paramoudras more minutely. How far the hope thus put in print influenced my friend, Mr. Fitch, I cannot say, but not long after 1838, Mr. Fitch, with whom I had long been intimate, took me to Horstead, and there to my no small surprise shewed me that the core of chalk in the Paramoudras when broken-up displayed a central green tube; this tube, surrounded with the chalk core, reminding one of a candle wick immersed in tallow. I at once urged my friend to make his discovery public. This he did in the pages of my own journal, the new series of the Magazine of Natural History for 1840, and I added the following note:—



“Can this curious tube be in any way connected with the aggregation of flinty matter forming the Paramoudral column? If the Paramoudras were originally sponges or organic bodies of any kind, how comes it that when broken they present no organic structure or, at any rate, nothing which distinguishes Paramoudra flint from flint as it occurs in the nodular flints of the horizontal layers. After spending a morning in the Horstead Quarry, with Mr. Fitch as guide, and breaking up a number of the Paramoudras, we found the tube present in every case; sometimes, however, so nearly obliterated as to be only traceable by the discoloration of the chalk round its original site. The tube varies in diameter from that of a quill to a finger. The wall of the tube is generally of a green colour and about as thick as the skin of an apple. The substance which fills it is chalk. Dr. Bowerbank finds it to consist of silicious particles.

I believe the sole merit of this interesting discovery rests with my friend Mr. Fitch.”

The late Professor Morris, of University College, in his most valuable list of all British Published Fossils, classes the Paramoudras as sponges; but this location of these bodies as sponges carries no weight with it, because Professor Morris was compelled either to adopt the position assigned by their original describer, or to locate them somewhere else. He being unable to adopt the latter course, naturally was content with following Dr. Buckland, and included the Paramoudras as sponges.

A period of about 50 years having elapsed, Sir C. Lyell renews the consideration of the Paramoudra enigma, and in his “Students’ Elements of Geology,” writes as follows:—

A more difficult enigma is presented by the occurrence (in the chalk) of certain huge flints . . . occurring singly or arranged in nearly continuous columns at right angles to the ordinary and horizontal layers of smaller flints. I visited, in the year 1825, an extensive range of quarries on the River Bure, near Horstead, . . . which afforded a continuous section a quarter of a mile in length, of white chalk, exposed to the depth of about 26 feet and covered by a bed of gravel. The Potstones (Paramoudras) . . . were usually about 3 feet in height and about one foot in transverse diameter, placed in vertical rows like pillars, . . . usually from 20 to 30 feet apart. . . . These rows did not terminate downwards in any instance which I could examine; or upwards, except at the point where they were cut off abruptly by the bed of gravel. . . . Dr. Buckland has described very similar phenomena as characterising the white chalk on the north coast of Antrim. These

masses of flint often resemble in shape and size the large sponges popularly known as 'Neptune's Cups,' which grow in the Seas of Sumatra; and if we could suppose a series of such gigantic sponges to be separate from each other, like trees in a forest and the individuals of each successive generation to grow on the exact spot where the parent sponge died, and was enveloped in calcareous mud, so that they should become piled one above another in a vertical column, their growth keeping pace with the accumulation of the enveloping calcareous mud, a counterpart of the Horstead sponge phenomena might be obtained.—*Vide* Sir Charles Lyell, "Students' Geology," Edition 1885, page 251.

Sir Charles is no longer vague. His new version of the Paramoudra enigma is not that the Paramoudras had in common with all chalk flints some obscure connection with sponges; but that they really are petrified sponges identical with, or allied to the well-known gigantic tropical sponge, popularly called "Neptune's Cup."

Now, how a man of the vast amount of knowledge acquired during a long life devoted to all the branches of study bearing on Geology, could commit such an utter absurdity as to identify Paramoudras with Neptune's Cup sponges is to me as great an enigma as is the Paramoudra itself. The Paramoudra is a cylindrical massive tube open at both ends—if water be poured in at one end, every drop of it runs out at the other. Neptune's Cup sponge is a cup crowning the summit of a massive stalk. If water be put into this cup there it remains, and as this Cup sponge is of a tough leathery nature wholly unlike the sponges in domestic use, I doubt if water would soak through the sponge at all. But whether this be so or not, the comparison of a cup to a tube open at both ends is so at variance with common sense, that to refute the comparison would be wasting words.

I have now, in dealing with the Paramoudra enigma, quoted Dr. Buckland, Professor Morris, Sir Charles Lyell, and myself, the last quotation being a supplementary note to the publication of the highly interesting and important discovery made by my friend, Mr. Fitch. Mr. Fitch wisely, as I think, limits himself to facts, and does not attempt to make his discovery throw light upon the nature of the Paramoudra. But the Members of the Victoria Institute may naturally ask whether, or not, I have any opinion as to the origin of the remarkable bodies I have this evening brought under their notice. Now, my reply is this. We have before us two conditions in the Paramoudra

story, either of which in my opinion is sufficient to negative the sponge, or any other hypothesis which refers the Paramoudras to some once living structure. One of these conditions is the entire absence of structure. Now, I am well aware that when Ventriculites and other forms of sponge life, which flourished in the cretaceous ocean, are found invested by flint, the sponge and the flint are sometimes so intimately blended, that though the shape of the sponge is perfectly preserved, the flint mass on being broken displays no sponge structure. But this obliteration of sponge structure is an exception to the general rule; whereas, in the case of the flint Paramoudras, if we assume them to have been forms of life, the total obliteration of their structure by silicious petrification is invariable; for Dr. Buckland admits that the only indication of structure in breaking up Paramoudras which he has ever met with was probably due to the accidental introduction of some foreign body.

The other condition is this. The Paramoudras, if once living, are all of adult growth. The difference we find in their dimensions is only such as holds good through all adult forms of life.\* What, then, has become of the Baby Paramoudras? Quarrying the chalk in the Norfolk Pits has been turning out Paramoudras, we know, for three quarters of a century, yet, up to this time, no Baby Paramoudra has come to light. The absence of structure, and the absence of Baby Paramoudras, are alike fatal to the organic theory adopted by Dr. Buckland and Sir Charles Lyell. Then we must fall back upon a non-organic origin for the Paramoudras, and here I fully admit that the discarding one hypothesis involves the necessity of attempting to frame another, and to frame that hypothesis is a task beyond my powers of speculative mineralogical construction.

Lastly, what is to be thought of the remarkable discovery made by Mr. Fitch, and the mysterious silence of Sir Charles Lyell, respecting it.

When the proprietorship and editorship of Mr. Loudon's "Magazine of Natural History" passed into my hands, Sir Charles Lyell was one among a band of distinguished men of science who became contributors to its pages.

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\* Mr. Horace B. Woodward, F.G.S., in a letter to the Hon. Secretary of the Victoria Institute says: "At St. James' Pit, Norwich, Mr. Whitaker and myself noticed one Paramoudra nearly 7 feet long, which extended through two bands of flint-nodules."

The supposition that Sir Charles was ignorant of Mr. Fitch's discovery cannot, therefore, for a moment be entertained, and his wholly ignoring that discovery is, to me, incomprehensible. Mr. Fitch's discovery is an additional factor of mystery in the Paramoudra story. Are we to regard the green tube occupying the centre of the chalk core, as an original part of the Paramoudra, or was it of subsequent introduction? It is impossible to overrate the interest of this strangely neglected feature in the Paramoudra story. But here I must bring my discourse to an end, hoping that what I have put before the Members of the Victoria Institute, may have the effect of renewing interest in the attempts to solve one of the most remarkable enigmas in the whole range of geological science.

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#### SUPPLEMENTARY NOTE BY THE AUTHOR.

Ehrenberg, the great authority on Microscopic Life, states as the result of his examination of Paramoudra flints that it exhibits no sponge structure, consequently he rejects the Sponge theory, but accompanies that rejection by a theory of his own, one that appears to me as little satisfactory as the theory advanced by Sir Chas. Lyell (see *Annals of Natural History*, 1893).

Dr. Hinde is the author of an illustrated catalogue of the fossil sponges in the Cromwell Road Museum, a work which will prove of the greatest possible value to students. In this work there is no reference to Paramoudras, an omission which can hardly be regarded as otherwise than a denial that these bodies are sponges. But on the other hand, Prof. Sollas, now of Dublin, a high authority on both recent and fossil sponges, unhesitatingly treats of the Paramoudras as representing the former existence in the cretaceous sea of Neptune's Cup sponges (*Annals of Natural History*, 1880). Now, this living sponge gives us four factors for comparison with its alleged fossil representative—

First, Neptune's Cup has sponge structure throughout its entire mass;

Secondly, it has the essential character of a cup;

Thirdly, the cup may be said to crown a massive stalk;

Fourthly, the stalk has a base of attachment or rather of implantation.

But, a Paramoudra is a hollow flint cylinder, under the microscope shewing—

No sponge structure;

It is not a cup;

Nor has it a stalk;

Nor a base of attachment.

Where then do we find one single point of correspondence between the two ?

Surely the authorities who claim Paramoudras as petrified Neptune's Cup sponges should be able to tell us on what they base the agreement between them.

The CHAIRMAN (T. CHAPLIN, Esq., M.D.) having moved a vote of thanks to the Author,

Professor J. LOGAN LOBLEY, F.G.S., thought we might obtain a clue to the solution of the difficulty if we remembered the behaviour of certain accessory mineral substances in masses of rock, the particles of such minerals having a tendency to aggregate together, thus for instance, one found in the chalk, besides its flint nodules, aggregations of metallic matter, iron pyrites.

Professor HENSON agreed with Professor Loblely.

The Rev. W. B. GALLOWAY, M.A., thought we must look for some force of regular operation in the case of Paramoudras.

Mr. J. T. DAY agreed with Professor Logan Loblely, and thought his view was supported by some investigations which proved that the chalk lying centrally between two layers of flints contained the highest percentage of silica, that in other parts being partly absorbed by the layers of flints, and the inference was that some organic matter, a twig or stem, had served as a nucleus in the case of the Paramoudras.

Other members having spoken, the Author replied.

Votes of thanks were passed to the Royal College of Physicians and to the Geological Society, for the specimens kindly lent to the Institute for the purpose of illustrating the subject under consideration, and the Meeting was then adjourned.

## ORDINARY MEETING.\*

THE PRESIDENT (SIR GEORGE G. STOKES, BART., F.R.S.),  
IN THE CHAIR.

The Minutes of the last Meeting were read and confirmed.

The PRESIDENT.—I regret to say that the Author of this Paper has been unable to leave Edinburgh University, as he had hoped, so as to have been present here this evening; he has therefore asked his friend, Mr. G. G. Chisholm, to read the Paper for him.

*THE GLACIAL PERIOD AND THE EARTH-MOVEMENT HYPOTHESIS.* By Professor JAMES GEIKIE, LL.D., D.C.L., F.R.S., etc.

PERHAPS no portion of the geological record has been more assiduously studied during the last quarter of a century than its closing chapters. We are now in possession of manifold data concerning the interpretation of which there seems to be general agreement. But while that is the case, there remain, nevertheless, certain facts or groups of facts which are variously accounted for. Nor have all the phenomena of the Pleistocene period received equal attention from those who have recently speculated and generalised on the subject of Pleistocene climate and geography. Yet, we may be sure, geologists are not likely to arrive at any safe conclusion as to the conditions that obtained in Pleistocene times, unless the evidence be candidly considered in all its bearings. No interpretation of that evidence which does not recognise every outstanding group of facts can be expected to endure. It may be possible to frame a plausible theory to account for some particular conspicuous phenomena, but should that theory leave

\* 9th Meeting, 27th Session.

unexplained a residuum of less conspicuous, but nevertheless well-proved facts, then, however strongly it may be fortified, it must assuredly fall.

As already remarked, there are many phenomena in the interpretation of which geologists are generally agreed. It is, for example, no longer disputed that in Pleistocene times vast sheets of ice—continental *mers de glace*—covered broad areas in Europe and North America, and that extensive snow-fields and large local glaciers existed in many mountain-regions where snow-fields and glaciers are now unknown, or only meagrely developed. As Professor Penck and others have shown, the line of perennial snow during the glacial period must have been depressed in Central Europe for 3,000 or 3,500 feet—a depression which would correspond approximately to a general lowering of the mean annual temperature of about  $10^{\circ}$  or  $11^{\circ}$  F.\* This, as Penck points out, would bring the climate of Northern Norway down to Southern Germany, and the climate of Sweden to Austria and Moravia, while that of the Alps would be met with over the Mediterranean. It is particularly worthy of notice that the lowering of the temperature was not confined to North-Western and Central Europe, but was general over the whole continent. The Scoto-Scandinavian inland-ice covered many thousands of square miles in the Northern and North-Western portion of the continent; in the Alps and other mountains of Middle Europe great snow-fields and glaciers existed; while further south, as in the Sierra Nevada, Corsica, the Apennines, the Despoto Dagh, etc., only a few isolated local glaciers appeared. Still further south and south-east, as in North Africa and Syria, rainy or pluvial conditions seem to have been contemporaneous with the glacial period of Europe. Thus, it is highly probable—one might almost say certain—that precipitation over the whole continent was greater than now. The geographical distribution of glacial, fluvio-glacial, and other Pleistocene deposits leads, in fine, to the conclusion that in glacial times a wholesale displacement of climatic zones took place. This is most clearly indicated by the Pleistocene system of Europe and Asia, but it is hardly less marked in the corresponding deposits of North America.

It is further to be observed that the glacial conditions of

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\* According to Dr. Brückner the general lowering of temperature may not have exceeded  $5\frac{1}{2}$  to  $7^{\circ}$  F. *Verhandl. d. 73 Jahresversam. d. Schweizerisch. Naturforsch. Ges. in Davos, 1890.*

the Pleistocene period were simply an exaggeration of those now existing. The great inland-ice of Northern Europe is represented to-day by the snow-fields and glaciers of Norway, while the glaciers of the Alps and other mountain-regions are the descendants of those of Pleistocene times. During the glacial period precipitation and accumulation of snow diminished from west to east, and the same is the case at present, for the snow-fields and glaciers of the Western Alps are on a larger scale than those that appear in the eastern portion of the chain. Again, while Norway has its glaciers, in the Urals there is none. Even during the climax of the glacial period the Ural Mountains nourished only a few small local glaciers. We note further that mountains which in our day do not reach the snow-line supported in glacial times relatively small snow-fields and glaciers. The contemporaneous phenomena of North America tell a similar tale. The north-eastern section of that continent was mantled with an immense ice-sheet, while in the far west only gigantic local glaciers existed. To-day the same contrast presents itself; in the north-east we have Greenland drowned in ice, but the loftier mountain-regions of the far North-West, although lying in the same latitude, support only local ice-flows. Were the climatic conditions of the glacial period to return, ice-sheets and glaciers would again extend over the same areas formerly occupied by them. This marked accord between the physical conditions of the Ice Age and those of the present, so far as the ratio of precipitation is concerned, cannot be too strongly emphasized. The old snow-fields, *mers de glace*, and local glaciers accumulated within those areas of northern and temperate latitudes where now-a-days snow and rain are precipitated most copiously; while traces of glaciation are either wholly wanting or very meagrely present in those northern and temperate latitudes which are even now notable for their dryness. It is needless to say that any theory that attempts to account for the glacial climate has these salient facts to reckon with.

The question of the origin of that climate has been greatly complicated by the rapidly increasing evidence which proves that the Ice Age was interrupted by one or more stages during which temperate conditions prevailed. So long as geologists had only one glacial epoch to account for they had less difficulty in suggesting feasible explanations. It was hard or even impossible, however, to reconcile such explanations with the occurrence of interglacial deposits. One is not sur-



prised, therefore, that for some time the evidence of climatic changes during the Ice Age should have been received with considerable doubt. That day of doubt, however, has now well-nigh passed, and geologists generally admit that there have been at least two glacial epochs, separated the one from the other by one well-marked interglacial stage. Indeed, as I shall presently point out, strong evidence has been adduced to show that three or even more glacial epochs, with intervening temperate stages, supervened during the Pleistocene period.

I have said that at least one interglacial epoch is generally admitted by geologists. But I may note here that attempts have often been made to explain away the evidence. It has been again and again suggested, for example, that the interglacial beds indicate no more than local retreats and readvances of ice-sheet and glacier, between the morainic accumulations of which the beds in question appear. This is so very obvious an explanation that it has doubtless occurred to every one who has ever had occasion to give the matter even the slightest consideration. I suppose no one who has been fortunate enough to discover an interglacial deposit has not tried first to account for its presence in this easy way. Nor is it improbable that certain beds containing arctic forms of life, and occupying an interglacial position, are to be thus explained. But there remain a large number of cases which refuse to be thus interpreted—interglacial deposits, which, according to those who have studied them on the spot, are eloquent of very considerable climatic changes. Geologists sometimes forget that in every region where glacial accumulations are well developed, good observers had recognised an upper and a lower series of "drift deposits," long before the idea of two separate glacial epochs had presented itself. Thus, in North Germany, so clearly is the upper differentiated from the lower "diluvium" that the two series had been noted and mapped as separate accumulations for years before geologists had formulated the theory of successive ice-epochs.\* The division of the German "diluvium" into an upper and a lower series is as firmly established as any other well-marked division in historical geology. The stratigraphical evidence has been much strengthened, however, by the discovery between upper and lower boulder-clays of true interglacial

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\* Wahnschaffe: *Forschungen zur deutschen Landes- und Volkskunde von Dr. A. Kirchhoff*. Bd. vi, Heft i.

beds, containing lignite, peat, diatomaceous earth, and marine, brackish, and freshwater molluscs, fish, etc., and now and again bones of Pleistocene mammals.\* A similar strongly-marked division characterises the glacial accumulations of Sweden, as has been clearly shown by De Geer,† who thinks that the older and younger epochs of glaciation were separated by a protracted period of interglacial conditions. In short, evidence of a break in the glacial succession has been traced at intervals across the whole width of the continent, from the borders of the North Sea to Central Russia. M. Krishtafowitsch has recently detected in the neighbourhood of Moscow‡ certain fossiliferous interglacial beds, the flora and fauna of which indicate a warmer or moister climate than the present. The interglacial stage, he says, must have been of long duration, and separated in Russia as in Western Europe two distinct epochs of glaciation.

No mere temporary retreat and re-advance of the ice-front can account for these phenomena. The occurrence of remains of the great pachyderms at Rixdorf, near Berlin, and the character of the flora met with in the interglacial beds of North Germany and Russia are incompatible with glacial conditions in the low grounds of Northern Europe. The interglacial beds, described by Dr. C. Weber§ as occurring near Grunenthal, in Holstein, are among the more recent discoveries of this kind. These deposits rest upon boulder-clay, and are overlaid by another sheet of the same character, and belong, according to Weber, to "that great interglacial period which preceded the last ice-sheet of Northern Europe." The section shows 8 feet of peat resting on freshwater clay, 2 feet thick, which is underlaid by some 10 feet of

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\* For interglacial beds of N. Germany see Helland : *Zeitschr. d. deutsch. geol. Ges.* xxxi, 879 ; Penck : *Ibid.* xxxi, 157 ; *Länderkunde von Europa (Das deutsche Reich)* 1887, 512 ; Dames : *Samml. gemeinverständl. wissenschaft. Vorträge, von Virchow u. Holtzendorff* : xx Ser. 479 Heft ; Schröder : *Jahrb. d. k. geol. Landesanst. f.* 1885, p. 219. For further references see Wahnschaffe, *op. cit.* I have not thought it worth while in this paper to refer to the interglacial deposits of our own islands. A general account of them will be found in my *Great Ice Age, and Prehistoric Europe*. The interglacial phenomena of the continent seem to be less known here than they ought to be.

† *Zeitschrift d. deutsch geolog. Gesellschaft*, Bd. xxxvii, p. 177.

‡ *Anzeichen einer interglazialen Epoche in Central-Russland*, Moskau, 1891.

§ *Neues Jahrbuch f. Mineralogie, Geologie, u. Palæontologie*, 1891, Bd. ii, pp. 62, 228 ; 1892, Bd. i, p. 114.

“coral sand,” with bryozoa. The flora and fauna have a distinctly temperate facies. It is no wonder, then, that continental geologists are generally inclined to admit that North Germany and the contiguous countries have been invaded at least twice by the ice-sheets of two separate and distinct glacial epochs. This is not all, however. While every observer acknowledges that the “diluvium” is properly divided into an upper and a lower series, there are some geologists who have described the occurrence of three, and even more boulder-clays—the one clearly differentiated from the other, and traceable over wide areas. Is each of these to be considered the product of an independent ice-sheet, or do they only indicate more or less extensive oscillations of the ice-front? The boulder-clays are parted from each other by thick beds of sand and clay, in some of which fossils have occasionally been detected. It is quite possible that such stratified beds were deposited during a temporary retreat of the ice-front, which when it re-advanced covered them up with its bottom-moraine. On the other hand, the phenomena are equally explicable on the assumption that each boulder-clay represents a separate epoch of glaciation. Until the stratified beds have yielded more abundant traces of the life of the period, our judgment as to the conditions implied by them must be suspended. It is worthy of note in this connection, however, that in North America the existence of one prolonged interglacial epoch has been well established, while distinct evidence is forthcoming of what Chamberlin terms “stages of deglaciation and re-advancing ice.”\*

When we turn to the Alpine lands, we find that there also the occurrence of former interglacial conditions has been recognised. The interglacial deposits, as described by Heer and others, are well known. These form as definite a geological horizon as the similar fossiliferous zone in the “diluvium” of Northern Germany. The lignites, as Heer pointed out, represent a long period of time, and this is still further illustrated by the fact that considerable fluvial erosion supervened between the close of the first, and the advent of the later glacial epoch. No mere temporary retreat and re-advance of the ice will account for the phenomena. Let us for a moment consider the conditions under which the accumulations in question were laid down.

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\* *Sixth Annual Report, U.S. Geol. Survey, 1884-85, p. 315.*

The glacial deposits underlying the lignite beds, contain, amongst other erratics, boulders which have come from the upper valley of the Rhine. This means, of course, that the ancient glacier of the Rhine succeeded in reaching the Lake of Zurich; and it is well known that it extended at the same time to Lake Constance. That glacier, therefore exceeded 60 miles in length. One cannot doubt that the climatic conditions implied by this great extension were excessive, and quite incompatible with the appearance in the low grounds of Switzerland of such a flora as that of the lignites. The organic remains of the lignite beds indicate a climate certainly not less temperate than that which at present characterises the district round the Lake of Zurich. We may safely infer, therefore, that during interglacial times the glaciers of the Alps were not more extensively developed than at present. Again, as the lignites are overlaid by glacial deposits, it is obvious that the Rhine glacier once more reached Lake Zurich—in other words there was a return of the excessive climate that induced the first great advance of that and other Swiss glaciers. That these advances were really due to extreme climatic conditions is shown by the fact that it was only under such conditions that the Scandinavian flora could have invaded the low grounds of Europe, and entered Switzerland. It is impossible, therefore, that the interglacial flora could have flourished in Switzerland, while the immigration of northern plants was taking place.

Lignites of the same age as those of Dürnten and Utnach occur in many places both on the north and south sides of the Alpine chain. At Imberg, near Sonthofen, in Bavaria, for example, they are described by Penck\* as being overlaid and overlaid by thick glacial accumulations. The deposits in question form a terrace along the flanks of the hills, at a height of 700 feet above the Iller. The flora of the lignite has not yet been fully studied, but it is composed chiefly of conifers, which must have grown near where their remains now occur—that is at 3,000 feet, or thereabout, above the sea. It is incredible that coniferous forests could have flourished at that elevation during a glacial epoch. A lowering of the mean annual temperature by 3° C. only would render the growth of trees at that height almost impossible,

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\* *Die Vergletscherung der deutschen Alpen*, 1882, p. 256.

and certainly would be insufficient to cause the glaciers of Algau to descend to the foot of the mountains, as we know they did—a distance of at least 24 miles. The Imberg lignites, therefore, are evidence of a climate not less temperate than the present. More than this, there is clear proof that the interglacial stage was long continued, for during that epoch the Iller had time to effect very considerable erosion. The succession of changes shown by the sections near Sonthofen are as follows:—

1. The Iller Valley is filled with glacier-ice which flows out upon the low grounds at the base of the Alps.

2. The glacier retreats and great sheets of shingle and gravel are spread over the valley.

3. Coniferous forests now grow over the surface of the gravels; and as the lignite formed of their remains attains a thickness of 10 feet in all, it obviously points to the lapse of some considerable time.

4. Eventually the forests decay, and their débris is buried under new accumulations of shingle and gravel.

5. The Iller cuts its way down through all the deposits to depths of 680 to 720 feet.

6. A glacier again descends and fills the valley, but does not flow so far as that of the earlier glacial stage.

In this section, as in those at Dürnten and Utznach, we have conclusive evidence of two glacial epochs, sharply marked off the one from the other. Nor does that evidence stand alone, for at various points between Lake Geneva and the lower valley of the Inn similar interglacial deposits occur. Sometimes these appear at the foot of the mountains, as at Mörschweil on Lake Constance, sometimes just within the mountain area, as at Imberg, sometimes far in the heart of the Alpine lands, as at Innsbruck. Professor Penck has further shown, and his observations have been confirmed by Brückner, Blaas, and Böhm, that massive sheets of fluvial gravel are frequently met with throughout the valleys of the Alps, occupying interglacial positions. These gravels are exactly comparable to the interglacial gravels of the Sonthofen sections. And it has been demonstrated that they occur on two horizons, separated the one from the other by characteristic groundmoraine or boulder-clay. The lower gravels rest on groundmoraine, and the upper gravels are overlaid by sheets of the same kind of glacial detritus. In short, three separate and distinct groundmoraines are recognised. The gravels, one cannot doubt, are simply the

torrential and fluvial deposits laid down before advancing and retreating glaciers; and it is especially to be noted that each sheet of gravel, after its accumulation, was much denuded and cut through by river-action. In a word, as Penck and others have shown, the valleys of Upper Bavaria have been occupied by glaciers at three successive epochs—each separated from the other by a period during which much river-gravel was deposited and great erosion of the valley-bottoms was effected.

On the Italian side of the Alps, similar evidence of climatic changes is forthcoming. The lignites and lacustrine strata of Val Gandino, and of Val Borlezza, as I have elsewhere shown,\* are clearly of interglacial age. From these deposits many organic remains have been obtained—amongst the animals being *Rhinoceros hemitæchus* and *R. leptorhinus*. According to Sordelli, the plants indicate a climate as genial as that of the plains of Lombardy and Venetia, and warmer therefore than that of the upland valleys in which the interglacial beds occur. Professor Penck informs me that some time ago he detected evidence in the district of Lake Garda of three successive glacial epochs—the evidence being of the same character as that recognised in the valleys of the Bavarian Alps.

In the glaciated districts of France similar phenomena are met with. Thus in Cantal, according to M. Rames,† the glacial deposits belong to two separate epochs. The older morainic accumulations are scattered over the surface of the plateau of Archæan schistose rocks, and extend up the slopes of the great volcanic cone of that region to heights of 2,300 to 3,300 feet. One of the features of these accumulations are the innumerable gigantic erratics, known to the country folk as *cimetière des enragés*. Sheets of fluvio-glacial gravel are also associated with the moraines, and it is worthy of note, that both have the aspect of considerable age—they have evidently been subjected to much denudation. In the valleys of the same region occurs a younger series of glacial deposits, consisting of conspicuous lateral and terminal moraines, which, unlike the older accumulations, have a very fresh and well-preserved appearance. With them, as with the older moraines, fluvio-glacial gravels are associated. M. Rames shows that the interval that supervened between

\* *Prehistoric Europe*, p. 303.

† *Bull. Soc. Geol. de France*, 1884.

the formation of the two series of glacial deposits must have been prolonged, for the valleys during that interval were in some places eroded to a depth of 900 feet. Not only was the volcanic *massif* deeply incised, but even the old plateau of crystalline rocks on which the volcanic cone reposes suffered extensive denudation in interglacial times. M. Rames further recognises that the second glacial epoch was marked by two advances of the valley-glaciers, separated by a marked episode of fusion, the evidence for which is conspicuous in the valley of the Cère.

The glacial and interglacial phenomena of Auvergne are quite analogous to those of Cantal. Dr. Julien has described the morainic accumulations of a large glacier that flowed from Mont Dore. After that glacier had retreated a prolonged period of erosion followed, when the morainic deposits were deeply trenched, and the underlying rocks cut into. In the valleys and hollows thus excavated freshwater beds occur, containing the relics of an abundant flora, together with the remains of elephant (*E. meridionalis*), rhinoceros (*R. leptorhinus*), hippopotamus, horse, cave-bear, hyæna, etc.—a fauna comparable to that of the Italian interglacial deposits. After the deposition of the freshwater beds, glaciers again descended the valleys and covered the beds in question with their moraines.\*

According to the researches of Martins, Collomb, Garrigou, Piette, and Penck, there is clear evidence in the Pyrenees of two periods of glaciation, separated by an interval of much erosion and valley-excavation. Penck, indeed, has shown that the valleys of the Pyrenees have been occupied at three successive epochs by glaciers—each epoch being represented by its series of moraines and by terraces of fluvio-glacial detritus, which occur at successively lower levels.

I have referred in some detail to these discoveries of interglacial phenomena because they so strongly corroborate the conclusions arrived at a number of years ago by glacialists in our own country. Many additional examples might be cited from other parts of Europe, but those already given may serve to show that at least one epoch of interglacial conditions supervened during the Pleistocene period. Before leaving this part of my subject, however, I may point out the significant fact that long before much was known of

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\* *Des Phénomènes glaciaires dans le Plateau Central de France, etc.*

glaciation, and certainly before the periodicity of ice-epochs had been recognised, Collomb had detected in the Vosges conspicuous evidence of two successive glaciations.\*

Having shown that alike in the regions formerly occupied by the great northern ice-sheet, and in the Alpine lands of Central and Southern Europe, alternations of cold and genial conditions characterised the so-called glacial period, we may now glance at the evidence supplied by those Pleistocene deposits that lie outside of the glaciated areas. Of these we have a typical example in the river-accumulations of the Rhine valley between Bâle and Bingen. Here and there these deposits have yielded remains of extinct and no longer indigenous mammals and relics of Palæolithic man—one of the most interesting deposits from which mammalian remains have been obtained, being the Sands of Mosbach, between Wiesbaden and Mayence. The fauna in question is characteristically Pleistocene, nor can it be doubted that the Mosbach Sands belong to the same geological horizon as the similar fluvial deposits of the Seine, the Thames, and other river-valleys in Western Europe. Dr. Kinkelin has shown,† and with him Dr. Schumacher agrees,‡ that the Mosbach deposits are of interglacial age; while Dr. Pohlig has no hesitation in assigning them to the same horizon.§ It is true there are no glacial accumulations in the region where they occur, but they rest upon a series of unfossiliferous gravels which are recognised as the equivalents of the fluvio-glacial and glacial deposits of the Vosges, the Black Forest, the Alps, etc. These gravels are traced at intervals up to considerable heights above the Rhine, and contain numerous erratics, some of which are several feet in diameter, while a large proportion are not at all waterworn, but rough and sharply angular. The blocks have unquestionably been transported by river-ice, and imply therefore cold climatic conditions. The overlying Mosbach Sands have yielded not only *Elephas antiquus* and *Hippopotamus major*, but the reindeer, the mammoth, and the marmot—two strongly contrasted faunas, betokening climatic

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\* *Preuves de l'existence d'anciens glaciers dans les vallées des Vosges*, 1847, p. 141.

† Kinkelin: *Bericht über die Senckenberg. naturf. Ges. in Frankfurt a. M.*, 1889.

‡ Schumacher: *Mittheilungen d. Commission für d. geolog. Landes-Untersuch. v. Elsass-Lothringen*, Bd. ii, 1890, p. 184.

§ *Zeitschr. d. deutsch. geolog. Ges.*, 1887, p. 806.



changes similar to those that marked the accumulation of the river-deposits of the Thames, the Seine, etc. Of younger date than the Mosbach Sands is another series of unfossiliferous gravels, which, like the older series, are charged with ice-floated erratics. The beds at Mosbach are thus shown to be of interglacial age: they occupy the same geological horizon as the interglacial beds of Switzerland and other glaciated tracts in Central and Northern Europe.

To this position must likewise be assigned the Pleistocene river-alluvia of other districts. There is no other horizon, indeed, on which these can be placed. That they are not of postglacial age is shown by the fact that in many places the angular gravels and flood-loams of the glacial period overlies them. And that they cannot all belong to preglacial times is proved by the frequent occurrence underneath them of glacial or fluvio-glacial accumulations. It is quite possible, of course, that here and there in the valleys of Western and Southern Europe some of the Pleistocene alluvia may be of preglacial age. But in the main these alluvia must be regarded as the equivalents of the glacial and interglacial deposits of northern and alpine districts. This will appear a reasonable conclusion when we bear in mind that long before the Pliocene period came to a close the climate of Europe had begun to deteriorate. In England, as we know, glacial conditions supervened almost at the advent of the Pleistocene period. And the same was the case in the alpine lands of the south. Again, in the glaciated areas of north and south alike, the closing stage of the Pleistocene was characterized by cold climatic conditions. And thus in those regions the glacial and interglacial epochs were co-extensive with that period. It follows, therefore, that the Pleistocene deposits of extra-glacial areas must be the equivalents of the glacial and interglacial accumulations elsewhere. If we refused to admit this we should be puzzled indeed to tell what the rivers of Western and Southern Europe were doing throughout the long-continued glacial period. There is no escape from the conclusion that the Pleistocene river-alluvia and cave-accumulations must be assigned to the same general horizon as the glacial and interglacial deposits. This is now admitted by continental palæontologists who find in the character of Pleistocene organic remains abundant proof that the old river-alluvia and cave-accumulations were laid down under changing climatic conditions. Did neither glacial nor interglacial deposits

exist the relics of the Pleistocene flora and fauna met with in extra-glacial regions would yet lead us to the conclusion that after the close of the Pliocene period, extremely cold and very genial climates alternated up to the dawn of the present. Thus during one stage of the Pleistocene "clement winters and cool summers permitted the wide diffusion and intimate association of plants which have now a very different range. Temperate and southern species like the ash, the poplar, the sycamore, the fig-tree, the judas-tree, etc., overspread all the low grounds of France as far north at least as Paris. It was under such conditions that the elephants, rhinoceroses, and hippopotamuses, and the vast herds of temperate cervine and bovine species ranged over Europe, from the shores of the Mediterranean up to the latitude of Yorkshire, and probably even further north still, and from the borders of Asia to the Western Ocean. Despite the presence of numerous fierce carnivora—lions, hyænas, tigers, and others—Europe at that time, with its shady forests, its laurel-margined streams, its broad and deep-flowing rivers, a country in every way suited to the needs of a race of hunters and fishers—must have been no unpleasant habitation for Palæolithic man." But during another stage of the Pleistocene period, the climate of our continent presented the strongest contrast to those genial conditions. At that time "the dwarf birch of the Scottish Highlands, and the Arctic willow, with their northern congeners, grew upon the low grounds of Middle Europe. Arctic animals, such as the musk-sheep and the reindeer lived then, all the year round, in the south of France; the mammoth ranged into Spain and Italy; the glutton descended to the shores of the Mediterranean; the marmot came down to the low grounds at the foot of the Apennines; and the lagomys inhabited the low-lying maritime districts of Corsica and Sardinia. The land- and freshwater molluscs of many Pleistocene deposits tell a similar tale: high alpine, boreal, and hyperborean forms are characteristic of these deposits in Central Europe; even in the southern regions of our continent the shells testify to a former colder and wetter climate. It was during the climax of these conditions that the caves of Aquitaine were occupied by those artistic men, who appear to have delighted in carving and engraving."\* Such, in brief, is the testimony of the Pleistocene flora and fauna of extra-glacial regions. It is from the deposits in those regions,

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\* *Prehistoric Europe*, p. 67.

therefore, that we derive our fullest knowledge of the life of the period. But a comparison of their organic remains with those that occur in the glacial and interglacial deposits of alpine and northern lands shows us that the Pleistocene accumulations of glacial and extra-glacial countries are contemporaneous—for there is not a single life-form obtained from interglacial beds which does not also occur in the deposits of extra-glacial regions. The converse is not true—nor is that to be wondered at, for interglacial deposits have only been sparingly preserved. In regions liable to glaciation such superficial accumulations must frequently have been ploughed up and incorporated with groundmoraine. It was only in the extra-glacial tracts that alluvia of interglacial age were at all likely to be preserved in any abundance. To fully appreciate the climatic conditions of the Pleistocene period, therefore, it is necessary to combine the evidence derived from the glaciated areas with that obtained from the lands that lay beyond the reach of the ice-plough. The one is the complement of the other, and this being so, it is obvious that any attempted explanation of the origin of the glacial period which does not fully realise the importance of the interglacial phase of that period cannot be accepted.

But if the climatic changes of Pleistocene times are the most important phenomena which the geologist, who essays to trace the history of that period is called upon to consider, he cannot ignore the evidence of contemporaneous geographical mutations. These are so generally admitted, however, that it is only necessary here to state the well-known fact that everywhere throughout the maritime tracts of the glaciated lands of Europe and North America, frequent changes in the relative level of land and sea took place during Pleistocene and postglacial times.

I must now very briefly review the evidence bearing on the climatic conditions of postglacial times. And first, let it be noted that the closing stage of the Pleistocene period was one of cold conditions, accompanied in North-Western Europe by partial depression of the land below its present level. This is shown by the late-glacial marine deposits of Central Scotland and the coast-lands of Scandinavia. The historical records of the succeeding postglacial period are furnished chiefly by raised beaches, river- and lake-alluvia, calcareous tufas, and peat-bogs. An examination of these has shown that the climate, at first cold, gradually became less ungenial, so that the Arctic-alpine flora and northern

fauna were eventually supplanted in our latitude by those temperate forms which, as a group, still occupy this region. The amelioration of the climate was accompanied by striking geographical changes, the British Islands becoming united with themselves and the opposite coasts of the continent. The genial character of the climate at this time is shown by the great development of forests, the remains of which occur under our oldest peat-bogs. Not only did trees then grow at greater altitudes in these regions than is at present the case, but forests ranged much further north, and flourished in lands where they cannot now exist. In Orkney and Shetland, in the far north of Norway, and even in the Færøe Islands and in Iceland relics of this old forest-epoch are met with. In connection with these facts reference may be made to the evidence obtained from certain raised beaches on both sides of the N. Atlantic, and from recent dredgings in the intervening sea. The occurrence of isolated colonies of southern molluscs in our northern seas, and the appearance in raised beaches of many forms which are now confined to the waters of more southern latitudes, seem to show that in early post-glacial times the seas of these northern latitudes were warmer than now. And it is quite certain that the southern forms referred to are not the relics of any preglacial or interglacial immigration. They could only have entered our northern seas after the close of the glacial period, and their evidence taken in connection with that furnished by the buried trees of our peat-bogs, leads to the conclusion that a genial climate supervened after the cold of the last glacial epoch and of earliest postglacial times had passed away.

To this genial stage succeeded an epoch of cold humid conditions, accompanied by geographical changes which resulted in the insulation of Britain and Ireland—the sea encroaching to some extent on what are now our maritime regions. The climate was less favourable to the growth of forests, which began to decay and to become buried under wide-spread accumulations of growing peat. At this time glaciers re-appeared in the glens of the Scottish Highlands, and here and there descended to the sea, as in Arran, Sutherland, and Ross. The evidence for these is quite conspicuous, for the moraines are found resting on the surface of postglacial beaches. Thus my friend Mr. L. Hinxman, of the Geological Survey, tells me that at the foot of Glen Thrail well-formed moraines are seen in section reposing on beach-deposits at the distance of about three-quarters of a

mile above the head of Loch Torridon.\* The evidence of this recrudescence of glacial conditions in postglacial times is not confined to Scotland. I believe it will yet be recognised in many other mountain-regions; but already Prof. Penck has detected it in the valleys of the Pyrenees.† Dr. Kerner has also described similar phenomena in the valley of the Stubai near Innsbruck, while Professor Brückner has obtained like evidence in the Salzach region.‡

I have elsewhere traced the history of the succeeding stages of the postglacial period, and brought forward evidence of similar but less strongly-marked climatic changes having followed upon those just referred to, and my conclusions, I may add, have been supported by the independent researches of Professor Blytt in Norway. But these later changes need not be considered here, and I shall leave them out of account in the discussion that follows. It is sufficient for my present purpose to confine attention to the well-proved conclusion that in early postglacial times genial climatic conditions obtained, and that these were followed by cold and humid conditions, during the prevalence of which considerable local glaciers re-appeared in certain mountain-valleys.§

We speak of Pleistocene or glacial and of postglacial periods as if the one were more or less sharply marked off from the other. Of course, that is not the case, and in point of fact it would be for many reasons preferable to include them under some general term. Taken together they form one tolerably well-defined cycle of time, characterised above all by its remarkable climatic changes—by alternations of cold and genial conditions, that were most strongly contrasted in the earlier stages of the period. It is further worthy of note that various oscillations of the sea-level appear to have taken place again and again both in the earlier and later stages of the cycle.

We may now proceed to inquire whether the phenomena

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\* For Scottish postglacial glaciers see J. Geikie: *Scottish Naturalist*, Jan., 1880; *Prehistoric Europe*, pp. 386, 407; Penck: *Deutsche geographische Blätter*, Bd. VI, p. 323; *Verhandlung d. Ges. f. Erdkunde, Berlin*, 1884, Heft i.

† Die Eiszeit in den Pyrenäen: *Mittheil. d. Vereins f. Erdkunde*, Leipzig, 1883.

‡ Eiszeit-studien in den südöstlichen Alpen: *X. Jahresbericht d. geograph. Ges. v. Bern*, 1891.

§ For a full statement of the evidence see *Prehistoric Europe*, Chaps. xvi, xvii.

we have been considering can be accounted for by movements of the earth's crust—a view which has recently received considerable support, more especially in America. I need hardly say that the view in question is not a novelty. Many years ago, while our knowledge of Pleistocene phenomena was somewhat rudimentary, it was usual to infer that glaciation had been induced by elevation of the land. This did not seem an unreasonable conclusion, for above our heads, at a less or greater elevation, according to latitude, an Arctic climate prevails. One could not doubt, therefore, that if a land-surface were only sufficiently uplifted it would reach the snow-line, and become more or less extensively glaciated. But with the increase of our knowledge of Pleistocene and postglacial conditions, such a ready interpretation failed to satisfy, although not a few geologists have continued to defend the "earth-movement hypothesis," as accounting fairly well for the phenomena of the glacial period. By these staunch believers in the adequacy of that view, it has been pointed out that elevation might not only lift lands into the region of eternal snow, but, by converting large areas of the sea-bed into land, would greatly modify the direction of ocean-currents, and thus influence the climate. What might not be expected to happen were the Gulf Stream to be excluded from northern regions? What would be the fate of the temperate latitudes of North America and Europe were that genial ocean-river to be deflected into the Pacific across a submerged Isthmus of Panama? The possibility of such changes having supervened in Pleistocene times has often been present to my mind, but I long ago came to the conclusion that they could not account for the facts. Moreover, I have never been able to meet with any evidence in favour of the postulated "earth-movements." Having carefully studied all that has been advanced of late years in support of the hypothesis in question I find myself more than ever constrained to oppose it, not only because it is grounded on no basis of fact, but because it altogether fails to explain the conditions that obtained in Pleistocene and postglacial times.

There are various forms in which the hypothesis has appeared, and these I shall now consider seriatim, and with such brevity as may be. It has been maintained, for example, that at the advent of the glacial period vast areas of Northern and North-Western Europe, together with enormous regions in the corresponding latitudes of North America,

stood several thousand feet higher than at present. But when we ask what evidence can be adduced to prove this we get no satisfactory reply. We are simply informed that a glacial climate must have resulted from great elevation, and that the latter, therefore, must have taken place at the beginning of the glacial period. Some writers, however, have ventured to give reasons for their faith. Thus Mr. W. Upham, pointing to the evidence of the fiords of North America, and to the fact that drowned river-valleys have been traced outwards across the 100-fathom line of the marginal plateau to depths of over 3,500 feet, maintains that the whole continent north of the Gulf of Mexico stood at the commencement of the glacial period some 3,000 feet at least higher than now. Of course he cites the fiords of Europe as evidence of a similar great upheaval for the northern and north-western regions of our continent. Mr. Upham even favours the notion that during glacial times a land-connection probably existed between North America and Europe, by way of the British Islands, Iceland, and Greenland. When "this uplifting attained its maximum, and brought on the glacial period," he says, "North America and North-Western Europe stood 2,500 to 3,000 feet above their present height."\*

That fiords are simply submerged land-valleys has long been recognised: that they have been formed mainly by the action of running water—just in the same way as the mountain-valleys of Norway and Scotland—has been the belief for many years of most students of physical geology. But it is hard to understand why they should have been cited by Mr. Upham in support of his contention, seeing that their evidence seems to militate strongly against the very hypothesis he strives to maintain. No one acquainted with the physical features and geological structure of Scotland and Norway can doubt that the valleys which terminate in fiords are of great geological antiquity. Their excavation by fluvial action certainly dates back to a period long anterior to the advent of the Ice Age. And a like tale is told by the fiords and drowned valley-troughs of North America, which cannot be referred to so recent a period as post-Tertiary times. Those who are convinced that our continental areas have persisted throughout long æons of geological time, and that rivers frequently have survived great geological revolutions—cutting their way across mountain-elevations as fast as these

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\* *American Geologist*, vi, p. 327.

were uplifted—will readily believe that some of the submarine river-troughs of North America, such as that of the Hudson, may belong even to Secondary times.\* It would be hard to say at what particular date the excavation of the Scottish highland valleys commenced—but it was probably during the later part of the Palæozoic era. The process has doubtless been retarded and accelerated frequently enough, during successive movements of depression and elevation, but it was practically completed before the beginning of Pleistocene times, and that is all that we may trouble about here. Precisely the same conclusion holds good for Norway: and such being the case it is obvious that the origin and age of the fiords have no bearing whatever on the problem of the glacial climate and its cause. In point of fact, the evidence, as already remarked, tells against the “earth-movement hypothesis” for it shows us that, during a period when Europe and North America stood several thousand feet higher, and extended much further seawards, rivers, and not glaciers, were the occupants of our mountain-valleys. It was not until all those valleys had come to assume much the appearance they now present that general glaciation supervened.

We are not without direct evidence, however, as to the geographical conditions that obtained in the ages that immediately preceded the Pleistocene period. The distribution of the Pliocene marine beds of Britain entitles us to assume that at the time of their accumulation our lands did not extend quite so far to the south and east as now. The absence of similar deposits from the coast-lands of North America is supposed to support the view of great continental elevation in pre-glacial times. All it seems to prove, however, is that in Pliocene times the North American continent was not less extensive than it is at present. It is even quite possible that in glacial times pre-existing Pliocene beds may have been ploughed out by the ice, just as seems to have been the case in the north-east of Scotland. But without going so far back as Pliocene times, we meet with evidence almost everywhere throughout the maritime regions of the glaciated areas of Europe and North America, to show that immediately before those tracts became swathed in ice the geographical conditions were much the same as at present. The shelly

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\* Professor Dana inclines to date the erosion of the Hudson Trough so far back as the Jura-Trias period. *American Journ. Science*, xl. (1890), 435.



boulder-clays in various parts of our islands, and the similar occurrence of marine and brackish-water shells in and underneath the "diluvium" of North Germany, etc., proves clearly enough that just before the coming-on of glacial conditions neither Britain nor the present maritime lands of the continent were far removed from the sea. It is true that the buried river-channels of Scotland indicate a preglacial elevation of some 200 or 300 feet above the existing sea-level, but it is quite certain that the Minch, St. George's Channel, the Irish Sea, the North Sea, and the Baltic, were all in existence at the commencement of the glacial period. And we are led to similar conclusions with regard to the geographical conditions of North America at that time, from the occurrence of marine shells in the boulder-clays of Canada and New England.

Thus there appears to be no evidence either direct or indirect in favour of the view that glacial conditions were superinduced by great continental elevation. But it may be argued that even although no evidence can be cited in proof of such elevation, still, if the glacial phenomena can be well explained by its means, we may be justified in admitting it as a working hypothesis. Movements of elevation and depression have frequently taken place—the Pleistocene marine deposits themselves testify to oscillations of the sea-level—and there can be no objection, therefore, to such postulations as are made by the hypothesis under review. All this is readily granted, but I deny that the conditions that obtained in Pleistocene times can be accounted for by elevation and depression. Let us see how the desiderated elevation of northern lands would work. Were North-Western Europe and the corresponding latitudes of North America to be upheaved for 3,000 feet, and a land-passage to obtain between the two continents by way of the Færøe Islands, Iceland, and Greenland, how would the climate be affected? It is obvious enough that under such changed conditions the elevated lands in higher latitudes might well be subjected to more or less extensive glaciation. Norway would become uninhabitable and glaciers might well appear in the mountain-valleys of Scotland. But it may be doubted whether the climate of France and Spain, or the corresponding latitudes of North America would be much affected. For were a land-passage to appear between Britain and Greenland no Arctic current would flow into the North Atlantic, while no portion of the Gulf-stream would be lost in Arctic seas. The North Atlantic

would then form a great gulf round which a warm ocean-current would circulate. The temperature of that sea, therefore, would be raised and the prevailing westerly and south-westerly winds of Europe would be warmer than now. However much such warm moist winds might increase the snow-fall in North Britain and Scandinavia, we cannot suppose they could have much influence in Central and Southern Europe, and in North Africa; and still less could they affect the climate of Asia Minor and the mountainous regions of the far east, in most of which evidence of extensive glaciation occurs. And how, we may ask, could the postulated geographical changes bring about the glaciation of the mountainous tracts on the Pacific sea-board? In fine, we may conclude, that however much the geographical changes referred to might affect North-Western Europe and North-Eastern America, they are wholly insufficient to account for the glacial phenomena of other regions. The continuous research of recent years has shown that the lowering of temperature of glacial times was not limited to the lands which would be affected by any such elevation as that we are considering. A marked and general displacement of climatic zones took place over the whole continent of Europe; and similar changes supervened in North America and Asia. Are we then to suppose that all the lands within the Northern Hemisphere were extensively and contemporaneously upheaved?

We may now consider another form of the earth-movement hypothesis. It has frequently been suggested that our glacial phenomena may have been caused by the submergence of the Isthmus of Panama, and the deflection of the Equatorial Current into the Pacific. But it may be doubted whether a submergence of that Isthmus, unless very extensive indeed, would result in more than a partial escape of Atlantic water into the Pacific Basin. The Counter Current of the Pacific which now strikes against the Isthmus might even sweep into the Caribbean Sea, and join the Equatorial on its way to the Gulf of Mexico. But putting that consideration aside, what evidence have we that the Isthmus of Panama was submerged during the glacial epoch? None whatsoever, it may be replied. It is only a pious opinion. Considerable movements of elevation and depression of the islands in the Caribbean Sea would seem to have taken place at a comparatively recent date, but those movements may quite well belong to Pliocene times. Whether they be of Pliocene or Pleistocene age, however, no one has yet

proved that the Isthmus of Panama was sufficiently submerged, either at the one time or the other, to permit the escape of the Atlantic Equatorial into the Pacific Basin. But let it be supposed that the Isthmus has become so deeply submerged that the Equatorial Current is wholly deflected, and that no Gulf-stream issues through the Straits of Florida to temper the climate of higher latitudes. What would result from such an unhappy change? Can any one, conversant with the geographical distribution of the glacial phenomena, imagine that the conditions of the glacial period could be thus reproduced? Norway might indeed become a second South Greenland, and perennial snow and ice might appear in the mountainous tracts of the British Islands. The climate of Hudson's Bay and the surrounding lands might be experienced in the Baltic and its neighbourhood, and what are now the temperate latitudes of Europe, north of the 50th parallel, would possibly approach Siberia in character. But surely these changes are not comparable to the conditions of the glacial period. The absence of a Gulf-stream would not sensibly affect the climate of South-Eastern Europe and Asia, and could not have the smallest influence on that of the Pacific coast-lands of North America.

Yes, but if we conceive the submergence of the Isthmus of Panama to coincide with great elevation of Northern lands, would not such geographical conditions bring about a glacial epoch comparable to that of Pleistocene times? It is hard to see how they could. No doubt, the climate of all those regions that would be affected by the withdrawal of the Gulf-stream alone would become still more deteriorated if they stood some 3,000 feet higher than now. A vast area in the north-west of Europe would certainly be uninhabitable; but it is for the advocates of the "earth-movement hypothesis" to explain why those inhospitable regions should necessarily be covered with an ice-sheet. For the production of great snow-fields and continental ice-sheets, considerable precipitation, no less than a low temperature, is requisite. Under the conditions we have been imagining, however, precipitation would probably be much less than it is at present. But to whatever extent North-West Europe might be glaciated, it is obvious that the geographical revolutions referred to could have little influence on the climate of South-Eastern Europe, not to mention Central and Eastern Asia. Nor could they possibly influence the climate of the Pacific coast-lands of North America. And yet, as is well-

known, the climate of all those regions was more or less profoundly affected during the glacial period. To account for the wide-spread evidences of glaciation by means of elevation it would therefore seem necessary to infer that all the affected areas were in Pleistocene times uplifted *en masse* into the Arctic zone that stretches above our heads. Now it seems easier to believe that the snow-line was lowered by several thousand feet than that the continents were elevated to the same extent. Glaciation, as we have seen, was developed in the same directions and over the same areas as we should expect it to be were the snow-line to be generally depressed. To put it in another way, were the snow-line by some means or other to be lowered over Europe, Asia, and North America, then, with sufficient precipitation, great ice-fields and glaciers would re-appear in the very regions which they visited during Pleistocene times. Neither elevation nor depression of the land would be required to bring about such a result. Certain advocates of the earth-movement hypothesis, however, do not maintain that all the glaciated areas were uplifted at one and the same time. The glaciation of the Alps, they think, may have taken place earlier or later than that of North-Western Europe, while the ice-period of the Rocky Mountains may not have coincided with that of Eastern North America. It is not impossible, they suppose, that the glaciation of the Himalaya may have been caused by an uplifting of that great chain, quite independent of similar earth-movements in other places. It can be demonstrated, however, that the glaciation of the Alps and of Northern Europe were contemporaneous and the facts go far to prove that the glaciers of the Rocky Mountains and the inland-ice of North-East America likewise co-existed. At all events all the old glacial accumulations of our hemisphere are of Pleistocene age, and it is for the advocates of the hypothesis under review to prove that they are not really contemporaneous. Their doubts on the subject probably arise from the simple fact that they are well aware how highly improbable or even impossible it is that all those glaciated lands could have been pushed up within the snow-line at one and the same time.

Let me, however, advance to another objection. We know that the glacial period was interrupted by at least one interglacial epoch of temperate and even genial conditions. Two glacial epochs with one protracted interglacial epoch are now generally admitted. How do the supporters of the earth-

movement hypothesis explain this remarkable succession of climatic changes? Their views as to the cause of glacial conditions we have considered. If we can believe that the glacial phenomena were due to elevation of the land, then we need have no difficulty in understanding how glacial conditions would disappear when the continents again subsided to a lower level. Not only did North America and Europe lose all their early glacial elevation, but by a lucky coincidence the Isthmus of Panama re-appeared, and the Gulf-stream resumed its beneficent course into the North Atlantic. This we are to suppose was the cause of the interglacial epoch. But I would point out that the geographical conditions which are thus inferred to have brought about the disappearance of the glacial climate, and to have ushered in the interglacial epoch are precisely those that now obtain—and, nevertheless, we are not yet in the enjoyment of a climate like that of interglacial times. The strangely equable conditions that permitted the development of the remarkable Pleistocene flora and fauna are not experienced in the Europe of our day. And what about the second glacial epoch? Are we to suppose that once more the lands were greatly uplifted, and that convenient Isthmus of Panama again depressed? Did the Alps, the Pyrenees, and the Plateau of Central France—in all of which we have distinct evidence of at least two glacial epochs—did these heights, one may ask, rise up to bring about their earlier glaciation, sink down again to induce interglacial conditions, and once more become uplifted at the succeeding cold epoch, to subside eventually in order to cause a final retreat of their glaciers?

But the climatic changes to be accounted for were in all probability more numerous and complex than those just referred to. Competent observers have adduced unmistakable evidence of three epochs of glaciation in the alpine lands of Europe. And we are not without distinct hints that similar changes have taken place in Northern and North-Western Europe. Nor in this connection can we ignore the evidence of several interglacial episodes which Mr. Chamberlin and others have detected in the glaciated tracts of North America. Even this is not all, for the upholders of the earth-movement hypothesis have still further to account for the climatic oscillations of postglacial times. If it be hard enough to allow the possibility of one great movement of elevation having affected so enormous an area of our

hemisphere, if we find it extremely difficult to believe either that one such wide-spread movement, or that a multitude of local movements, each more or less independent of the other, could have lifted the glaciated regions successively within reach of the snow-line—we shall yet find it impossible to admit that such remarkable upheavals could be repeated again and again.

We seem driven to conclude, therefore, that the earth-movement hypothesis fails to explain the phenomena of Pleistocene times. One cannot deny, indeed, that glaciation might be induced locally by elevation of the land. It is quite conceivable that mountains now below the limits of perennial snow might come to be ridged up to such an extent as to be capable of sustaining snow-fields and glaciers. And such local movements may possibly have happened here and there during the long-continued Pleistocene period. But the glacial phenomena of that period are on much too grand a scale, and far too widely distributed to be accounted for in that way. And if the occurrence of even one glacial epoch cannot be thus explained, we may leave the supporters of the earth-movement hypothesis to show us what light is thrown by their urim and thummim on the origin of succeeding interglacial and glacial climates.

While we have no evidence of wide-spread elevation having coincided with glacial conditions, proofs of subsidence are almost everywhere associated with the glacial phenomena of the maritime districts of North America and Europe. Raised beaches and marine deposits are traced on the coasts of North America, from an elevation of 50 feet or so in Southern New England up to 75–100 ft. near Boston; of 200 ft. or thereabout in Maine; of 520 ft. at Montreal; of 1,500 ft. in Labrador; and of 1,000–2,000 ft. in Arctic regions. None of the raised beaches of glacial age met with in Europe reaches such an elevation as these last—the highest being met with in Norway at 580 ft. or thereabout. Marine shells occur in the glacial series of Scotland at a height of 500 ft., but the highest raised beach of the period does not exceed 100 ft. in elevation. It is doubtful if all those indications of submergence can be assigned to one and the same stage of the glacial period. So far as regards Scotland they certainly belong to separate stages. Thus the shell-beds at 500 ft. are of interglacial age—they rest upon and are covered by boulder-clay, while the 100 ft. beach pertains to the close of the last glacial epoch. But putting such considerations aside, it

must be admitted that considerable submergence of the land took place in glacial times. The advocates of the earth-movement hypothesis naturally attach much importance to this evidence. If it can be shown that the crust of the earth has been depressed in northern regions to depths of 1,500 to 2,000 ft. it is less hard to believe that at other times it may have been uplifted to as great an extent above its present level. We have seen, therefore, that they do not hesitate to infer that, in early glacial times, North America and the north-western regions of Europe, if not a still larger area of that continent, stood some 3,000 ft. or so higher, and that those regions subsequently became submerged to the depths indicated by the raised beaches. The amount of subsidence in New England must therefore have amounted, according to this view, to more than 3,000 ft., say 3,200 ft., in Canada to 3,500 ft., in Labrador and the far north to 4,500 or 5,000 ft. In North-West Europe likewise the earth-movement must have ranged between 3,500 and 3,600 ft. Fortunately for mankind, our continents, when re-elevation ensued, were not uplifted to the great height which they are supposed to have attained at the beginning of the glacial period.

The remarkable association of evidence of glaciation with proofs of submergence has long been noted by geologists, and various attempts have been made to show that the drowning of the lands may have been caused by the great ice-sheets. Thus Croll and others have maintained that vast accumulations of ice in northern latitudes would tend to displace the earth's centre of gravity, and thus cause the sea to rise on the glaciated hemisphere. This is probably a *vera causa*, but it is very doubtful if it can account for the extreme submergence indicated by the more elevated raised beaches. Again, it has been supposed that the attractive influence of the great ice-sheets would bring about a deformation of the sea-level, but, as Dr. Drygalski has shown, this cause is quite insufficient to account for the amount of submergence which is known to have taken place. But the view which has met with most acceptance is that advocated by Mr. Jamieson, who thinks that the earth's crust was simply pressed down under the weight of overlying ice-masses. Even those geologists who most distrust Sir William Thomson's conclusion that the earth is substantially solid may well hesitate before they admit the feasibility of Mr. Jamieson's hypothesis. Were the crust so readily deformed as he supposes, it is hard to understand how great mountain-chains can be supported

above the surrounding low grounds, or how, indeed, continents can rise above abysmal oceanic depressions. Professor George Darwin has lately shown that the prominent inequalities of the earth's surface could not be sustained unless the crust be as rigid as granite for a depth of 1,000 miles. "If the earth," he remarks, "be solid throughout, then at 1,000 miles from the surface the material must be as solid as granite. If it be fluid or gaseous inside, and the crust 1,000 miles thick, that crust must be stronger than granite, and if only 200 or 300 miles in thickness much stronger than granite. This conclusion is obviously strongly confirmatory of Sir William Thomson's view that the earth is solid throughout." Now if the crust have anything like the solidity attributed to it by Professor Darwin—if there be no liquid stratum underlying a relatively thin crust, Mr. Jamieson's hypothesis cannot be maintained. The connection between glaciation and submergence, if it be not a mere coincidence, still remains, therefore, to be explained. Recently, however, a new interpretation of the facts, which may possibly approve itself to physicists, has been advanced by Dr. Drygalski. This author is of opinion that a thick ice-sheet, by reducing the temperature of the underlying crust, would cause this to contract, and so bring about subsidence. The resulting depression of the surface would continue so long as the ice-sheet endured, but after it had disappeared free radiation of earth-heat would be resumed, the depressed isogeotherms would rise, and a general warming of the upper portion of the lithosphere would take place. But the space occupied by the depressed section, owing to the spheroidal form of the earth, would be smaller than that which it filled before sinking had commenced, and consequently, when the ice vanished, expansion of the crust would follow, and the land-surface would then rise again. But it might not be able to attain its former elevation, and it is quite conceivable that the amount of elevation might vary throughout the newly risen area. If this explanation should commend itself to physicists it would be welcomed by geologists, for it is more readily reconcilable with the facts than any other which has yet been advanced. Especially would it throw some light on that irregular deformation to which the region of the great lakes of North America seems to have been subjected in glacial times.

The advocates of the earth-movement hypothesis have gladly hailed Mr. Jamieson's view as being in perfect harmony with theirs. They are under the impression that it gets them



out of a difficulty. Having postulated an amount of elevation for which no evidence can be cited, but which they conceive necessary for the generation of great ice-sheets and glaciers, they next attribute the subsidence of the highly elevated continents to the weight of those ice-masses. The ice-sheets, in fact, are supposed to have brought about their own destruction. Thus the responsibility for the various earth-movements required by the hypothesis is partly shifted from Pluto's shoulders. We first have great continental uplifts induced by subterranean action; next, the lands sink down again under their load of snow and ice. Thus reduced in elevation they cease to favour the accumulation of snow and ice, whereupon the *mers de glace* melt away, and the overburdened crust, relieved of its load, again rises. It seems all very simple and plausible, but let us see what it involves. The thickness attained by the European ice-sheet in the basin of the North Sea probably did not exceed 3,500 ft. or 4,000 ft.; and if we take 3,000 feet as its average thickness throughout the whole area covered by it we shall certainly be over the mark. Now let it be remembered that at the beginning of the Ice Age Europe is supposed to have stood some 3,000 feet higher than at present, and to have subsequently become depressed for some 500 or 600 feet below the existing sea-level. In other words, we are asked to believe that an ice-sheet, not 3,000 feet thick, succeeded in pressing down the crust of the earth to the extent of 3,500 or 3,600 feet! The North American ice-sheet was considerably greater than ours, but even allowing it to have been three times thicker, we shall yet hardly be persuaded that it could possibly depress the crust for 3,000 to 5,000 feet. We may safely conclude, then, that if the raised beaches and marine beds of the Atlantic borders owe their origin to submergence caused by the weight of ice-sheets, the continents could not have been so highly elevated at the advent of glacial conditions. On the other hand, if we accept the hypothesis of former great elevation of the land, then we must infer that the subsidence indicated by the raised beaches cannot have resulted from the pressure of the ice-sheets.

There are many other objections to the earth-movement hypothesis which the limits of this paper forbid me entering upon. But those already indicated may suffice to show that the hypothesis is not only baseless but wholly fails to explain the facts, most of which, indeed, tell strongly against it. It accounts neither for the wide-spread phenomena of the Ice

Age, nor for the remarkable climatic conditions of interglacial times. Finally, it throws no light whatsoever on the fact that cold and genial climates alternated during the Pleistocene and postglacial periods.

The PRESIDENT.—I will now ask you to accord your thanks to Professor Geikie for his Paper, and also to Mr. Chisholm, who has so kindly read it in the Author's unavoidable absence. (Applause.) I now invite remarks on the Paper, and am glad to see that many geologists are present.

Professor E. HULL, LL.D., F.R.S.—As I come within the category of geologists, and as this is a subject I have had before me for a good many years, especially in my official capacity on the Geological Survey, I am very pleased to take part in this discussion. We are certainly favoured this evening in having an elaborate Paper on the subject of which the Author may be considered the chief exponent amongst British geologists. Professor James Geikie has made the subject of glaciation his own, to a great extent, by the publication of his well-known work *The Great Ice Age*, and this Paper contains so much that is interesting—and that calls for discussion—a good deal of which I acknowledge was previously unknown to me, that I listened to it with great interest. He leaves us, however, very much in the position, as regards the question of the origin of the Great Ice Age, in which we were before the Paper was read. He combats a view, or an interpretation, of that cause which we must not forget was originated, or at any rate elaborately maintained, by so distinguished an observer and interpreter of natural phenomena as Sir Chas. Lyell; and of course when the Author combats a view which has been elaborately defended and maintained by so great an authority on Physical Geology and Geography of past times, as Lyell, we must feel that he is treading on very dangerous ground; and for my part I fully expected that if my old friend and brother colleague, Professor Geikie, endeavoured in this Paper to demolish what he calls "the Earth-movement hypothesis," he would have presented us with

something in its place which would have given us a more clear and adequate idea of the causes which brought about this remarkable epoch in the earth's history which immediately preceded, or was partly contemporaneous with, the appearance of man. The Author, however, has not done so, as he may have considered that this was not within the scope of his Essay. He endeavours to show that the Earth-movement hypothesis is untenable, but he does not give us anything in its place. The very distinguished physicist and astronomer, Sir Robert Ball, has within recent times given us from his (an astronomical) point of view, an hypothesis to account for this remarkable period, and, I supposed or hoped, that perhaps Professor Geikie would have discussed Sir Robert Ball's hypothesis. Again, we also know that there is Croll's hypothesis, also of an astronomical character, and as Dr. Croll was a fellow-countryman of Professor Geikie's, I had also supposed that he was prepared either to maintain or to argue against Dr. Croll's hypothesis. Under these circumstances I shall not, on the present occasion, attempt to offer to the Institute any hypothesis: it is not my province to do so, but I would point out one or two arguments in defence of the Earth-movement hypothesis.

I do not understand why it is that the Author supposes 3,000 feet as the necessary elevation of the earth's surface. He says, in order to bring about the glacial condition of the Great Ice Age, it was necessary that the Northern hemisphere should have been elevated 3,000 feet. It seems to me that this is carrying your demand for elevation very much beyond what is at all necessary. For my part, I think it could be very easily shown that an elevation of 1,000 feet would probably cause such a change in the climatic conditions of the Northern hemisphere that a very large amount of glaciation would take place amongst the mountainous regions of Europe and the British Islands, which would also have a very material effect on the climate of the adjoining lands to the southward. I do not see, therefore, that it is necessary to demand such an enormous general elevation as that of 3,000 feet.

Then, as to the movement of the earth's crust. We have in the British Islands the most clear evidence that the inter-glacial epoch, of which Professor Geikie speaks, was contemporaneous with a depression of the land surface, amounting to at least 1,300 feet, because beds of sand and gravel with marine shells have been found, both on the mountains of Ireland and of North Wales, at

an elevation of 1,300 feet above the present level of the sea, and in several other places at a lower level; and those shells are certainly referable to the inter-glacial stage. So that this one fact shows that the earth's crust is capable, in comparatively recent periods (speaking geologically of course), of undergoing considerable alternations of elevation. I will not go farther into this topic because I fear, if I did, I should occupy too long a time; but I should like to refer to one effect which the glacial epoch of the Northern hemisphere had upon the regions which Professor Geikie calls extra-glacial. He, in this Paper, has very clearly defined what were the limits of these great ice sheets in Europe and adjoining countries; but the point I wish to refer to is to show the effect which the glaciation of Northern Europe must have had on the regions immediately to the south of the great ice sheets. Now when travellers explore the central and northern parts of Africa, Arabia Petræa, the Great Arabian Desert and Palestine, they are struck by the fact that those regions which are extra-glacial, are traversed by magnificent valleys which were once, undoubtedly, the channels of considerable rivers. Along the bottoms of those valleys we have alluvial strata in great terraces, extending from side to side—perhaps two or three miles in width, with well-defined banks on either side; yet those valleys are now absolutely dry, or almost dry. The rivers are dispersed; and we ask ourselves—was there a time when these great river valleys, which, for example, traverse the Sinaitic Peninsula and Southern Palestine for many miles, were filled with streams? No geologist can hesitate as to the answer to that question. Every geologist will say, at once, "Yes, there must have been rivers occupying those channels." The interesting point connected with the subject is that we have to refer to this glacial period as affording us an explanation of the mode of formation of these great river valleys. We can quite understand that if the northern half of Europe and the Lebanon were covered with perennial snows and glaciers, the climate of the regions to the south of them would be very different to what it is at present. They would, in fact, have a climate similar to that of the British Islands at the present day. Instead of being absolutely rainless, or nearly so, they would have their proportionate rainfall, as is the case with our own country. Therefore, we have in the glacial period a very interesting explanation, as it seems to me, of the occurrence of these valleys

which are now dry, but which were formerly filled with streams. That is the special inference I wish to draw from this Paper; which exhibits a wide knowledge of the subject it treats of.

The Rev. W. B. GALLOWAY, M.A., urged the greater probability, in his opinion, of the older views of Cuvier and Buckland, which accounted for the phenomena in question by a universal Deluge. He alluded to the mammoth found in the River Lena, as making against the long periods of time required by the glacial theory, and suggested that the deluge had been caused by a change in the earth's axis, mentioning that this appeared to have been the view of the great astronomer Halley, who read a paper on the subject in 1694, which appeared in the Transactions in 1724.

Professor J. LOGAN LOBLEY, F.G.S.—The Paper, so far as it goes, is noteworthy for its elaboration and clearness of expression and for the weighty argument that it brings against the Earth-movement theory to account for the climate of the Glacial Period; but I could have wished it had advanced some hypothesis to explain the cause of that very remarkable epoch. An elevation of 1,000 feet would, as Professor Hull suggests, doubtless cause a great alteration of climate, and might produce such masses of ice and snow as would account for much of the phenomena we observe; but I would point out that the depression which has been deduced from the presence at high levels of recent shells on Moel Tryfaen and other places, has been disputed. At a recent meeting of the Geological Society a paper was read, in which the occurrence of these shells was attributed to the elevating action of ice, so that we can scarcely accept the great depression and elevation in question as absolutely proved. Too much, I think, is made of the Gulf Stream and its effects on the climate of North-West Europe. I attribute our mild climate not so much to the action of the Gulf Stream as to the general flow from the south of warm water through the North Atlantic, and to the south-west winds that come over those warmer waters. To Professor James Geikie is due great credit for having investigated the phenomena produced by the Glacial Period, but it is evident that still further observations are required, before we can come to any satisfactory conclusion on this interesting subject.

Mr. G. G. CHISHOLM.—Professor Logan Lobley mentions one possible way of accounting for beds of mollusca at considerable elevation, and he implies that the mere fact of those beds of

mollusca existing at that elevation is no proof that the land was depressed to such an extent as to allow of their being deposited there, and refers to a suggestion that they may have been forced up by other action. I should say that much would depend upon the precise position in which the beds were found, and the indications afforded by the surrounding circumstances, as to the manner in which deposits were made, and I feel no doubt that Professor Geikie would hardly have spoken of such beds of mollusca being deposited by the sea instead of being pushed up by the ice, if he had not thought that the evidence was sufficient for their being deposited in that manner. As to the idea that mollusca or small boulders can be pushed up to considerable elevations by the means of ice, I have myself heard Professor Geikie point out instances of that kind, and he has cited examples of small boulders that must have travelled from all parts of the North of Scotland down the valleys and up the mountains, and so forth, under the action of ice; so I do not think that Professor Lobley's supposition of the possibility of mollusca beds being found in the position in which they are found, would have been absent from Professor Geikie's mind; only in regard to the particular mollusca beds to which he has referred I suppose his impression was that the evidence was not favourable to the idea of that mode of deposition. I will make one more remark as to Professor Lobley's observation concerning Professor Geikie's use of the term "Gulf Stream" as applied to the agency which undoubtedly moderates the climate of Western Europe. It is safe to say that in using that expression, Professor Geikie was quite aware of the fact that as a distinct marine river, the Gulf Stream can hardly be said to reach the shores of Western Europe at all. It is well known that as a marine river the Gulf Stream cannot be detected further north than between the latitudes  $40^{\circ}$  and  $50^{\circ}$ , but for all that the effect of the Gulf Stream on Western Europe must be very considerable indeed, for the great body of heated water which leaves the Gulf of Mexico by the Straits of Florida, and then flows as a distinct marine river into a considerably higher latitude, must modify the temperature of the surface or drift currents which succeed the Gulf Stream proper in still more northerly seas, and of the winds that blow over those seas and carry their temperature to more northern regions.

The Meeting was then adjourned.

## COMMUNICATION

From Mr. WARREN UPHAM ; Assistant, United States Geological Survey.

The very important Paper by Professor Geikie I have read with the greatest interest, since his conclusions as to the probable causes of the accumulation of the ice-sheets of the Glacial period differ so widely from the views which from much observation and study I have come to hold with a good degree of confidence. He has devoted this Paper to the exposition of the difficulties and objections which beset my explanation of ice-accumulation as due to climatic conditions, chiefly the prevalence of snowfall during nearly all the year, attendant upon great elevation of the regions that became glaciated.

Most of these difficulties I cheerfully acknowledge, and yet think that the evidences of such Pleistocene elevation of North America and North-Western Europe are decisive. The researches of N. H. Winchell, McGee, Chamberlin, Salisbury, Leverett, and myself, in the United States indicate the divisibility of the Glacial period into at least two epochs of glaciation, divided by a long interglacial epoch, when the North American ice-sheet may have been entirely melted away. We thus agree with Professor Geikie, the late Dr. Croll, Wahnschaffe, Penck, De Geer, and other European glacialists, who find similar proofs of two or more glacial epochs, separated by intervals of mild climate. This repetition of the conditions producing ice-accumulation is justly insisted on by Professor Geikie as the strongest objection that can be urged against its explanation by high uplifts of the land. The relationship, however, which I suppose to have existed between the earth's contraction and the processes of mountain-building, whereby the earth-movements producing high altitude and glaciation were induced, may well have caused ice-sheets to be accumulated successively upon various parts of the earth's surface, not necessarily nor indeed probably existing at the same time upon all drift-bearing countries; and after an interglacial epoch, the same conditions might, as I have shown, be renewed upon any given area, as in North America and North-Western Europe. The supposed difficulties on account of widely distributed areas of glaciation and repetitions of ice-accumulation are duly considered in my *Probable Causes of Glaciation*, published

as an appendix in Professor G. Frederick Wright's *Ice Age in North America*. I may also mention, as treating this subject, in addition to the paper in the *American Geologist* cited by Professor Geikie, my articles in the *American Journal of Science*, III, vol. xli, pp. 33-52, Jan., 1891, and *Popular Science Monthly*, vol. xxxix, pp. 665-678, Sept., 1891.

But if the supposed interglacial beds are more properly to be referred to oscillations of the ice-front during a single glacial epoch, as is held by Wright, Lamplugh, Falsan, and others, there would be no such repetition of uplifting of the glaciated regions.

The vertical extent of the uplift needed to reinstate the Glacial period in Europe and North America, would be probably 3,000 to 5,000 feet, as Prof. T. G. Bonney has shown that an average lowering of the temperature of Europe by 18° Fahr. and of the northern part of North America by 13° would suffice. Though Professor Geikie is inclined to relegate the time of land elevation shown by the fjords to some epoch long antecedent to the Ice age, I feel sure that they can be proved to be of Pleistocene age. In North America submerged river valleys both on the Atlantic and Pacific Coasts extend to the depth of 3,000 feet beneath the present sea level; and the Sogne fjord, the longest in Norway, has, according to Mr. T. F. Jamieson (*Geol. Mag.*, III, vol. viii, p. 390, Sept., 1891), a depth of 4,080 feet. These glaciated countries stood lately at least 3,000 to 4,000 feet above their present height. This very remarkable condition and the equally extraordinary accumulation of ice-sheets belong to the same Pleistocene period, and I believe that they were causally related, the high altitude being the cause of the ice-sheets.

That the earth-movements which thus uplifted North America and North-Western Europe, permitting streams to erode the fjords and now submerged valleys, occupied the closing part of the Pliocene period and culminated in the early part of the Pleistocene or Glacial period, has been discussed and apparently demonstrated by Prof. J. W. Spencer, Prof. Joseph Le Conte, and the present writer. (*Bulletin of the Geological Society of America*, vol. i, 1890, pp. 65-70, 563-7; vol. ii, 1891, pp. 323-330, 465-476. Le Conte's *Elements of Geology*, new edition, 1891, pp. 589-594. *Geol. Magazine*, III, vol. vii, 1890, pp. 208-213, 492-7; vol. viii, pp. 92, 262-272, 330.)

In Europe, there is no better advocate of great earth-movements



during Pleistocene or Quaternary time than Professor Geikie himself, who, with Ramsay, has proved that the earth's crust at the Strait of Gibraltar, since the end of the Tertiary era, has been repeatedly uplifted much above its present height, allowing African animals to cross on dry land into Europe (*Quarterly Journal of the Geological Society, London*, vol. xxxiv, 1878, pp. 505-541), and who also believes that a land connection existed during the Glacial period from Britain to the Færøe Islands, Iceland, and Greenland (*Prehistoric Europe*, 1881, pp. 518-522, and 568, with Plate E). In Professor Geikie's admirable memoir on the geology of the Færøe Islands (*Trans. Roy. Soc. Edinb.*, vol. xxx, 1882, pp. 217-269), he shows that a vast amount of erosion has been effected there, and in like manner upon other lands bordering the North Atlantic, since the Miocene period. In comparison with the late Tertiary erosion so impressively exhibited, it is easy to accept the view that the deep but narrow Scandinavian fjords belong to a geologically short stage of great uplift during the late Pliocene and early Pleistocene epochs. The rivers continued to flow along the bottoms of these fjords until the increasing elevation of the land, as I think, brought on the ice-sheets, beneath which the land sank somewhat below its present height.

It is true that the duplication of glacial epochs accords beautifully with Croll's astronomic theory, which for several years met with general acceptance in America as well as in Europe. But the recency of the latest glaciation on both continents, which has been well stated by Wright, N. H. Winchell, Andrews, Gilbert, and Russell in American publications, and by Mackintosh, Southall, and others in the *Journal of Transactions of the Victoria Institute* (vol. xiii, and especially vol. xix, pp. 73-92), showing that the length of the postglacial epoch has been no more than 6,000 to 10,000 years, is inconsistent with the reference of that glaciation to astronomic conditions which ended 80,000 years ago.

Before receiving this Paper by Professor Geikie, I had it in mind to send, for some meeting of the Victoria Institute next year, a review of the principal theories which have been held to account for the climate of the Ice age; and in that Paper I hope to present more fully the grounds for my view as here briefly noted, and the difficulties which seem to me to forbid the acceptance of the other two theories which Evans and Croll proposed nearly thirty years ago.

## THE AUTHOR'S REPLY.

I am sorry that my old friend Professor Hull is disappointed because, in trying to knock the "Earth-movement hypothesis" on the head, I have not presented him with some other explanation of the origin or cause of the glacial conditions of Pleistocene times. But I would remind him that the critic who essays to condemn a work of fiction is, fortunately for himself, not expected to produce another in its place. From the remarks made by Professor Hull, Professor Lobley, and Mr. Upham, it might be inferred that I do not believe in movements of elevation and depression. This is certainly not the case: all that I deny is that we have any evidence to show that the former excessive glacial conditions of Europe and North America were caused by great elevation of the land. Formerly I used to believe with most geologists that the Moel Tryfaen deposits were evidence of a depression of the land to the extent of 1,200 feet or thereabout, but after visiting that region some years ago, I felt convinced that the accumulations in question had been dragged into their present position by the old ice-sheet—the materials having of course been rearranged by the action of sub-glacial water.

Mr. Upham merely reiterates his belief in the Pleistocene age of the fiord-valleys of North-west Europe, remarking that it has apparently been demonstrated by himself and other American writers that the excavation of those valleys "occupied the closing part of the Pliocene period and culminated in the early part of the Pleistocene or Glacial period." This will be news to European geologists who have long thought that our fiord-valleys (in Norway and Scotland) are amongst the oldest valleys of erosion in Europe. Yet if Mr. Upham's contention were admitted, we should also have to admit that the fiord-valleys of North-west Europe are of more recent origin than the great lake-valleys of the Alps! Mr. Upham strangely does not see that if the fiord-valleys are simply partly-submerged land-valleys which owe their excavation to fluvial action, their age and origin can have no bearing on the

question I have been discussing. The valleys were hollowed out by running water when the land stood 3,000 to 4,000 feet higher than now. Their excavation must necessarily have occupied a prodigious time, yet throughout that protracted period, rivers and not glaciers were their occupants. Clearly, then, if the fiord-valleys were excavated in late Pliocene and early Pleistocene times the land had then all the elevation required by Mr. Upham for the production of great ice-sheets, and yet no general glaciation took place until the hollowing out of the valleys had been practically completed. All that the glaciers have done has been to grind out hollows in the bottoms of the valleys, and to modify the general contour of the ground.

## LETTERS RECEIVED.

Major-General A. W. DRAYSON, F.R.A.S., writes:—

The geological portion of Professor Geikie's Paper in regard to the Glacial Period shows such vast research and attention to detail, that I cannot presume to offer any remarks thereon. When, however, I find that he has devoted some three pages to demolishing what he terms the "Earth-movement hypothesis" and does not even refer to any other cause, I venture to offer some remarks: more especially am I disposed to offer these remarks, because a writer on the Ice Age in the *Edinburgh Review* for April, 1892, after pointing out that the assumption of the Earth being pulled away from the Sun, and thus causing the Ice Age, lacks the essential element of scientific truth, despondingly remarks that "there is nothing else to fall back upon."

Instead of there being nothing else to fall back upon, other than "assumptions" and mere hypothesis, there is a cause for the Ice Age, which has merely to be examined by competent geometers, and the proof will be manifest that it is unanswerable. I make this statement, not on my own conclusions only, but because a considerable number of able geometers have carefully tested every detail and have told me that the case is proved.

As briefly as possible I will explain what this cause is.

More than 300 years ago the three principal movements of the earth were said to be, a daily rotation, an annual revolution round the sun, and a conical movement of the axis of daily rotation round the Pole of the Ecliptic *as a centre*.

The reason why the earth's axis was supposed to trace a circle round the Pole of the Ecliptic *as a centre* was, because the observations of 300 years ago were not sufficiently accurate to reveal the fact that the Pole of the heavens (which is that point in the heavens to which the axis points) was continually decreasing its distance from the Pole of the Ecliptic, the imagined centre of the circle.

About 150 years ago it became generally admitted that the Pole of the heavens in its circular course, slowly decreased its distance from the Pole of the Ecliptic, and had so decreased its distance during 2,000 years at least.

Although this decrease in distance of the two Poles was a recognized fact, writers on astronomy continued to state that the one pole traced a circle round the other pole *as a centre*.

More than 30 years ago the above contradiction was brought to my notice, and I devoted ten years to the investigation of the problem, with the following results.

First, that the movement hitherto defined as a conical motion of

the earth's axis was in reality a slow second rotation of the earth, which caused the two half axes of the earth to describe cones.

Second, that the centre of the circle which the earth's axis traced was 6 degrees from the Pole of the Ecliptic, and the radius of this circle was  $29^{\circ} 25' 47''$ , and that the decrease in distance of the two poles was due to this position of the centre of the circle.

From a knowledge of these facts I was able to arrive by calculation at results hitherto imagined to be impossible in astronomy, and the proof that the radius and position of the centre of the circle were as above stated was undeniable.

Third. From the fact that the centre of the circle traced by the earth's axis was 6 degrees from the Pole of the Ecliptic, it followed that during the tracing of this circle there would be a variation of 12 degrees in the distance of the two Poles, and a corresponding variation of 12 degrees in the extent of the Arctic circles and tropics.

From a knowledge of this curve, I was able to state more than 20 years ago that at about 3000 B.C. the Arctic circles and tropics extended about 2 degrees more than at present. That at about 5600 B.C. they extended about  $6\frac{1}{2}$  degrees more than at present. That at about 13500 B.C. they extended nearly 12 degrees more than at present, at which date the Last Glacial Period was at its height. At about 21500 B.C. the Arctic circles extended about  $6\frac{1}{2}$  degrees more than at present, and at about 24000 B.C. about 2 degrees more.

Hence the Last Glacial Period terminated not longer than about 6,000 years ago, and lasted not longer than about 18,000 years.

These dates were 20 years ago so utterly at variance with geological theories, that my proofs would not even be looked at. Within the last year or two, however, geologists from geological evidence have come to exactly the same dates that geometrical astronomy proved 20 years ago.

As the movement herein briefly described is proved by geometry, has been tested and found accurate by numerous competent examiners, and as it proves that 15,000 years ago the Arctic circle reached to 54 degrees latitude, and hence explains the main facts of the Ice Age, and also gives its date, it appears remarkable that eminent geologists should despondingly state that besides those vague speculations which they have demolished, there is nothing else in astronomy to fall back upon.

As remarked by Professor Geikie the assumed elevation and depression of the earth's surface is not only a mere speculation but fails to explain the facts. When geologists examine the movement of the earth herein described they will find an ample explanation of that which they require.

Mr. H. P. MALET writes:—

Whilst thanking Professor Geikie for his interesting Paper may I offer a few brief remarks on the Glacial theory.

We are told that "it is no longer disputed that in Pleistocene times vast sheets of ice. . . covered broad areas in Europe and America." The Pleistocene time is at the head of the Tertiary system, but no date is as yet fixed for the group. James Croll tells us in *Climate and Time* that the Glacial epoch began about 240,000 and ended about 80,000 years ago. Mr. Smith in his *Great Ice Age of North America*, gives about 15,000 or 20,000 years ago for the end of the frozen time. Professor James Geikie kindly sent me his papers on the *Evolution of Climate*, including some very suggestive maps of the varied condition of this Earth:—No. 1 gives the Palæozoic epoch, when the sea ran up through Central America, Europe, and Asia; No. 2 gives the Mesozoic condition, when the same highways were open. It is an accepted fact that the light warm water of the tropics runs up to replace the cold sinking water of the Arctic region. We know that this warm current gives warmth directly and indirectly to the neighbouring regions, therefore no Glacial Period existed in Europe or America at that time.

No. 3 gives the same regions in the Tertiary system; the American Channel is closed, but the Atlantic and the European channels are open. As the Pleistocene group is in this system, and as warm water still found its way through Europe, it is difficult to suppose that glacial times existed in the Temperate Zone of Europe. The maps seem to represent a very true geographical condition at each period—without date. We have no charts giving altitude in those old days, but there can be no doubt how the Highlands have at all times contributed to the filling up the lowlands by their denuded particles.

The Address points out several "salient facts" to reckon with before the glacial climate can be securely accepted. In addition to these I found in India the same actions going on by water forces as are attributed to ice by the glacial theorists. I found old moraines in the midst of plains in the Taptee and Beenea Valleys. These were left by river water-falls: rocks fall on the water and on the ice, they are carried as far as the forces can carry them, and are left to mark the spots where the moving power left them.

The subject has been much complicated by clever theories, but when we return to nature and trace the changes of climate as I did in *The Times* of February, 1891, I see no reason to give a Glacial Period to Europe in the Pleistocene group, when the geographical conditions were approaching their present state. Professor James Geikie told us in his *Evolution of Climate*, that "Geological climate has been determined chiefly by geographical conditions—therefore if Europe and America were covered by ice sheets in the Tertiary system, why were they removed?"

Mr. JOSEPH JOHN MURPHY writes:—

I have read Professor Geikie's Paper on the Glacial Period with

interest, and yet with disappointment at seeing so little new light thrown on the difficult and interesting subject of secular changes of climate.

Before speaking of the general question, there is a special point, on which, though not myself a geologist, I must venture to differ from Professor Geikie. He says:—“No one acquainted with the physical features and geological structure of Scotland and Norway can doubt that the valleys which terminate in fiords are of great geological antiquity. Their excavation by fluvial action certainly dates back to a period long anterior to the Ice Age.” On general grounds I think this statement is partly misleading. Not very many sea-coasts are cut up into fiords; and it cannot be a mere coincidence that fiords have been formed chiefly on those coasts where glaciation is most favoured by the geographical conditions, namely, on mountainous coasts, in high latitudes, and where exposure to prevailing west winds from the ocean promotes an abundant snowfall. Norway, Scotland, and the west of Ireland, presents such coasts; but the most conspicuous instances will be seen by a glance at a map of the world, to be at the northern and southern ends of the western coast of the American continent. From Vancouver’s Island northward, and from Chiloe southward, the coasts of the continent are cut up into fiords and islands by sounds which are submerged valleys; while in the lower latitudes, both northern and southern, the coast, from Vancouver’s Island to Chiloe, is remarkably unbroken.

The connection between glaciation and the formation of fiords is obvious enough. Most valleys have been excavated; and these, except some which have been eroded by the sea, are due either to fluvial or to glacial action. Mountain valleys excavated by running water are in general deep and narrow—the most conspicuous instances are the canyons of the Colorado, and the Via Mala in Switzerland, which is a canyon—and, though on a much smaller scale, the ravine-like valleys of the so-called Saxon-Switzerland are of this class. Mountain valleys excavated by glaciers are on the contrary deep and wide; and it appears to be generally agreed that most of the valleys of our European mountains are of this origin. When such a valley descends into the sea it becomes a fiord. It may be the fact that most of the greater valleys of Norway and Scotland existed as river valleys before the Glacial Period, but if so, during that period they became filled with glaciers, which, by their excavating action, gave the valleys their present form and contour.

I am fully convinced that no merely geographical changes can possibly account for the glacial climate; and I agree with Mr. Croll that its causes were astronomical. But I think he has failed to explain rightly how these causes operated.

I must here point out that the extent of glaciation depends in no degree on mean temperature, but exclusively on summer temperature. The “snow-line” is the line of summer snow, and theory and

observation agree in showing that the extent of glaciation depends chiefly on the height of the snow-line so defined. There is a region in Eastern Siberia where the ground, at the depth of a few feet, is frozen all the year round, showing that the mean temperature of the year is below frost; and yet over that frozen subsoil cattle graze, crops of rye are harvested, and pine forests flourish. It is obvious that if from any cause the extremes of that climate were to disappear, while its mean temperature were to remain unchanged, so that there was a temperature below freezing for every month of the year, all the precipitation would be of snow, which would remain unmelted, and the land would be covered with continual ice like Greenland.

There is an astronomical cause which must produce such changes. The major axis of the earth's orbit is unchangeable, but the minor axis is subject to slow fluctuating changes of length; and as the sun is always in one of the foci of the ellipse of the earth's orbit, it follows that the narrower the orbit, the greater is its eccentricity, and the greater the difference between the earth's perihelion and aphelion distances—in other words, its least and its greatest distance from the sun. Now, when the earth's aphelion occurs in the summer of either hemisphere, there must in that hemisphere be a cold summer; and a cold summer, as we have seen, produces glaciation, so that the hemisphere having an aphelion summer had a glacial climate. During the winter of the same hemisphere, the earth was at its perihelion, or minimum, distance from the sun, giving the glaciated hemisphere a mild winter, which had no effect whatever on its glaciation; and the opposite hemisphere had at the same time an intensely hot summer, which promoted evaporation, part of which evaporation must have fallen in snow on the glaciated hemisphere. It thus appears that at definite times in the past, the two conditions of maximum glaciation must have been fulfilled in each of the Earth's hemispheres, namely, a cold summer and a snowy winter.

If this view of the nature and cause of the glacial climate is correct, the northern and southern hemispheres were never glaciated at the same time. But the periods of great eccentricity of the earth's orbit last for a long time, and during their continuance the two hemispheres were glaciated alternately, at the geologically short interval of about 10,500 years; at the end of which period the perihelion and aphelion have arrived at opposite points in the circle of the year to those which they respectively occupied at its beginning. Either solstice coincides with the perihelion or aphelion once in about 21,000 years; so that if, as is nearly the case at present, the northern mid-winter falls in perihelion and the southern in aphelion, at the end of half this period, or 10,500 years, the positions of perihelion and aphelion, relatively to summer and winter, will be reversed.

This explains the fact of inter-glacial periods: while there was



a Glacial Period in one hemisphere there was an inter-glacial period in the other; during the continuance of great eccentricity in the earth's orbit, glacial and inter-glacial periods alternated with each other in the opposite hemispheres.

My theory on this subject has been suggested by Mr. Croll's, but it is not the same. Mr. Croll, for reasons which I fail to understand, though I have read them carefully, places the glacial climate in the hemisphere which has its summer when the earth is nearest the sun, and consequently, as it seems to me, when the heat of summer is greatest, and the snow of the previous winter is most completely melted away. It is certain that at the present time, the nearest approach to a glacial climate, as shown in the greatest extent and the lowest descent of glaciers, is not to be found in countries of intense winter cold like Siberia, but in regions of cold summer and abundant snowfall, like the shores of the Straits of Magellan. Practically these remarks summarise my views as given to the Geological Society (*on the Nature and Cause of the Glacial Climate*), and the Belfast Natural History Society.

#### NOTE.

Professor Geikie has seen the foregoing letters. He offers no further remarks.—Ed.

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## ORDINARY MEETING. †

THE PRESIDENT (SIR GEORGE G. STOKES, BART., F.R.S.),  
IN THE CHAIR.

The Minutes of the last Meeting were read and confirmed.

The following Paper was read by Professor E. Hull, LL.D., F.R.S., the Author being in Canada.

*NOTES ON USEFUL AND ORNAMENTAL STONES  
OF ANCIENT EGYPT.* By SIR J. WILLIAM DAWSON,  
C.M.G., LL.D., F.R.S.

WHEN, in the winter of 1883-4, I had the pleasure of visiting some parts of Egypt and Syria, I had prepared myself, by previous study of books and collections, to devote as much of my time as possible to the investigation of certain critical and uncertain questions in the geology of those regions, and especially of the geological facts bearing on the advent and early history of man. Preliminary notes on these points were published in a short series of papers in the *Geological Magazine* (1884), and a paper on the "Bone Caves of the Lebanon," in the *Transactions* of this Society (vol. xviii), and the results were more fully given in my work *Modern Science in Bible Lands*, published in 1888, although the pressing occupations of the intervening three years did not leave sufficient time to work up all my notes and specimens.\*

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\* Reference may also be made to Professor Hull's Paper, "A sketch of the Geological History of Egypt and the Nile Valley," in vol. xxiv of the *Institute's Journal*,  
† 11th Meeting, 26th Session.

Among these are some relating to a subject which impresses itself very strongly on a geological traveller in the Nile Valley, namely, the various rocks and minerals used by the Egyptians from very early times, the purposes to which they were applied, and the manner in which they were quarried and worked. I made large collections to illustrate these points; not, however, I may be excused for saying, by defacing monuments, but by collecting broken fragments lying on old sites, and by visiting quarries and natural exposures. Egypt affords unlimited material of this kind to a lithological collector, without detriment to existing works of art, and much may also be obtained from the people, who quickly understand the value both of rock specimens and fossils when pointed out to them, and who cannot fabricate these in the manner of clay scarabs and other imitations of antiques.

The present notes may be considered supplementary to what is stated in the work above referred to.

#### 1. GRANITIC, DIORITIC, AND GNEISSIC ROCKS.

To these groups belong a large part of the monumental stones of Egypt; and from the First Cataract and the hilly ranges east of the Nile they were transported to every part of the country, even to the shores of the Mediterranean and the neighbourhood of the Isthmus, and this not in small blocks but often in great masses much more weighty than any used in modern architecture or sculpture. For this, no doubt, the navigable water of the Nile and its canals, and the variations of its level in the inundations, afforded great facilities.

The most important of all these rocks is the celebrated red granite of Syene, so generally employed in the greater Egyptian monuments. I have given detailed descriptions of this rock and its varieties in the Appendix to *Modern Science in Bible Lands*, and may merely say here that it is essentially a holo-crystalline rock, often coarse-grained and consisting mainly of orthoclase and plagioclase feldspars, with a little microcline—associated with hornblende and quartz, the latter usually in small quantity. When mica is present, it appears to be biotite, and there are sometimes minute crystals of apatite, sphene, and magnetite.

The study of this rock in place at Assouan convinces me that in regard to its mode of occurrence it is sometimes an intrusive or indigenous granite, and sometimes a true bedded

gneiss. Though the minerals in these two kinds of rock may be the same, they are distinct both in macroscopic and microscopic characters and mode of occurrence, and should not be confounded by geologists. Though the granites may in some cases be locally impressed with a laminated texture, there is no necessity for confounding them with gneisses, which are true bedded rocks; and their practical value, as well as the natural products derivable from the two classes of rocks (as soils and sands, for instance), are quite different. Huge dykes of the intrusive granite occur at Assouan, traversing the gneissic beds, and thick beds of the gneiss, interstratified with micaceous and hornblendic schists. Both species were worked by the ancient Egyptians. The great obelisks and the lining stones of the Temple of Bubastis, or some of them, are examples of the former. The broken colossus of Rameses at the Ramesseum, in Thebes, is a good example of the latter. The stupendous fragments of this statue confirm the description of Diodorus, who commends it not only for its great size, but for the "excellence of the stone." This is, in fact, not a granite, but a mass taken from a thick bed of gneiss of fine colour and uniform texture, and more dense and imperishable than any true granite. It must have sat 60 feet high, and before it was sculptured must have weighed about 900 tons. It was surpassed by but one other statue in Egypt, that gigantic one discovered by Petrie at Tanis, known only in fragments, which seems, without its pedestal, to have been at least 80 feet in height. It also was of the red stone of Syene. Of the two kinds of so-called Syene granite, the gneissic variety is the more compact and durable, and the more resisting to the action of the weather. This is a usual circumstance elsewhere, and probably depends on the fact that the gneisses have been subjected to extreme pressure during their crystallisation. The orthoclase gneisses and granites of Assouan are not distinguishable from those of the Laurentian series of North America. The gneissic variety used in some of the older structures at Gizeh is porphyritic, or an "augen-gneiss," having large crystals of pale-reddish felspar.

The shallower sculptures on many monuments of this stone seemed to have been chiselled in the usual way, but the more deeply-cut hieroglyphics and figures were probably worked in the first instance with the hollow drill.

A very remarkable stone employed in Egyptian sculpture is that variety of gneissoid rock known to Canadian geolo-

gists as anorthite rock or gneissic anorthosite. It occurs in various parts of the Laurentian districts of Canada, and more especially in those portions held by Logan to be Upper Laurentian. In Egypt, this rock first attracted my attention as the material of a magnificent statue of Kephren, the builder of the second pyramid, now in the Gizeh Museum. In this statue the lines of black hornblendic matter which mark the foliation are distinctly visible, especially on the right side. I was informed by M. Emil Brusck Bey that several similar statues in a broken condition had been found, and was enabled, through his kindness, to obtain some chips for examination. These were subsequently studied by Dr. B. J. Harrington, and compared with the analogous rocks of the Laurentian of Canada.\*

More recently, some new slices were cut and were examined by Mr. F. D. Adams, whose description is as follows:—

“In the hand specimen, it cannot be distinguished from a variety of anorthosite found at New Glasgow, P.Q., and elsewhere in the Laurentian system of Canada.

“When the slide is examined under the microscope, the rock is seen to be very fresh, and to be composed essentially of felspar with a very small amount of hornblende, which, in one place, is intergrown with a little pyroxene.

“Nearly one-half of the felspar grains show polysynthetic striations, and are, therefore, plagioclase; the remainder, although occurring as untwinned individuals, show in almost every case good cleavages, and a biaxial figure when cut normally to an optic axis, and in appearance differ in no way from the twinned grains. They are probably also plagioclase, since, as Hawes pointed out several years ago, the plagioclase in Canadian anorthosite rocks frequently shows no striations.

“The hornblende is present in very small amount as compared with the felspar, and occurs in irregular-shaped grains. It is pleochroic in green and yellowish tints. No quartz, iron ores, mica, or other minerals are present.

“In the thin section, as in the hand specimen, it bears a strong resemblance to many of our Canadian anorthosite rocks.”

I have placed in the Redpath Museum a specimen of

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\* *Modern Science in Bible Lands*, pp. 270, 573.

anorthosite from a Canadian locality with the Egyptian specimen to show the resemblance.

I did not see this rock in place, but Newbold seems to have found it in the mountain range eastward of the Nile, and it will no doubt be found to be related to the Laurentian axis of that range. The banded varieties or anorthosite gneisses, to which the material of the statue belongs, used to be regarded as altered sedimentary rocks. They are now more usually classed with igneous products, as either intrusive masses laminated by pressure or bedded igneous rocks consolidated and altered. In all probability, the latter is the more correct view.

It has been usual to call the material of these anorthosite statues diorite. For this there is a justification in the fact that the materials are in great part similar to those of that rock; but the lamination, the crystalline structure, and the proportions of the constituents are different. A singular conjecture has also been started, to the effect that this material was derived, as well as the diorite found on the old Chaldean site of Tel-loh, from quarries in the Sinaitic Peninsula, and it has even been imagined that a primitive school of sculpture existed at Sinai. Such hypotheses are, however, altogether baseless. The Chaldeans could obtain such materials from the mountains on the Persian frontier, and the Egyptians from those of their own eastern territory, and neither could easily have transported large masses of stone from the Sinaitic district.

The stone in question has many good points as a material for sculpture. It is of uniform texture and of moderate hardness, between that of marble and quartz. It is free from the quartz grains that render granite intractable. It is tough and takes a high polish. Its colour is agreeable, like that of a banded white and grey marble, and its lustre is superior to that of marble. It is extremely durable and resisting, and not liable to discoloration by weathering. Such properties, no doubt, commended it to the sculptors of the remote period of King Kephren, and it is perhaps remarkable that a stone with so many good qualities has been neglected by more modern artists. The statue of Kephren now in the Gizeh Museum bears testimony by its excellent preservation to these properties, and probably the other statues which accompanied it would have been equally perfect had they not been wilfully broken. In the later times of Egyptian art this stone seems to have lost its attractions or fallen out of fashion, except for small objects.

It would be extremely interesting to examine the quarries from which it was obtained, and to ascertain, if possible, the date when they began to be worked.

Diorite of many varieties—black, greenish, black with white blotches or mottled with black and white—forms great dykes and eruptive masses in the crystalline district of Upper Egypt, and was always and deservedly esteemed by the Egyptians. I have elsewhere remarked that, as diorite is one of the best materials for the formation of polished stone hatchets, it must have very early attracted attention; and its toughness, lustre, and susceptibility to a good polish must have indicated it as a material for sculpture. Accordingly, it is applied to a great variety of uses, from colossal statues down to platters and trays. A large proportion of the finest Egyptian statues are cut in diorite.

A dark grey granite has also been employed. It differs from the diorite in containing a little free quartz, and in having orthoclase felspar; but hornblende is usually its chief constituent. I have observed this black granite in a doorway at Karnak, in loose pieces on the site of a temple at Gizeh, in a sarcophagus at Thebes, in one of the Apis sarcophagi at Sakkara, in statues of Bast, and in a figure of Nectanebo and a hawk from Pithom in the British Museum.

True diorite occurs in the Rosetta Stone and the Great Scarabæus and several sarcophagi in the British Museum,\* in the Pithom Sphinxes now at Ismailia, and in the Hyksos Sphinx and the fish offerers in the Gizeh Museum, and a great number of statues. One of Rameses II in the British Museum is a stone from the junction of red granite and diorite, and thus consists of two distinct kinds of rock.

## 2. BASALT WITH OLIVINE.

The term basalt has been used in a somewhat loose sense by writers on Egypt, apparently to designate any dark crystalline or subcrystalline rock. Some of the objects designated by this name prove to be dark-coloured hornblending granites, others are diorites. One rock to which the name very properly applies, occurs plentifully in loose chips on some parts of the pyramid plateau, as if portions of the temples or tombs which have disappeared from that area had been composed of it. A chip from this place has been sliced and has been examined for me by Mr. Frank D. Adams, of McGill University, with the following results:—

\* Professor T. Rupert Jones, F.R.S., in Proc. Geol. Association, vol. viii.

*"From Old Temple, Gizeh."*

"This is a medium-grained basalt (plagioclase basalt), in all probability belonging to the subdivision of olivine Basalts.

"It is composed of plagioclase, augite, olivine (?), iron ore, and apatite, with a small amount of glass.

"Under the microscope, the rock is seen to be porphyritic, a few larger individuals of plagioclase and augite occurring scattered through the rock. These porphyritic plagioclase crystals are occasionally somewhat decomposed. The rest of the plagioclase occurs in well-twinned, lath-shaped crystals, and is quite fresh. The augite is often well crystallised, and shows its characteristic cleavage and inclined extinction. There are also a number of more or less rounded grains which seem to have been olivine, but which are now almost entirely altered to a brown decomposition product, showing aggregate polarisation, and which is apparently for the most part hydrated ferric oxide. This material stains the other minerals of the rock, and seems in some cases to result also from the decomposition of the augite or the glass. The iron ore, which is black and opaque, resembles magnetite, and occurs in irregular-shaped grains. The apatite is somewhat abundant, occurring in long, slender needles."

A rock of this kind is described by Zittel and by Beyrich and Schweinfurth as forming eruptive masses in Lower Egypt, and probably of Tertiary age. One locality is at Abu Zabel less than 20 miles to the north-east of Cairo, and other localities occur in the Lybian Desert to the westward. Schweinfurth has found a rock of similar aspect in hills near the Red Sea, where it appears to have been quarried. The description given by Arzruni of the variety found at Abu Zabel closely corresponds with that of Mr. Adams quoted above.

This kind of rock, probably because of its accessibility and abundance, or perhaps because of the good polish of which it is capable, and the slight play of colours of the felspar and olivine when seen in a bright light, was much used for small objects, especially in Lower Egypt. As examples of this, I have in my collection a palette for grinding colours, a polisher, a perforated disk, two scarabs, some beads, and one of the sacred eyes used as charms. Statues and ornamental work in temples seem also to have been made of it; but it is not well suited to long exposure to the weather, as the olivine and augite are acted on by the atmosphere, and become rusty.



In *Modern Science in Bible Lands*, I have referred to this rock as an olivine-dolerite, and some of the varieties of it seem to contain more olivine than that examined by Mr. Adams. The use of this material suggests the question whether the artists who first employed it may have taken a lesson from the ancient nations who used a similar material so extensively in Northern Syria, or whether, on the other hand, Egyptian masons may have been employed in Bashan. In every country, however, the builder seeking for material comes to similar conclusions, according as he attaches more or less importance to accessibility, durability, or beauty.

### 3. THE NUBIAN SANDSTONE.

This takes precedence in point of architectural use of all stones in Egypt, except, perhaps, the Eocene limestones. It is not only a soft and easily cut stone, and one which in the climate of Egypt is sufficiently durable; but where the Nile cuts through its outcrop in the gorge of Silsilis, or Silsileh, it presents exposures and facilities for shipment unsurpassed in the world. It was, however, quarried at other places, as in the vicinity of Assouan and in Nubia, where the great temple of Abu Simbel is excavated in this rock. Here, and in the great colossal figures of Amenophis, in the Plain of Thebes, its use in sculpture of the colossal sort is seen, and at Karnak, Kom-ombos, Edfou, and Denderah, its architectural employment on the most gigantic scale. That it is the material of the stupendous hypostyle hall of Seti I at Karnak, should perhaps give it precedence over all other stones of construction. The way in which in interiors it was coated with a gypseous cement and painted, I have elsewhere explained. In one quarry behind Assouan the patient excavator, instead of cutting rectangular blocks, had cut out at one operation large drums for columns, leaving semi-circular niches in the face of the rock. Regarded as a rock, it is a siliceous sandstone, composed of angular grains very loosely cemented, so that it is easily crumbled, and its colour varies from a light cream colour, or nearly white, to a yellowish-brown. Its age probably ranges from Permian to Lower Cretaceous,\* and it differs from the newer sandstone of Jebel Ahmar in its less amount of siliceous cement and of red oxide of iron, and in the absence of any rounded grains. Its

\* Fossils and stratigraphical arrangement seem to indicate that there may be two Nubian sandstones, one later Palæozoic, the other Cretaceous; but they cannot at present be separated with certainty. I have discussed this question elsewhere.

date is evidently altogether anterior to the operation of that wind-drift which has produced the modern rounded desert sand.

#### 4. LIMESTONE, &C.

In a country where cliffs of this rock present themselves on every side, it is necessarily of great importance, both as a stone of construction and as cement. It is mostly of Eocene age, though some Cretaceous beds have been locally quarried, and it is of very various qualities. It may be coarse and unequal in grain, or filled with fossil shells, as Nummulites, &c., or may be fine and uniform in texture. It is sometimes hard as marble, in other cases soft and chalky. It may be grey or brown, or of a pure white. All these varieties were more or less used, the coarser and more unsightly for cores of pyramids, foundations, and other structures not intended to be seen. The stepped pyramid of Sakkara, one of the oldest known, is wholly composed of a brownish limestone, found in the vicinity. The pure white and fine grained varieties were employed for lining and casing buildings, and for ornamental work and sculpture.

The finer varieties present under the microscope various characters. The most common and softest is of the nature of an indurated chalk; a congeries of microscopic foraminiferal shells, and must be an oceanic deposit similar to chalk and globigerina ooze. This is the variety employed for casing the Great Pyramid, for lining many temples and tombs, for statues and monumental tablets, and it is the whitest kind quarried at Turra at present. A variety observed at Abydos is of a light grey tint and earthy aspect, but this has been coated with a white cement and coloured. Other varieties used in sculpture have a fine concretionary or oölitic structure, or are so cemented with infiltrated matter as to assume a minutely crystalline character. The fine-grained foraminiferal limestone lends itself to the cutting of hieroglyphic inscriptions of all kinds, and to the art of the colourist, so that it is admirably adapted to the uses to which it was applied in tombs and temples.

A more modern limestone of later Tertiary age exists on the coast near Alexandria, and is quarried for building purposes. It is an organic rock, made up of fragments of shells, and is apparently similar in age and origin to the Pleistocene limestones found near Jaffa and Beyrout, on the Syrian coast, and to the modern shelly sandstones of the coast of the Red Sea, which are used for purposes of construction at Suez.

Alabaster, as distinguished from limestone, is a crystalline, translucent material, deposited in the manner of stalagmite, in veins, or filling caverns in the limestone. It is thus a local and irregular deposit; but the Egyptians managed to obtain it in several places, in quantities not only sufficient for vases and minor ornamental purposes, but in blocks and slabs sufficiently large to form shrines and to line portions of tombs, and even of temples. One locality where it has been extensively quarried is in the cliffs on the west side of the Nile, near Beni Suef.

The Egyptian alabaster is sometimes colourless, but more frequently banded with agate-like lines of grey and light brown, whence the name onyx-marble sometimes given to it.

Gypseous or soft alabaster does not seem to have been much used in Egypt, but small vases and other objects made of it are sometimes found.

Cleavable transparent calc-spar, probably obtained from veins in the limestone, was sometimes used by the Egyptians for minor ornaments and beads, probably as a substitute for rock crystal.

##### 5. MIOCENE QUARTZITE OF JEBEL AHMAR, &C.

My first acquaintance with this stone dates from a time long anterior to my visit to the locality. My late friend, Dr. Douglas, of Quebec, had formed in successive visits to Egypt a large and interesting collection of antiquities, in examining which I noticed a small slab, or funereal stela, inscribed with hieroglyphics, and which specially attracted my attention from the fact that it was executed in quartzite of so great hardness as to defy ordinary sculpture with steel tools. At the time, I knew such rocks only as occurring in the old Cambrian series in Canada, and had not learned that they occurred in Egypt. The choice of a stone so hard seemed strange on the part of a people whom I had scarcely supposed capable of dealing with material so refractory, the use of the diamond drill by the ancient Egyptians being then unknown. I remarked at the time that the sculptor, or his employer, had evidently determined to possess an indestructible monument, "regardless of expense," but it seemed impossible to understand how he could by any expenditure have succeeded in his purpose.

Jebel Ahmar, the Red Mountain, lies a little to the east of the Mokattam Hill, in the vicinity of Cairo, and from its

peculiar rugged and dark-coloured appearance attracts, more or less, the attention of all travellers, who have usually regarded it as of volcanic origin. Geologists, as Russegger, Newbold, Schimper, Fraas, Delesse, Schweinfurth, and Owen, have naturally given attention to it, and have discussed its relation to the fossil wood of the so-called petrified forests in its vicinity.

Stratigraphically it consists of beds of more or less indurated siliceous sandstone resting on the Upper Eocene limestones of the Mokattam hill, but differing entirely from them in appearance and mineral character. The stratigraphy thus proves that these sandstones are newer than the Eocene, and they have usually been regarded as of Miocene age, so that we have here an example of an intensely indurated rock of comparatively modern date. Quite recently Mayer-Eimar has, on the ground of certain fresh-water shells found in connection with these beds, assigned them to the Tongrian, or Lowest Miocene age,\* and with this view the evidence of the fossil trees is sufficiently in harmony. Of the older authorities, Russegger and Newbold seem to have very clearly understood the character and relations of these singular deposits. In point of fact, Jebel Ahmar, and some neighbouring eminences of similar character, constitute the undened remnants of thick beds of sandstone once spread uniformly over this region on both sides of the Nile, and deposited in shallow water succeeding the deeper water in which the Eocene limestones were laid down. Into this shallow water drifted many trunks of trees, principally of the genus *Nicolia*, and other exogenous trees believed to be allied to certain modern species of interior Africa.† With these are trunks of palms, and of Coniferous trees allied to the yew. The wood was silicified, and the sandstone in places hardened into quartzite by the percolation of siliceous waters. The action of the sea and of atmospheric agencies in later Tertiary times have removed the less consolidated portions, leaving the silicified trees scattered about, while there remained as rugged eminences those portions of the beds which had been hardened into quartzite by siliceous infiltration.

That this is the origin of these hills is evident from the

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\* *Bulletin Zurich Academy*, 1889.

† R. Brown, *Quart. Journ. Geol. Soc.*, iv. Carruthers, *Geol. Mag.*, vii. Schenk, in Zittel's *Lybischen Wüste*. One of the Conifers in my collection is a *Taxites* of modern aspect.

nearly horizontal position of their layers, from their containing silicified wood so distributed, and with its cracks filled by sandstone, &c., as to show that it was embedded in the natural state, and afterwards silicified, and by the irregular pipes or craters passing through the hardest parts of the beds, and apparently the channels of geysers, or fountains of heated water. The date of these aqueous outflows must have been little later than that of the beds of sand, and while they were still unconsolidated, and their drift wood in a recent state. Direct volcanic action is not known in connection with Jebel Ahmar, but volcanic masses of Tertiary age exist near Abu Zabel, between Cairo and Ismailia, and also in the Nubian Desert, which may be of the same age. These have been described by Beyrich, Schweinfurth, and Arzruni, and by Zittel.\* They afford the basalt mentioned in previous pages.

The Miocene or "Tongrien" sandstone of Jebel Ahmar may be estimated at 400 feet in thickness. It consists of siliceous sand partially rounded like the desert sand, but with many angular grains, and with the interstices more or less filled in with hyaline silica, sometimes entirely consolidating the mass. In some of the beds are layers of pebbles of quartz, agate, and jasper, many of which are evidently derived from the siliceous concretions in the underlying Eocene limestones. The colours vary from pure white to light red and dull purple, and the rock is often beautifully striped and mottled. From the enormous mass of chips around the hill, and the deep excavations in its sides, these beds of sandstone would seem to have been quarried from the earliest times, and they still furnish materials for millstones and for macadamising the streets of Cairo.

The harder varieties must have afforded the earliest colonists a desirable material for hoes, diggers, hatchets, and war-clubs, and their successors continued to use it largely for hammers and polishers and pestles, as well as for mortars and millstones. But from the earliest periods of Egyptian sculpture and architecture, the beauty and durability of this rock were recognised, and the perfecting of the art of drilling hard stones in the palmy days of ancient Egypt enabled this refractory material to be employed even for the formation of monolithic shrines and colossal statues.

Of the former, a shrine taken from the temple of Pithom,

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\* *Proceedings of Royal Academy, Berlin, 1882.*

and now in the square of Ismailia, forms a good illustration. I have already described this relic,\* and may here merely remark that it is a rectangular, monolithic chamber, 6 feet long and 4 feet high, with a sphinx, left in hollowing the rock, in the centre. It is formed of the red variety of the stone, with the bedding in a vertical position, and appears to be of the age of Rameses II. A similar shrine is noticed by Petrie, as found in the ruins of Tanis, but I have not seen specimens of the stone of which it is made.

One of the six monolithic statues, each about 20 feet high, sitting in front of the southern propylon of Karnac, is of a hard, light-brown variety of this rock with rows of agate pebbles, and though the upper part of the figure is gone, what remains impresses one very strongly with the audacity and perseverance of the Egyptian artist, who could attempt such a work in a material as hard as agate. Petrie informs us that the remains of the two colossal statues described by Herodotus as standing on pyramidal pedestals in Lake Moeris, show that they were of this stone. That such statues should have been broken up seems strange; but it is accounted for by the demand for millstones and pestles, &c., of this material, so that a statue of quartzite was more likely to be destroyed than one of limestone.

Among smaller works of this material the most perfect I have seen are two square slabs or tables of offerings, about 4 feet wide, with bowls elaborately worked on their upper sides, and hieroglyphic inscriptions round their margins. They are in the Gizeh Museum. They are wonderful trophies of skill and patient work, apparently belonging to a very ancient period.

Some travellers have stated that the two great Colossi of the plain of Thebes are of this stone, but this is an error. They are of a much softer rock, the Nubian Sandstone. The quarrying of this material may have been done by wedging out blocks, taking advantage in this of the joints and bedding of the stone. It could then be roughly shaped by chipping and hammering, but the finishing, especially in shrines and statues and in cutting inscriptions, must have been effected with the hollow drill, armed, perhaps, with diamond, as in the modern diamond-drill. Finally, the surface was probably polished by rubbing with sand of emery or other hard stones. Petrie has shown that the use of the hollow metallic drill, armed with gems, was well known in

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\* *Modern Science in Bible Lands*, p. 279.

Egypt, and Pliny (xxxvi. 1 and 14) mentions its use in classical times, while, in sculptures in the Roman catacombs, we see the sculptor's journeyman hard at work drilling the sides of Roman sarcophagi of stone.

Small objects, as pestles, polishers, and drill-sockets, were made of this stone. I have one of the latter with the depression for receiving the drill finely polished by long use.

## 6. VARIOUS STONES AND GEMS.

The following occur in the collections which I made in Egypt, and in specimens presented to the Peter Redpath Museum by the Egypt Exploration Fund.

*Talcoae schist* and *talc rock*, images of Osiris, moulds for casting small objects.

*Serpentine*, scarabs, images of Osiris or ushebti, small vase.

*Chlorite schist*, a small figure of Osiris.

*Argillite* or *clay slate*, small figures or charms of various kinds, spear or knife.

*Red carnelian*, beads and seals.

*Agate*, peculiar variety of moss agate with circular ferruginous markings, also various agates and jaspers, some rudely shaped, others finely worked as beads, &c.

*White milky quartz*, fragment of circular object.

*Green jasper*, cubical bead, with angles truncated.

*Amethyst*, beads and ornaments.

*Flint*, knives, scrapers, piercers, arrow-heads. All are of the kinds of flint common in the Eocene limestones.

*Garnet*, beads in carbuncle and rosy varieties.

*Lapis lazuli*, scarab, Otus eye, bead, &c.

*Steatite*, small figures of animals, &c.

*Hematite*, black and finely-polished Otus eye.

*Labradorite*, oval button or knob, broken at base.

*Fluor spar*, purple beads.

*Porphyry*, red and other colours in various small objects.

*Fuchsite*, or chromiferous green variety of mica schist, a rude fragment, possibly used for inlaying. This rock is found in the Tyrol, and in Maine, in the U.S. of America. No Egyptian locality is known. The specimen came from Naukratis.

*Mica schist*, perhaps a whetstone, also fragments unworked.

*Turquoise*, a ring stone.

*Emerald* or *beryl*, in beads.

The precise dates of these objects are of course unknown, but they were obtained mostly by Arabs from old Egyptian

graves, and some of them may be of great antiquity, while others are probably comparatively modern.

### 7. FLINT FLAKES, KNIVES, SAWS, &C.

It may be well to add here a few words as to the use of flint among the ancient Egyptians. There has been much unprofitable discussion as to whether the numerous flakes which may be picked up on the surface, especially near ancient sites, are natural or artificial, and if the latter, whether they are "prehistoric," or belong to the historical era. A few general statements of fact may serve to dispose of these questions.

(1.) The Eocene limestones of Egypt are rich in flint concretions. Some beds are especially stored with these; and even in the fine-grained white limestones used for the more important architectural purposes, the artist was often troubled by kernels of siliceous matter. Where the limestones have been denuded, great numbers of these concretions remain on the surface, just as in the chalk districts of England, and the gravel beds belonging to the older deposits of the Nile Valley, as near Thebes, at Helouan, &c., are largely composed of flints. Hence at all periods flint has presented itself to the Egyptian as an available material for tools and other purposes, and at many localities, as at Helouan, at Jebel Assart, Thebes, and in the desert, east of the Nile, ateliers with cores as well as flakes, and arrow-heads, saws, &c., may be found.

(2.) Besides the flints worked by man, innumerable chips exist that have been produced by nature. Some flints split or scale off under changes of temperature, and small rounded flakes produced in this way, and flints with conchoidal depressions are not uncommon. Torrential action, in all countries of flint gravel, has struck off numerous irregular flakes, and split the more friable flints into pieces, so that in some of the gravels a large proportion of the flints have been broken. On the one hand, there is little doubt that such naturally broken flints have been used as implements. On the other hand, any one who supposes all flint chips to be of human workmanship, even when they show a "bulb of percussion," is unduly credulous.

(3.) As to date, there is abundant proof that in historic times flints were used for surgical purposes, for incisions in corpses, for circumcision, for sacrificial purposes, and probably for common arrow-points. Careful study of the finer hieroglyphics of the calcareous tombs has also convinced me



that these were scraped in the soft limestone with pointed flints, such as are often found abundantly in the vicinity of such tombs.

(4.) It is, however, probable that in very ancient times when metals were scarce and dear, flint implements were in much more common use than in later times. Perhaps the most interesting case of this is the comparison made by Petrie (*Nature*, Dec. 5th, 1889) of two towns, Kahun and Gurob, 50 miles south of Cairo, and on the two sides of the entrance to the Fayum. The former town belongs to the early time of the 12th Dynasty, the latter to the 19th. In the former flint flakes are abundant, of various forms, and evidently applied to many uses. Among other tools a wooden sickle was found, armed with saw-edged flint flakes on the cutting side, thus connecting flint flakes with the reaping of grain. Petrie figures an example of this. In the other and later site flint flakes scarcely occur, and are rude and evidently applied to fewer uses. This seems to be an excellent illustration of the progress in one locality from a stone to a metal age. The interval of time amounts, however, to at least a thousand years, and the earlier period, that of Usurtasen II, was a time of high civilisation and great progress in the arts of life, though farmers in the central district of Egypt were still reaping their fields with flint flakes. A parallel to this is found in the prevalent use of stone for hoes, &c., among the more civilised American nations, to which I directed attention in a paper on "Fossil Agricultural Implements," in the *Transactions* of this Society several years ago.

This continuous use of flint flakes among a civilised people, and the fact remarked by Petrie, and which has been observed also in Scotland and America, that the flint implements become ruder and more coarse as they are supplanted by metal, should furnish a caution against sweeping generalisations as to ages of stone and metal, and of progress in the manufacture of flint tools and weapons. While at some times and in some localities there has been an advance from rude to finer implements, in other instances the process has been reversed.

In connection with the materials referred to in this paper, certain geological and historical facts impress themselves very strongly on our minds.

All the rocks of the Nile Valley, from the ancient crystalline and probably Laurentian granites and gneisses to the modern

limestones on the coast, have furnished materials for construction and sculpture in Egypt, and this from a very early period. This is an indication of the mental activity, observation, and intelligent industry of the people and their rulers, and, with their other achievements in irrigation and in utilising animals and plants, shows the enterprise of an early and active-minded state of society, as distinguished from the fixity and conservatism which appear in later times.

In connection with this, it is, however, to be observed that no country in the world presents greater facilities for the discovery and exploration of its mineral treasures. The proximity of the different kinds of stone to the river in cliffs easily accessible, and the unrivalled facilities for transport are important factors in this matter. Still, in the hands of an unintelligent and unprogressive people, these facilities might have long remained undeveloped.

It is also to be observed that from the earliest colonisation of Egypt there seems to have been a settled and orderly state of society, an exemption from foreign aggression, and an abundance of food, all tending to a large population, and giving facilities for the execution of public works: while the necessity of combination of effort in the irrigation and embankment of the land gave the habit of united action under leaders.

The great works of Egyptian construction thus indicate to us a country rich in materials and having admirable means of conveyance and an abundant population, and a surplus of food products. On the other hand, they show that there was an educated class capable of forming and executing great plans with precision and taste, and this again, aided by a multitude of skilled artisans, and by ample command of unskilled labour, especially at certain seasons of the year.

Historically, it is worthy of note that the great works of the Egyptians in stone, if we except the Pyramids, culminated in that period in which there is reason to believe the Hebrews had their residence in Egypt—the time of the great 18th and 19th dynasties. Within this time fall the Temple of Karnac and the greater buildings of Thebes, as well as the greatest works in statuary. We cannot, however, regard these works as other than purely Egyptian, for this was their plan and style; but the fact that the Pharaohs of this period had at their disposal the peoples and the wealth of Western Asia must have been no unimportant determining cause of their enormous expenditures of material and labour. It was a time when the artistic skill and ambition of the Egyptians

had at command an abundance of men and means, and these they employed in quarrying and working stone for temples and statues on a scale which has not since been equalled in any part of the world. In more modern times there may be equally great triumphs of design and mechanical execution, but they run in different directions, and aim at different results from those of the ancient people of Khemi, who, with all their ordinary wants superabundantly supplied by the fertility of their soil and their own eminent agricultural skill, could afford to spend a vast amount of energy in great works of art, commemorative of their lives and national achievements or tributary to their religion.

The PRESIDENT.—I will ask you to return your thanks to Sir William Dawson for his most interesting Paper and also to Professor Hull for having been so kind as to read it. The Paper is rather technical in its nature, that is, it can hardly be discussed except by geologists and those who are to a certain extent versed in Egyptology. In inviting remarks thereon from those present, I think, in the first place, I should ask Professor Hull himself to speak.

Professor HULL, LL.D., F.R.S.—We have here before us the results of the observations and reflections of a very accomplished naturalist—one well known in this country. Sir William Dawson was in Egypt in the years 1883–4, the same time that I went there and to Palestine, and during his stay he made ample use of the material before him, but he is very careful to inform us that he did not do what I am sorry to say some travellers do, namely, take chips from the works of art themselves. I feel grateful to Sir William Dawson myself for having had the opportunity of reading this Paper, and if any present should desire further information on any subject touched on in it I shall be happy, so far as I am able, to answer any question.

The HON. SECRETARY (Captain F. PETRIE, F.G.S.).—Before the discussion commences may I read a letter received from Major Conder (D.C.L., LL.D.), who is unable to be present this evening.

“ Dear Sir,—I have no remarks to offer on Sir W. Dawson’s valuable Paper except as regards the use of diorite or other such stone in Chaldea. There is, as he says, no reason to suppose that such stone was brought westward, but it has been thought that the Chaldeans brought it from Sinai. The reason is found in the inscription from Tell Loh to which Mr. Pinches referred in his paper on Babylonia read before the Institute. This text, dating probably about 2,500 B.C. speaks of the *usu* or *esu* stone from *Magan*. In another well-known inscription we read of this same country

  
*KUR Ma- Kan- na,*

which was the ‘country of copper.’ It is connected with Egypt in an inscription of Assur-bani-pal. (See Lenormant, *Trans. Bib. Arch. Society*, vol. vi., p. 347–9). It was a stony country on the Egyptian east frontier, and ships and papyrus reeds are mentioned in connection with it. The situation of the Sinaitic peninsula seems best to agree with this description, and some render the name ‘Land of the Wall,’ connecting it with *Shur*, the ‘wall’ east of Egypt.

“ If this identification be correct it would appear that diorite was thence obtained by the Akkadians for the statues now in the Louvre, the stone of which might with advantage be examined by a geologist to determine whether it could have come from Sinai.

Yours truly,

C. R. CONDER.”

Mr. W. BRINDLEY, F.G.S.—I have much pleasure in stating that an examination of the Louvre statues made two days ago proves them to be of Siniatic diorite.

I wish the Author had told us more about the rocks of the eastern or Arabian desert; as these rocks supplied the ancients with some of their most important building and decorative materials, the quarries of which gave employment to thousands of workmen.

Of the igneous rocks there are three ranges running parallel with the Red Sea coast. The first commences south of Suez and terminates at Zeitai, where are the petroleum wells which supplied bitumen for “embalming.” The second is the range now called “Gebel Esh” which ends at Abu Shaar (the ancient Myos Hormos), which was the port for the commencement of the caravan

route from India to Egypt and Europe. The third range consists of high mountains some 7,000 feet high; these are about 20 miles inland, and extend south to Berenice, with a branch sweeping to the west across the Nile at Assouan, forming the first cataract. In this range are the ancient quarries of Egyptian Porphyry; these supplied the Romans with all their choice material, and the granite quarries produced half the columns in the portico of the Pantheon in Rome, and all those of the Forum of Trajan. The quarries of Breche Verde of the Egyptian, also in this range, supplied choice material for their sarcophagi (as see the one in British Museum) and innumerable columns of later date. A green serpentine was also quarried, and much used for turnery. The boulders out of the Breche Verde supplied choice hard materials for their best "scarabea." The quartz veins contained gold and copper, giving employment to an immense number of miners. The southern range near Berenice contain the famous emerald mines, probably the oldest known. These mines and the whole of this desert are being exploited by Mr. E. A. Floyer, F.G.S.

MR. W. ST. C. BOSCAWEN, F.R. Hist. Soc.—There is a remarkable point that seems to indicate a connection with Egypt, and that is that upon the knee of two of the statues at the Louvre, is a kind of tablet upon which is drawn a plan of a building, very carefully done with a burin or graver, and by the side of it, on the edge of the drawing board, if I may use the expression, is a finely carved scale divided accurately into divisions which could not be the Babylonian cubit, but the Egyptian cubit. We now know that at a much earlier period than was thought there was a close intercourse between Babylonia and Egypt both by land and by sea. There is one other point that I may throw a little light on, perhaps, and that is with regard to the use of the diamond drill. The diamond drill was used amongst the early Babylonians at a very early period, and was very likely got from Egypt. That is shown by the curious fact that in the list of stones the Hebrew *shāmīr* or diamond is expressed by two signs which mean a boring stone, and a French sculptor about fifteen years ago, who devoted a good deal of attention to the study of working the stone in which the statues from Babylonia were carved and the way in which the hard gems were cut, told me he had come to the conclusion, from the unfinished specimens he had seen, that the work was done by a series of little drilled holes and then working it down with

some hard powder such as emery or corundum; in regard to this apparent connection between Babylonia and Egypt, I may mention that two accounts of discoveries at Tell-Loh have been published recently, one by a French geologist; both speak of diorite being found at Tell-Loh and porphyry as of Egyptian production; that it came from that district, and was brought into Babylonia by sea, there can be no doubt. There is an artistic point that I would notice in regard to the statues—that from the 4th to the 12th dynasty Egyptian work was at its highest. It had not reached that conventional stereotyped style of work which afterwards appeared in the 18th and 19th dynasties; but the statues were then in every case, as much as possible, portraits.

Mr. WILLIAM MORETON MIDDLETON.—With regard to the flint implements, I should like to ask Professor Hull if he has any further evidence as to the date when the flint implements ceased to be worked. Sir William Dawson says at the time of the 12th dynasty flint flakes were abundant, but at the time of the 19th dynasty they were apparently rare. It may be that there is further evidence on that point; if so, it would be of great interest as enabling us to tell, to some extent, when the use of flint implements was at its zenith, so to speak, and of flint arrow-heads from the banks of the Suez Canal. I should also like to ask Professor Hull if he happens to know how the agate was worked. I see that apatite is mentioned in one or two places. If apatite in anything like its pure form were found in any quantity it would be very valuable to the agriculturists of Egypt. It is, of course, well known that apatite is used as a phosphatic fertiliser in Canada and elsewhere. In regard to early Egyptian and Roman art, the Roman cornices, for instance, bear excellent portraits for the first century or so, and afterwards degenerated very much, and at the time of the third or fourth century became intentionally conventional.

Mr. J. D. CRACE.—It may be worth mentioning in reference to implements for which flints were used, for instance the sickle; the identical sickle referred to in the paper was exhibited in London by Dr. Petrie, it was a wooden sickle with a flint edge. The bow-drill is represented pretty frequently in Egyptian sculpture and is in use at the present day in Egypt. The sandstone described as miocene sandstone, containing strata of agate and cornelian, occurs also at Wady Halfa and in another place in its neighbour-

hood, where the whole surface of the ground there is rich with the most beautiful forms of agate and cornelian.

Professor J. LOGAN LOBLEY, F.G.S.—There is one point which I may throw some light on, as to there being actual alabaster in Egypt. Professor Hull has rightly held that the alabaster that Sir Wm. Dawson mentions is now called “limestone stalactite.” It was originally called alabaster, from the Arabic word *alabastron*, but it is not now so called. The original alabaster was a substance similar to the onyx stone, that is, carbonate of lime; but what we now call alabaster is sulphate of lime, and banks of that have been found in Egypt. As to the Nubian sandstone, I observe that Sir William Dawson says its age probably ranges from Permian to Lower Cretaceous.

Mr. W. H. HUDLESTON, F.R.S., President Geol. Soc.—I suppose I should say a few words. We owe a great deal to Sir Wm. Dawson for his information on the rocks of Egypt. There is one difficulty with regard to these rocks, and that is that you find there are such large numbers and varieties of this gneiss rock associated with diorite and granite and so forth. There are not many rich mineral veins there. We do not hear of many minerals produced from Egypt—in fact, Egypt though rich in stone seems to be poor in metallic minerals.

The question of the Nubian sandstone, as Professor Hull knows, is one that concerns a wide area, and few have done more than Professor Hull himself in settling what that formation represents in the Sinaitic Peninsula—that it is in part Carboniferous, whilst the upper portions are probably of Cenomanian age. With reference to the masses of Siliceous rock in more recent deposits such as those of the Mokattam hills, the Author says:—“That this is the origin of the hills is evident from the nearly horizontal position of their layers, from their containing silicified wood so distributed and with its cracks filled by sandstone, &c., so as to show that it was embedded in the natural state, and afterwards silicified, and by the irregular pipes or craters passing through the hardest parts of the beds, and apparently the channels of geysers or fountains of heated water.” He then proceeds to say that there is no evidence of such action in that neighbourhood—I do not suppose there is likely to be; in fact, I am myself inclined to think this formation may more probably be the result of the action of cold rather than hot water. Possibly Professor Hull will be

able to tell us something in further explanation of the immense quantities of siliceous matter in these beds. I can only say that I have heard with great pleasure Sir Wm. Dawson's Paper and I regard him as a great authority on the geology of the East, and I recommend anyone who can to get his "Modern Science in Bible Lands," which is a most excellent book.

Professor HULL.—I will venture to make what answer I can to those who have spoken. I need not say that I listened with great pleasure to the observations of Mr. Brindley. He has given us the positions of the quarries of the gray granite as distinguished from the red grauite, showing that although they come from the neighbourhood of Syene they are quarried from distinct quarries.

Mr. BRINDLEY.—No; pardon me—I should say you find acres of the dark-coloured granite immediately covering the red, but the *starling* granite comes from elsewhere, but I would say that I do not believe the Egyptians ever quarried it; but that was done during the Roman occupation.

Professor HULL.—Then that bears out what I say. In regard to the two columns at Venice, both come from Syene. I believe porphyry may have been got either from the neighbourhood of Syene itself, where Mr. Newbold was the first to discover it, or it may have come from the Sinaitic Peninsula, where there is similar stone to this—in fact all the stones of Egyptian works of art are found repeatedly, I think, in the Sinaitic Peninsula and along the Edomite side of the Arabah valley.

Mr. Middleton, who, I am glad to see again after our former meeting, has asked, When did the manufacture of flint implements cease in Egypt? As a matter of fact I believe it has never ceased to this day. I think Sir William Dawson himself describes in his book on the geology of Bible lands the villages on the banks of the Nile where flint implements are made to the present day,\* as was once done in England, but those to which he refers are, of course, of a very ancient period. Reference has been made to the relations between the strata of this period in Egypt and in the Sinaitic Peninsula? As far as I am able to ascertain at present there occurs in the Sinaitic Peninsula a Carboniferous limestone. That is interspersed between the red sandstone below, which I

\* In the discussion on a Paper by the late Rev. F. W. Holland, M.A. (Transactions, Vol. xiv, p. 1), the Author mentions, in regard to the manufacture of flint implements, that on more than one occasion during



have ventured to call the Desert sandstone, and the red sandstone above which I believe to be true Nubian sandstone. Therefore Sir William Dawson has a right to hold the opinion, if he chooses, that as the Nubian sandstone is found in the Sinaitic Peninsula resting on Carboniferous strata it may be of an age intermediate between the Permian and Lower Cretaceous; but it is impossible to say; and my own opinion is that the Nubian sandstone proper is of the Lower Cretaceous age.

The meeting was then adjourned.

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his travels in Egypt, when his Arabian servant wanted something in the shape of a knife, he searched for and took up a couple of flints and formed a flint knife for himself on the spot, throwing it away again when it had served his purpose.—Ed.

## APPENDIX.

## CAUSES OF CLIMATAL CHANGES.

The discussion upon Professor Geikie's valued paper having tended to show how diverse are the views held upon this subject, it may be permitted to quote certain remarks made "On the Causes of Climatal Changes," by Sir J. William Dawson, C.M.G., F.R.S., in his last new work.\*

The subject . . . is one which has been in dispute ever since I began to read anything on geology, nearly sixty years ago. It ought to have been settled, but up to to-day one finds in geological works and papers—especially those relating to the Glacial age—the most divergent views.

Mr. Searles V. Wood, in an able summary of the possible causes of the succession of cold and warm climates in the northern hemisphere, enumerates no fewer than seven theories which have met with more or less acceptance, and he might have added an eighth. These are:—

1. The gradual cooling of the earth from a condition of original incandescence.
2. Changes in the obliquity of the ecliptic.
3. Changes in the position of the earth's axis of rotation.
4. The effect of the precession of the equinoxes, along with changes of the eccentricity of the earth's orbit.
5. Variations in the amount of heat given off by the sun.
6. Differences in the temperature of portions of space passed through by the earth.
7. Differences in the distribution of land and water in connection with the flow of oceanic currents.
8. Variations in the properties of the atmosphere with reference to its capacity for allowing the radiation of heat.

\* *Some Salient Points in the Science of the Earth.* Hodder and Stoughton. 1893. This valuable work is "intended as a closing deliverance on some of the most important questions of Geology, on the part of a veteran worker."

Something may be said in favour of all these alleged causes; but as efficient in any important degree in producing the cold and warm climates of the Tertiary period, the greater number of them may be dismissed as incapable of effecting such results, or as altogether uncertain with reference to the fact of their own occurrence.

1. That the earth and the sun have diminished in heat during geological time seems probable; but physical and geological facts alike render it certain that this influence could have produced no appreciable effect, even in the times of the earliest animals and plants, and certainly not in the case of Tertiary floras and faunas.
2. The obliquity of the ecliptic is not believed by astronomers to have changed to any great degree, and its effect would be merely a somewhat different distribution of heat in different periods of the year.
3. Independently of astronomical objections, there is good geological evidence that the poles of the earth must have been nearly in their present places from the dawn of life until now. From the Laurentian upward, those organic limestones which mark the areas where warm and shallow equatorial water was spreading over submerged continents, are so disposed as to prove the permanence of the poles. In like manner all the great foldings of the crust of the earth have followed lines which are parts of great circles tangent to the existing polar circles. So, also, from the Cambrian age the great drift of sediment from the north has followed the line of the existing Arctic currents from the north-east to the south-west, throwing itself, for example, along the line of the Appalachian uplifts in Eastern America, and against the ridge of the Cordilleras in the west.
4. The effects of change of eccentricity and precession have been so ably urged by Croll, and recently by Ball, and have so strongly influenced the minds of those who are not working geologists, that they deserve a more detailed notice.
5. The heat of the sun is known to be variable, and the eleven years' period of sun spots has recently attracted much attention as producing appreciable effects on the seasons. There may possibly be longer cycles of solar energy; or the sun may be liable, like some variable stars, to paroxysms of increased energy. Such changes are possible, but we have no evidence of their occurrence, and they could not account for periods of refrigeration of limited duration like the Glacial age.
6. It has been supposed that the earth may have at different

times traversed more or less heated zones of space, giving alternations of warm and cold temperature. No such differences in space are, however, known, nor does there seem any good ground for imagining their existence.

7. The differences in the form and elevation of our continents, and in the consequent distribution of surfaces of different absorbent and radiating power, and of the oceanic currents, are known causes of climatal change, and have been referred to in these papers as competent to account for many, at least, of the phenomena.
8. Reference has already been made, in connection with the distribution of plants, to the possibility that the primeval atmosphere was richer in carbon than that of more modern times, and that this might operate to produce diminution of radiation, and consequent uniformity of temperature ; but this cause could not have been efficient in the later geological periods.

Sir William Dawson having further reviewed the fourth and seventh theories enumerated by Mr. Wood, urges the sufficiency of the old Lyellian theory of geographical changes, with such modifications as recent discoveries have rendered necessary to account for facts.—ED.